

Wearing body cameras increases assaults against officers and does not reduce police use of force: Results from a global multi-site experiment

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Barak Ariel

University of Cambridge, UK; Hebrew University, Israel

Alex Sutherland

RAND Europe, UK

Darren Henstock

West Midlands Police, UK

Josh Young

Ventura Police Department, USA

Paul Drover

West Midlands Police, UK

Jayne Sykes

West Yorkshire Police, UK

Simon Megicks

Cambridgeshire Constabulary, UK

Ryan Henderson

Police Service of Northern Ireland, UK

Corresponding author:

Alex Sutherland, RAND Europe, Westbrook Centre, Milton, Cambridge, CB4 1YG, UK.
Email: alex_sutherland@rand.org

Abstract

Police use of force is at the forefront of public awareness in many countries. Body-worn videos (BWVs) have been proposed as a new way of reducing police use of force, as well as assaults against officers. To date, only a handful of peer-reviewed randomised trials have looked at the effectiveness of BWVs, primarily focusing on use of force and complaints. We sought to replicate these studies, adding assaults against police officers as an additional outcome. Using a prospective meta-analysis of multi-site, multi-national randomised controlled trials from 10 discrete tests with a total population of +2 million, and 2.2 million police officer-hours, we assess the effect of BWVs on the rates of (i) police use of force and (ii) assaults against officers. Averaged over 10 trials, BWVs had no effect on police use of force ($d = 0.021$; $SE = 0.056$; $95\% CI: -0.089-0.130$), but led to an increased rate of assaults against officers wearing cameras ($d = 0.176$; $SE = 0.058$; $95\% CI: 0.061-0.290$). As there is evidence that cameras may increase the risk of assaults against officers, more attention should be paid to how these devices are implemented. Likewise, since other public-facing organisations are considering equipping their staff with BWVs (e.g. firefighters, private security, traffic wardens), the findings on risks associated with BWVs are transferrable to those occupations as well.

Keywords

Body-worn cameras, use of force, assaults, police, multi-site experiment

Introduction

Violence by the police and against the police undermines the rule of law (Kirk, 2011; Tyler, 1990). Recent cases of killings by the police, particularly of unarmed minority civilians, have raised concerns about due process and justice (Kennedy, 2011). These events mirror a developed body of research on police legitimacy, accountability and transparency in social sciences (Tankebe and Bottoms, 2014). More than any other behaviour,

use-of-force by police officers has the potential to decrease public trust in the police. While most citizens recognize the occasional need for force, the overall frequency of force used by police and force that is perceived to be excessive are clearly of concern to the public. (Stewart et al., 2013: 1)

So what would it take for the police to act with fairness and restraint when interacting with citizens?

At present, there is a world-wide uncontrolled social experiment taking place with one potential answer to this question: equipping police officers with body-worn videos (BWVs) in order to improve procedural compliance by officers and ‘take the heat’ out of encounters with citizens. This social experiment – underpinned by feverish public debate and billions of dollars of government expenditure around the world (Friedman, 2015) – is one where robust evidence can, just, keep pace with the adoption of the new technology (Lum et al., 2015; White, 2014). Our contribution here is to create that evidence base with one of the largest multi-site prospective randomised controlled trials (RCT) in the history of criminal justice research. In this note, we report that, averaged over 10 trials,

camera use had no average effect on police use of force, while in some sites BWVs appear to *increase* use of force against suspects compared to control conditions. Also, crucially, BWVs appear to lead to an increased rate of assaults against officers.

Background

Police use of force is arguably the most controversial aspect of police work. The problem, pushed vividly into public consciousness by the rasping last breaths of Eric Garner, is viewed by some across society as suggestive of a reliance on force too often, and in excess of what is often required, by police officers in the US (*Daily Mail*, 2015). Yet police violence is not just a major concern for America. In a span of 10 years (2005–2014), 5000 people from Brazil have been killed by police (Amnesty International, 2015). Deaths and serious injury in British police custody led to several judicial reviews over the years (Home Office, 2015; Teers, 2015). Globally, the grim roll call of police brutality threatens police legitimacy in the eyes of the populace they purport to ‘protect and serve’ (Tankebe and Bottoms, 2014).

Leaving aside the severest form of police violence, many jurisdictions struggle with the lack of accountability surrounding non-lethal use of force by police. For example, prompted by the recent spate of police killings in the US, US police departments were asked to release data on the use of force against citizens in 2014. The released data drew an ignominious response from the press, including prompting *The Guardian* to start compiling data on police killings (*The Guardian*, 2015).¹ Where information on police use of force does exist, it is piecemeal or historical in nature. For example, in 2001 the International Association of Chiefs of Police (ICAP) published a report that detailed police use of force in the US, summarising this as a low-frequency event in the region of 3.61 incidents per 10,000 police–public encounters (IACP, 2001).

In other jurisdictions such as England and Wales, there is no national, organised collection of data on police use of force, barring the use of Tasers. This means that it is not possible to establish baseline levels of use of force for English and Welsh police forces. If this is the case, then much of what is published on use of force may be confounded by this lack of systematic evidence (McDowall et al., 2015). Conversely, when looking at assaults *against* police officers in the US, there are long-run time-series going back several decades based on the Uniform Crime Report. For example, the FBI law enforcement officers killed and assaulted (LEOKA) data (FBI, 2014) and data collected by the Police Roll of Honour Trust in the UK (Police Roll of Honour Trust, 2015) show that assaults against officers have declined, as have deaths of officers (but there are notable limitations to how UK data on assaults against police are recorded).² At the same time, and very similar to the recording of use of force, these datasets are generally incomplete and inconsistent. Thus, we have no reliable systematic figures on the extent of ‘force’ in police–public encounters, nationally as well as locally.

Notwithstanding the data concerns, we know that police use of force can be contentious, leading to challenges of the delicate relationship between the police and the public. Despite incredible efforts to better these ties, it often seems that existing initiatives – training, altering procedures and in-house as well as public inquiries – are insufficient. Within this framework – and by means of response to these perceived threats to the

legitimacy of the police in the US – President Obama’s administration announced an additional ‘\$263m of spending investment package that will increase use of body-worn cameras [...] add more resources for police department reform, and multiply the number of cities where Department of Justice facilitates community and local law enforcement agencies engagement’ (White House, 2014). This reaction complements the view of BWVs as a panacea to America’s problem with policing (Floyd et al. v City of New York, 2013) – or perhaps, the problem with America’s police.

The basic motivation for police BWVs is to reduce: (i) use of force by police and (ii) complaints against the police. Change in these outcomes is expected to lead to myriad benefits, including enhanced police legitimacy and professionalism. Increased officer safety is another. The theoretical basis for the use of cameras – that being monitored changes behaviour – is well established in many fields of research (Adair, 1984; Sherman, 1990). Nevertheless, many encounters with police are filmed, but filming does not appear to alter police behaviour. Research on other types of cameras – closed-circuit television (CCTV) and various forms of surveillance – tells us that their effects on behaviour are limited (Welsh and Farrington, 2009). So why would police *wearing* a camera make a difference? In the psychological, social, political and natural sciences, work has focused on the idea of deterrence (Nagin, 2013). Deterrence requires a ‘guardian’, a rule-enforcer, with high certainty and celerity of punishment. The greater the perceived likelihood of apprehension by a rule-enforcer, the less likely rule-breaking is to occur, across nearly all types of human behaviours (e.g. Becker, 1978). CCTV typically fails because people are not aware they are being filmed. Police BWVs – at least as implemented in our trials – fulfil these requirements and are well placed as a mechanism to regulate police–citizen encounters (Ariel, 2016 ; Ariel et al., 2015).

Leaving aside the political and legal issues surrounding the use of ‘on-cop’ surveillance technology (Harris, 2010), the question we try to answer is: can BWVs reduce the use of force? In the published, peer-reviewed experiments on this topic to date,³ police use of force was reduced compared to control conditions (Ariel et al., 2015; Jennings et al., 2015; see also Owens et al., 2014). However, a handful of novel experiments provide insufficient evidence to justify the billions of public funds being spent globally on police body-worn technology (Ariel et al., 2015; White House, 2014). Given the current state of police–public relations, robust empirical evidence is urgently needed (see the recent systematic review by Lum et al., 2015), even if only to act as a catalyst for institutional change and better-informed debate. This urgency is underlined when nearly 5000 of more than 14,000 local US police departments, and countless countries worldwide, are already equipped with BWVs, without evidence to their efficiencies, cost-effectiveness or wider societal impacts.

Methods

Our data come from 10 RCTs from eight police forces in six jurisdictions, covering a total population of more than 2,000,000 citizens. Two forces had had two separate geographic areas included in the trial, hence the greater number of trials than police forces. Information on participating sites is presented in Table 1 below. Jointly, the trials involved 2122 officers in eight police departments, with 2,188,712 officer-hours.⁴

Table 1. Descriptive statistics for the 10 participating sites.

Site	Population size	Total arrests during trial	N of shifts	N of frontline officers	Officer hours during trial	Follow-up period post-RA (in weeks)	N use of force during treatment shifts	N use of force during control shifts	N of assaults against officers during treatment shifts	N of assaults against officers during control shifts
Site B	285,700	267	134	23	9,112	26	25	53	1	0
Site D	285,700	313	134	22	9,112	26	31	53	6	1
Site H	108,817	2591	734	115	176,160	50	20	34	68	58
Site F	188,400	1886	784	120	235,200	52	363	363	38	21
Site A	161,400	1889	462	546	221,760	22	79	75	12	9
Site I	26,757	876	180	60	494,640	45	32	28	12	21
Site E	751,500	3390	462	870	369,600	22	111	88	19	21
Site C	203,800	1097	462	111	410,256	22	33	23	8	6
Site J	151,533	2193	675	150	135,000	45	53	26	15	16
Site K	249,470	1175	888	105	127,872	43	38	18	17	24
Grand total	2,127,377	15,677	4915	2122	2,188,712		785	761	196	177
M	236,375.22	1567.70	491.50	212.20	218,871.20	35.30	78.50	76.10	19.60	17.70
SD	207,470.21	1,004.64	276.99	275.44	163,573.99	12.68	103.85	103.45	19.69	16.68

Note: RA = random assignment. Site codes used to mask research locations.

Table 2. Trial measures by treatment allocation within each participating site.

Site × day of week	Site J	Site I	Site A	Site C	Site E	Site B	Site D	Site K	Site F	Site H
Cameras on	337	88	229	232	229	66	62	445	392	367
Monday	68	–	33	34	32	10	11	66	56	52
Tuesday	67	–	33	33	32	8	9	64	56	52
Wednesday	67	–	32	33	33	9	9	64	56	52
Thursday	69	–	33	33	33	11	8	61	56	54
Friday	–	44	32	33	33	9	7	62	56	52
Saturday	–	44	33	33	33	8	8	65	56	53
Sunday	66	–	33	33	33	11	10	63	56	52
No cameras	338	92	233	230	233	68	72	443	392	367
Monday	67	–	33	32	34	10	9	63	56	52
Tuesday	68	–	33	33	34	11	10	65	56	52
Wednesday	68	–	34	33	33	11	11	62	56	52
Thursday	66	–	33	33	33	8	11	65	56	52
Friday	–	46	34	33	33	10	12	64	56	54
Saturday	–	46	33	33	33	10	10	61	56	53
Sunday	69	–	33	33	33	8	9	63	56	52
Grand total	675	180	462	462	462	134	134	888	784	734

Each study was a two-arm trial that randomly assigned officer shifts to either experimental (with cameras) or control (no cameras) conditions, on a weekly basis. This resulted in 4915 shifts being assigned ($M = 491.50$; $SD = 276.99$ per site), with no differences between treatment and control conditions in terms of the distribution of shifts (Table 2). Beyond large-scale cluster-randomised designs – which would perhaps be a ‘gold standard’ design in this context if properly administered – randomising shifts is the most practical approach to implementing BWVs trials with police, as even small forces can leverage large sample sizes (Ariel et al., 2015). Our pre-published protocol (see Supplementary Materials) states that all officers doing ‘camera on’ shifts had to wear a camera, keep the camera on during their entire shift (typically between 8 and 12 hours) and inform members of the public during any encounter that they were wearing a camera that was recording their interaction (see Supplementary Materials). This means that the intervention consisted of [camera + notification]. To be clear, the trial design meant that officers did not have discretion about when cameras were turned on – cameras were supposed to be kept on throughout their shift. The only exceptions were circumstances when officers responded to specific types of incidents that were pre-agreed with senior staff in each force (e.g. when conversing with informants, serious sexual assaults or major public events).

The two outcomes reported here for all studies were whether an officer used force during a shift (if so, how many times) and whether or not officers were assaulted (if so, how many times). These were then standardised as rates per 1000 arrests per shift because forces ranged in size. There are differences in how police forces define ‘force’ (e.g. incidents that do or do not account for compliant handcuffing), as well as how assaults against officers are recorded (e.g. physical and/or verbal) – with both likely to vary by

jurisdiction and/or police force. To mitigate differences in how 'force' was defined, our analyses focus on any physical restraint on the force continuum (Garner et al., 1995; Terrill, 2001) *beyond the use of verbal commands during an arrest* (so if an officer uses pepper spray, for example). A consistent benchmark of what level of force is included in the trial helps with comparability between sites, even if they have differing base rates for use of force. (The differing base rates do not affect the within-force results from the trials – the RCTs are testing between-group differences in each trial site – but may be important for understanding variation in outcomes between trials, particularly in light of the different base rates in use of force. We discuss this point below.) Assaults against officers – as noted above – can be more difficult to capture, because it depends on the willingness of the officer to report their assault (see Bierie, 2015). For this study, we had to be pragmatic in how data on assaults against police were captured. Imposing new reporting requirements would have been problematic (particularly in having this agreed across forces and jurisdictions). As such, we relied on the routine reporting requirements already in place.

We used the *Comprehensive Meta-Analysis Version 2* software (CMA) to synthesise the results from the trials and present the overall results, using standardised difference of means (Cohen's d ; Cohen, 1992) to compare treatment and control conditions across all sites. We then used the standardised mean difference as a summary statistic in a meta-analytic procedure, and the corresponding 95 percent confidence intervals as a measure of reliability of the estimation procedure. As each trial uses the same design and outcomes then it is appropriate to combine and report them in this manner (Lipsey and Wilson, 2001). The data inputted into CMA consisted of: (i) the number of treatment and control shifts; (ii) the rate of incidents of use of force per 1000 arrests; and (iii) the rate of recorded assaults on police officers on patrol per 1000 arrests in each shift.

Results

We have two main results. First, there was no overall discernible effect of using BWVs on police use of force. Second, cameras increased the likelihood of an officer being assaulted during a shift compared to not wearing the cameras. Raw data from all sites are given in Table 1. Figure 1 is a forest plot with combined estimates from the fixed-effects meta-analytic specification for police use of force per 1000 arrests. The overall result was that there were no significant differences between treatment and control arms ($d = 0.021$; $SE = 0.056$; 95% CI: $-0.089-0.130$). These results were heterogeneous ($Q = 17.90$; $p < .05$; $I^2 = 49.7\%$) and this heterogeneity appears to be driven by (i) the 55 percent difference in the prevalence of use of force in treatment compared to the control conditions in three studies (Site B, Site D and Site H); a result (ii) countered by negative findings at most sites (i.e. use of force *increased* in treatment vs control shifts) from seven trials (43% increase in prevalence). These puzzling results flip the theoretical basis for the study on its head: there was no reason to suspect that the use of force by officers would *increase* when cameras were turned on, as these acts are virtually guaranteed to be caught on camera and should, thus, deter officers (as with the recent Samuel DuBose homicide; *The Guardian*, 2015).

To date, there is no reported evidence on the effect of wearing cameras on assaults *against* officers, but the working assumption has been that cameras would make officers

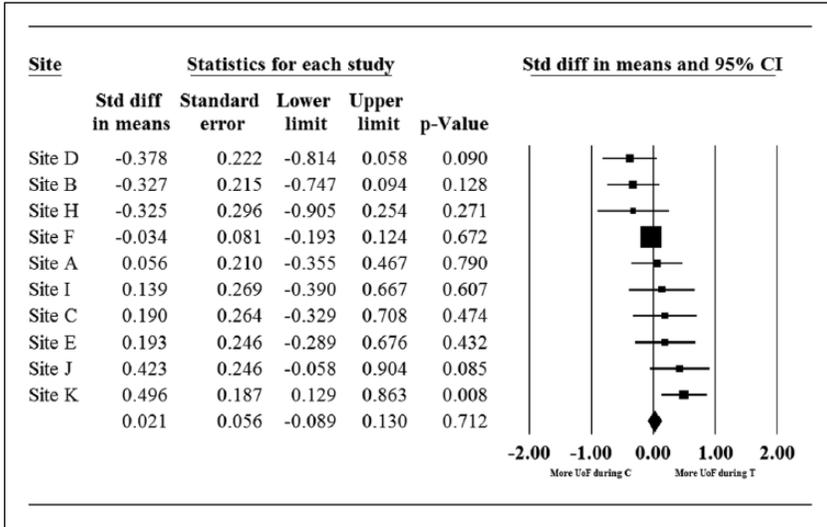


Figure 1. Rate of use of force (UoF) by officers per 1000 arrests per shift.

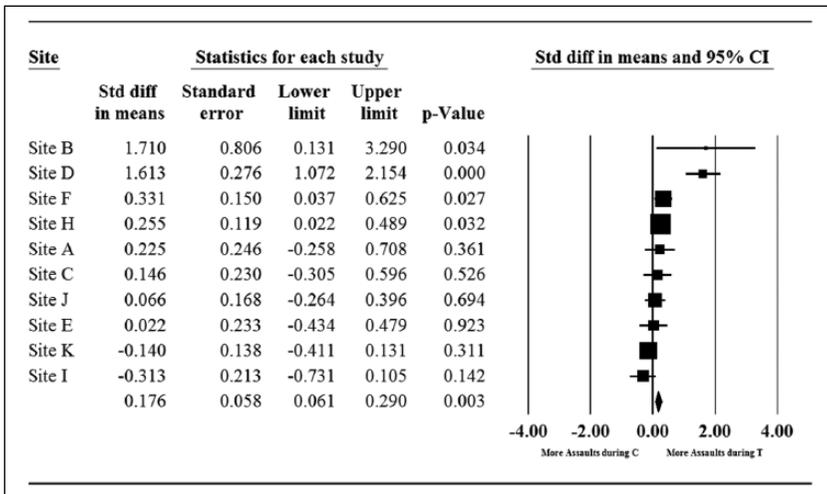


Figure 2. Rate of assaults against officers per 1000 arrests per shift.

‘safer’ (Jennings et al., 2014; but cf Goodall, 2007). Figure 2 shows the synthesised results. Contrary to expectations, assaults against officers were higher when cameras were used ($d = 0.176$; $SE = 0.058$; $95\% \text{ CI}: 0.061\text{--}0.290$), but again, with significant heterogeneity ($Q = 43.629$; $p < .001$; $I^2 = 79.4\%$). Using the original metric, the rate of assaults against officers per 1000 arrests was 14% higher when cameras were present. In real terms this meant that for every 22 assaults in control shifts per 1000 arrests

[(177/7977)*1000] there were 25 assaults in treatment shifts [(196/7700)*1000]. There were opposing results, and effects where assaults increased came from the smallest studies (Site B and Site D). Removing these smaller studies means that the result was non-significant, but the point estimate was again positive ($d = 0.051$; $SE = 0.060$; 95% CI: $-0.066-0.169$).

Discussion

The overall null use-of-force result may dampen the enthusiasm of those calling for cameras to be used by all police forces. Some may even see this as justification for body-worn videos to be abandoned altogether. We urge caution before coming to any firm conclusions for three reasons. First, there are still more studies being conducted as part of this research and as results come in the synthesised results reported here may change. Second, despite the robust methods used in these studies from around the world, the data used represent a convenience sample of police forces, and experiences in other jurisdictions may vary. Third, as we acknowledge above, different jurisdictions have varying definitions of use of force or how assaults against officers are classified, and this may affect the comparability of results between jurisdictions. Thus, an alternative explanation for the overall null result is that heterogeneity in how use of force was measured between the six jurisdictions accounts for the different results. That is, although the studies operated under the same research design protocol, differences in how the police defined use of force might account for the variation observed between forces. However, this argument is countered by the fact that we implemented a consistent approach to *measuring* force within all sites, meaning that comparisons within forces are valid. Additional sub-group analyses are needed to unpick these issues; however, such data are not presently available. This is one limitation this study faces, and will require addressing in future research.

Furthermore, the variability in our results also tells us that the BWVs worked in *some places, some of the time*, but did not work in others. Compared to the control conditions, an increase in use of force against suspects as a result of using BWVs is a puzzle. By virtue of deterrence alone, BWVs should increase compliance and, subsequently, less force will be used; yet, BWVs were found to exacerbate force in some instances. We need to understand more about this. One direct explanation might be that BWVs escalate an already inflamed police–public encounter, which results in more rather than less force being used. It might be that when BWVs are introduced into some ongoing police–public interactions, the suspect, officer or both become more aggressive.

Our second result concerning the increase in assaults against police – unexpected as it was – also demands attention. Does this mean that officers should be advised to remove BWVs immediately? If the results are accepted uncritically, that is, that BWVs increase the likelihood of assaults against officers, then this might be the conclusion one comes to. However, we cannot rule out alternative explanations at this stage. First, (yet un-evidenced) is the idea that with an ‘objective’ record of events, officers feel more able (or compelled) to report instances when they are assaulted. Second, officers may be less assertive (‘toned down’) because of monitoring, and this could make them more vulnerable to assault. (Officers sometimes deal with people who would very much like to do

them – and others – harm.) Third, the strongest results for assaults against police came from the smallest studies – these may be atypical results driven by small sample variation. Fourth, the role of officer discretion is not clear and we will explore this further in a subsequent article. Finally, increased assaults against police may also be a corollary of the decreased use of force in some instances. Whereas police may have taken a tit-for-tat approach – using force when assaulted as a form of ‘natural justice’ – cameras may inhibit the reactive use of force and give officers the impetus and/or confidence to report the incident. The question about the reason for the increased assaults is not something that can be left to debate and must be scrutinised empirically.

Notes

1. <http://www.theguardian.com/us-news/ng-interactive/2015/jun/01/the-counted-police-killings-us-database>.
2. https://www.gov.uk/government/uploads/system/uploads/attachment_data/file/445667/police-officers-assaulted-mar15.pdf.
3. On other matters difficulties with estimating the casual effect of BWCs in policing, see Grossmith et al. (2015), Owens et al. (2014), and Ready and Young (2015).
4. There are more departments than jurisdictions because some police forces are from the same jurisdiction but cover different geographical areas.

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