



Open to Interpretation: Confronting the Challenges of Understanding the Current State of Body-Worn Camera Research

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Abstract

In only five years, both the implementation of police body-worn cameras (BWCs) and the evidence base evaluating the technology has diffused at a breakneck pace. As the number of studies has increased, so too has the uncertainty surrounding BWCs and their impact on various outcomes. In this commentary, we bring together the differing viewpoints on the five existing summaries of the BWC literature, highlight the key sources of contention, and make recommendations for BWC scholars and consumers moving forward.

Keywords Police · Body-worn cameras (BWCs) · Technology · Research base · Review

Introduction

By 2014 – just before the deaths of Eric Garner, Michael Brown, and Freddie Gray – police interest in body-worn cameras (BWCs) had grown slowly but steadily (Lum, Koper, Merola, Scherer, & Reioux, 2015; White, 2014). In 2013, roughly one-third of surveyed agencies had some form of BWC program, primarily small-scale pilot tests (Reaves, 2015). The explosive outcry and civil unrest following the deaths of Garner, Brown, Gray, and other primarily young, unarmed, minority men served as a flashpoint for the rapid diffusion of BWCs as a tool for police reform. By 2016, nearly half of law

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enforcement agencies had BWCs, including 80% of those with 500 or more full-time sworn officers (Hyland, 2018). This swift implementation of BWCs proceeded without a significant evidence base from which to discern best practices for policy development and program implementation.

BWC evaluations were conducted as early as 2005 in the United Kingdom and Canada, but they were methodologically weak and had limited generalizability (Edmonton Police Service, 2013; Goodall, 2007; ODS Consulting, 2011). The earliest study in the United States was conducted in Rialto (CA) in 2012, the first-ever BWC randomized controlled trial (RCT) (Ariel, Farrar, & Sutherland, 2015). Since then, a host of similarly robust studies have been undertaken in medium- to large-sized agencies across the U.S. and in the United Kingdom (see Lum, Stoltz, Koper, & Scherer, 2019). In fact, the research base has grown substantially over the last few years: In 2014, there were only five published studies (White, 2014); by June 2018, there were 70, “a 14-fold increase” (Lum et al., 2019, p. 96); and by late December 2019, there were 119.¹ Most early studies focused on the impact of BWCs on behavioral outcomes (i.e., use of force and complaints), and the findings were consistently positive. Moreover, statistically nonsignificant findings were still viewed, in many cases, to be substantively important. Over time, however, the evidence base on BWCs has become much more mixed (White & Malm, 2020).

Inconsistencies in the BWC literature have muddied the waters for practitioners and academics alike. Many researchers look to systematic reviews or meta-analyses—like those published by the Campbell Collaboration—to bring sense to a wide range of publications e.g., (Mazerolle, Bennett, Davis, Sargeant, & Manning, 2012). To date, there is no systematic review or meta-analysis of BWC studies.² However, five resources (see Table 1) have attempted to synthesize the disparate research on BWCs.

Unfortunately, these five summaries of BWC research vary considerably in their approaches, their analysis, and most importantly, their conclusions. Maskaly et al. (2017) and Malm (2019) are both positive in their assessment of the research base; Cubitt et al.’s (2016) conclusion is more measured, and Lum et al. (2019) are considerably more skeptical. Taken together, what do these summaries tell us about the state of BWC research? Do BWCs work? An assessment of this systematic research proves valuable in answering these questions. In particular, we are able to explore the implications of the mixed findings throughout the BWC literature. We compare the five available summaries of BWC research and, based on this review, draw our own conclusions about the state of BWC research, offering explanations for the variation in

¹ This includes: Peer-reviewed, empirical studies on any outcome related to BWCs; reports documenting agency evaluations of BWC trials or pilot tests; and reports documenting independent evaluations of BWC trials or pilot tests. In addition to these 119 documents, our search found one book, three reports detailing findings from the LEMAS survey (including BWC supplement), six summary reports, 15 commentaries, and 37 law review articles.

² Our focus here is on existing summaries of BWC research, the inconsistencies across those summaries, and the potential reasons (and implications) for those inconsistencies. This focus is certainly more modest than engaging in our own systematic review or meta-analysis of BWC research. We chose this focus for two reasons. First, our research questions center on what consumers of BWC research can learn from existing summaries, and differences in the conclusions of those summaries can reasonably be interpreted. Why are there differences? What are the causes of those differences? Second, Lum et al. (2019) note that they are commissioned by the Campbell Collaborative to complete systematic reviews, including meta-analyses, of BWC research across multiple outcomes. As such, this paper does not attempt to duplicate that effort.

Table 1 Summaries of BWC Research

Author(s)	Title	Type of Summary
Cubitt et al. (2016)	Body-Worn Video: A Systematic Review of the Literature <i>Australian and New Zealand Journal of Criminology</i>	Narrative review
Maskaly et al. (2017)	The Effects of Body-Worn Cameras (BWCs) on Police and Citizen Outcomes: A State-of-the-Art Review <i>Policing: An International Journal of Police Strategies and Management</i>	Narrative review
White et al. (2019a, 2019b)	Impacts of BWCs on Use of Force: Directory of Outcomes Impact of BWCs on Citizen Complaints: Directory of Outcomes <i>BJA BWC TTA website</i>	Visual summary of findings
Lum et al. (2019)	Research on Body-Worn Cameras: What We Know, What We Need to Know <i>Criminology & Public Policy</i>	Narrative review
Malm (2019)	The Promise of BWCs <i>Criminology & Public Policy</i>	Policy essay

both research findings and collective assessments of the body of evidence and recommendations for BWC researchers moving forward.

Summaries of Available Research

Cubitt et al. (2016)

Cubitt et al. (2016) conducted searches of the major criminology and criminal justice electronic databases (e.g., Criminal Justice Abstracts, SocINDEX), grey literature, Google, and Google scholar, with a search window of 1990 – August 2015. The August 2015 end date makes this resource the earliest published assessment of the BWC (also referred to as “body-worn video,” or BWV³) literature. The authors selected articles and reports if they described “an operational trial” of BWCs (Cubitt et al., 2016, p. 381). The authors identified 36 articles for full review and selected 11 for analysis (only studies involving operational trials of BWC were included): Five peer-reviewed studies of BWCs (three examined officer perceptions only) and six articles from grey literature (mostly police department reports describing internal evaluations).

Cubitt et al. (2016) describe the methodologies, results, and conclusions of the 11 articles, grouped as “RCTs of BWCs,” “Officer perception on function and outcomes of BWCs,” and “Grey literature evaluations of BWCs.” They conclude that “the quality of evidence around BWCs is largely weak,” as only two of the 11 articles employed an RCT design, and the authors acknowledge the limitations of the preferred shift-based randomization procedures (Cubitt et al., 2016, p. 392). Nevertheless, they note positive effects with citizen complaints, use of force, crime, and community perceptions.

³ The original article refers to BWCs as BWV; for the sake of consistency and clarity, we change “BWV” to “BWCs,” even in direct quotes.

Maskaly et al. (2017)

Maskaly et al. (2017) apply similar search criteria to Cubitt et al. (2016) with four academic databases, resulting in the identification of 52 articles, reports, and news articles.⁴ Maskaly et al. (2017) narrow the pool to 21 studies by focusing only on “empirical assessments and/or evaluations” (p. 673). The authors group the review of the studies into two categories: Effects on *citizens* (citizen complaints, citizen resistance against officers, citizen perceptions of police, and willingness to report a crime) and effects on *officers* (officer attitudes toward BWCs, office use of force, officer report writing, and evidence collection). They conclude that “the evidence from BWC evaluations suggests that the use of BWCs can have benefits across a number of citizen and officer outcomes including for police-public encounters” (Maskaly et al., 2017, p. 672). For example, they note that officers are “generally receptive to BWC adoption” (p. 685), and that BWCs influence both officer and citizen behavior—though not always as expected (Maskaly et al., 2017). They also identify several methodological issues (e.g., effect sizes, selection effects, treatment contamination) with current studies, noting these issues can influence the results from even rigorous BWC evaluations.

White et al. (2019a, b) Outcome Directories

The outcome directories developed by White et al. (2019a, 2019b) are notably different from the other four research summaries, as they are a visual resource for both researchers and practitioners. Specifically, they serve as a user-friendly tool summarizing the rapidly expanding—and increasingly mixed—body of BWC research on specific outcomes of interest. Each directory is an Excel spreadsheet that includes basic information on each published study and is presented in summary and detailed versions. White et al. (2019a, 2019b) identified studies for inclusion through a multi-pronged search process of academic databases, Google Scholar, other online resources, media outlets, and peer networks, and cross-checked against existing summaries of research such as Lum et al. (2019). For each study, both directory versions contain the agency being evaluated, the agency’s state or country, the researchers conducting the study (with a link to the study), the year in which the study was published, an assessment of the study’s methodological rigor using the Maryland Scale of Scientific Methods (Sherman et al., 1998), and a summary of the study’s findings using visual indicators (green down-arrow, red up-arrow, or yellow dot). They are not peer-reviewed, though each entry has been approved by the primary author of the study (or an independent reviewer) to ensure accuracy. Currently, two outcome directories have been completed (use of force and complaints) and are available on the website of the Body-Worn Camera Training and Technical Assistance team (TTA), with more in progress.⁵

The Use of Force Directory describes the 19 studies or reports examining the impact of BWCs on the prevalence of use of force (all studies published through March 2019). Eleven of the 19 studies report substantial or statistically significant reductions in use of

⁴ Maskaly et al. (2017) used Criminal Justice Abstracts, EBSCO Host, PsychInfo, and Google Scholar.

⁵ The TTA team provides support to all agencies that have received federal funding through the US Department of Justice Body-Worn Camera Policy and Implementation Program (PIP). One of the authors is the Co-Director of the TTA team.

force, post-BWC deployment. White et al. (2019b) conclude that “taken together, the current body of research suggests that police BWCs can lead to reductions in use of force by police.” Twenty of the 26 published studies or reports contained in the Citizen Complaint Directory report substantial or statistically significant declines in complaints following the deployment of BWCs. White et al. (2019a) determine “the current body of research strongly demonstrates that police BWCs lead to reductions in citizen complaints against police.”⁶

Lum et al. (2019)

In January 2019, Lum et al. (2019) published a comprehensive narrative review of all BWC studies published through June 2018. In addition to the standard academic database and Google Scholar searches, Lum et al. (2019) also note:

Since 2015, we have been collecting information from ongoing research projects through criminal justice conferences and symposia, grant awards from both government and nongovernment sources, and from colleagues in the field, which helped to identify studies that did not initially emerge in our database search. (p. 96)

The authors identified 70 empirical studies, making it the most comprehensive resource on BWC research to date. The authors separate studies into six categories (noting that a study could fall into multiple categories): *Officer behaviors* (32 studies; use of force, arrests and citations, etc.), *officer attitudes* (32 studies), *citizen behaviors* (16 studies; compliance with police, willingness to call the police, etc.), *citizen/community attitudes* (16 studies; satisfaction with police, etc.), *investigations* (7 studies; court proceedings and outcomes, etc.); and *organizational impacts* (8 studies; accountability, training, supervision, etc.).

In most cases, they identify positive findings (pp. 110-111):

“Officers seem supportive of BWCs, particularly as they gain more experience with them.”

“BWCs seem to reduce complaints against officers.”

“BWCs may curb some of the worst police behaviors.”

“Fears of depolicing from the use of BWCs have not been realized.”

“BWCs do not seem to have discouraged most proactive field contacts or officer-initiated activities.”

“For their part, citizens are also generally supportive of police using BWCs.”

“There is also likely to be a growing expectation among the public that adopting BWCs is a marker of a responsive, transparent, and legitimate police organization.” (see also White, 2019)

Despite these generally positive assessments across outcomes, Lum et al. (2019) take a “deeper dive” with each outcome, which lead them to question the strength,

⁶ The White et al. (2019) directories are outcome-specific. Studies that do not examine use of force or complaints are excluded. As a result, the directories focus on a much smaller universe of studies than Lum et al. (2019).

direction, and consistency of findings. For example, they note that “BWCs may curb some of the worst police behaviors but have little impact otherwise” (p. 110). In fact, their overall conclusion is quite skeptical:

Although officers and citizens are generally supportive of BWC use, BWCs have not had statistically significant or consistent effects on most measures of officer and citizen behavior or citizens’ views of police. Expectations and concerns surrounding BWCs among police leaders and citizens have not yet been realized by and large in the ways anticipated by each. [...] Policy implications from available evidence are not clear-cut, but most likely BWCs will not be an easy panacea for improving police performance, accountability, and relationships with citizens (Lum et al., 2019, p. 93).

Malm (2019)

Malm (2019) responded to Lum et al. (2019) in an accompanying policy essay. Malm (2019) acknowledges the rather cynical conclusion by Lum et al. (2019) but explores the BWC literature through the *Effect, Mechanisms, Moderators, Implementation, Economic Cost* (EMMIE) framework to investigate the variation in findings across outcomes. Regarding *Effects*, Malm (2019, pp. 119-122) finds promising results for reduced citizen complaints and use of force, noting:

“If an agency wants to reduce complaints against officers, it should consider a BWC program”.

“The positive results (and noticeable lack of negative results) [for use of force studies] over several sites is still a promising finding for police departments”.

“Taken en masse, however, the results are again promising and suggest that a BWC program can positively affect case outcomes”.

“Agencies implementing a BWC program should not automatically expect specific benefits (or costs for that matter) regarding officer activity as the evidence is far from clear”.

The EMMIE framework permits a review of factors that can mitigate positive effects, such as low activation compliance and department starting point (*Moderators*), as well as poor implementation. Malm (2019) delves deeply into those issues, and she offers an interesting perspective on the research to date:

There are no absolutes. After all, what social science program or strategy is effective 100% of the time? Medications approved by the U.S. Federal Drug Administration (FDA) rarely cure everyone afflicted with an illness, and many medications produce a host of side effects. Programming in criminal justice is no different. Even programs that are considered to have a robust evidence base, such as hot-spots policing, are still vulnerable to implementation issues, dosage concerns, and vagaries of reporting mechanism. We should not expect any difference with police BWC studies. (p. 122)

Differences in Interpretation

These summaries were published within a four-year window (2016–2019) and review between 11 and 70 studies. They applied similar search and inclusion methodologies and recognize mixed findings across outcomes. That said, they fail to reach consensus. Several of the resources hint at the possible explanations for this lack of agreement, but a more complete discussion is warranted. There are three categories of explanations for the interpretational division: 1) The state of the department and local context pre-BWC deployment; 2) the level of integration and cooperation across major stakeholders before, during, and after implementation; and 3) methodological concerns and variance in definitions across studies. The first two impact the outcomes themselves and the latter affect the interpretation of findings, but all of them can create confusion among researchers and practitioners alike when “taking stock” of the research base.

State of the Department and Local Context

The context of both the agency involved in a BWC study and the study itself are important points of reference when interpreting results (Arizona State University, 2018; White, 2019). In particular, agencies that decide to implement BWCs often have a very different starting point. For example, the Rialto Police Department (RPD) had a rocky history leading up to their BWC study, with several years of scandals and an attempt by the Rialto City Council to disband the police department⁷ (Arizona State University, 2018; Kelly & Reston, 2007). Chief Tony Farrar was hired in 2011 and implemented a number of reforms, including the deployment of BWCs using an RCT design, which led to substantial declines in both use of force and complaints (Ariel et al., 2015). Five years later, Sutherland et al. (2017) found these declines had persisted.

Some agencies, like RPD, begin at a low point, with substantial room for improvement. Others have taken steps to reform their agency prior to implementing BWCs. Both the Las Vegas Metropolitan (LVMPD) and Spokane Police Departments (SPD) implemented their BWC programs following involvement in Collaborative Reform⁸ and witnessed more marginal declines in use of force and citizen complaints. Even more pronounced are the experiences of the Washington (DC) Metropolitan Police Department (MPD) and Los Angeles Police Department (LAPD), both of which were under federal oversight through consent decree. After nearly a decade of federally-imposed reform efforts, neither agency experienced reductions in use of force or citizen complaints after BWCs were deployed (Alikhan, 2019; Yokum, Ravishankar, & Coppock, 2017). The link between successful completion of a consent decree and non-significant BWC findings is straightforward. Agencies find themselves under

⁷ In 2007, the Rialto City Council voted to disband the police department and the San Bernardino County Sheriff's Office made preparations to take over law enforcement operations within the city limits. This decision by the city council was later overturned by court order, and a new police chief was installed (Kelly & Reston, 2007).

⁸ Collaborative Reform is a U.S. Department of Justice technical assistance mechanism available to law enforcement agencies that “offers recommendations based on a comprehensive agency assessment for how to resolve...issues and enhance the relationship between the police and the community.” The form and function of Collaborative Reform has changed considerably under the Trump Administration (<https://cops.usdoj.gov/collaborativereform>).

federal oversight because of an established “pattern or practice” of unconstitutional behavior, often problematic use of force. The federal court-imposed remedies in such cases focus on core police operations, from hiring and training to supervision and discipline, in an effort to eliminate the pattern or practice. Agencies like LAPD and Washington DC Metro implemented BWCs *after* successfully remedying the unconstitutional behavior that led to federal oversight (e.g., excessive use of force). In fact, the Washington (DC) BWC evaluators highlighted federal oversight as a potential explanation for the nonsignificant findings (Yokum et al., 2017).

Given the different local stories, it is no surprise that there is a spectrum of effects, with Rialto on one end, Washington (DC) and LAPD on the other, and Las Vegas and Spokane somewhere in between. Scandal-ridden agencies typically suffer from organizational deficiencies that lead to excessive and unnecessary force, high levels of citizen complaints, and low reserves of police legitimacy (White, 2019). It is likely the magnitude of the reductions in force and complaints experienced by the RPD reflected, at least in part, the poor state of the agency prior to Chief Farrar’s arrival. Alternatively, professional organizations with robust training, effective supervision, and proper accountability systems have a vastly different starting point. Perhaps there were no large reductions in use of force and citizen complaints in Washington (DC) or LAPD because officers were already using appropriate levels of force (White & Malm, 2020). Malm (2019) takes this a step further, noting:

Suburban agencies with low crime and positive community relations are a marked difference from some urban policing environments plagued by high crime, low morale, scant resources, and tense relationships with the community. These starkly different contexts may be crucial in determining how BWCs are implemented, supported, deployed, and perceived by the community and the police. (p. 126)

Additionally, regional differences may play a role. Most BWC studies have been conducted in the western, midwestern, or northeastern United States. The “southern culture of violence” may affect how BWCs impact violent police-citizen encounters (Cardarelli, 1968; Kaminski, Jefferis, & Chanhatasilpa, 2000). Similarly, officers in the South may be more reticent to accept BWCs; the limited studies of officer perceptions among southern agencies have found measured support for the technology, but a notable concern related to media use of BWC footage (Fallik, Deuchar, & Crichlow, 2018; Jennings, Fridell, & Lynch, 2014; Smykla, Crow, Crichlow, & Snyder, 2016). The possibility of the “southern culture of violence” exerting a direct effect on BWC perceptions has yet to be tested, and would be a fruitful avenue for future research.

Implementation Process

The local story can also color the pathway to BWC implementation (Arizona State University, 2018; White, Todak, & Gaub, 2018b). For some, BWCs were forced upon the department or were made a condition of reform efforts, as in New Orleans (LA) or New York City; for others, BWCs were a swift reaction to a high-profile event, as was the case with Ferguson (MO), where officers were using BWCs within one month of the shooting of Michael Brown (CBS News, 2014). In some instances, however, BWCs

were a deliberate decision followed by careful planning, as was the case in Tempe (AZ; White et al., 2018b). The extent to which departments follow best practices in program planning and implementation can have a significant effect on outcomes such as the degree of resistance from officers, use by courts and other criminal justice actors, and reactions from citizens (White et al., 2018b).

In May 2015, the Bureau of Justice Assistance (BJA) released a National Body-Worn Camera Toolkit designed to provide BWC resources ranging from research to guidance on policy, training, and stakeholder engagement. The Toolkit includes a *Law Enforcement Implementation Checklist*, a best-practices guide for successful planning and execution of a BWC program, grounded in decades of research on criminal justice program implementation. White, Todak, and Gaub (2018b) analyze the Tempe (AZ) Police Department's BWC program, and they argue that the department's positive experience with BWCs is at least partially explained by adherence to the Checklist components. Recall Malm's (2019) analogy to medications approved by the FDA. To extend the analogy, medications not taken in the recommended dosage cannot be expected to cure an illness. Interventions—whether they be medication or BWCs—must be implemented properly to work. Understanding this background can contextualize findings across BWC studies.

Methodological Concerns

Substantial variation in the methods used by researchers *evaluating* BWCs can cause confusion for those *implementing* BWCs. Some studies rely on rigorous research designs, such as randomized controlled trials. Others use more simplistic designs that fail to include proper controls. Efforts to document the fidelity of treatment (i.e., BWC v. non-BWC conditions) also vary notably, as does the manner in which officers are assigned to those conditions (e.g., randomized, volunteers, etc.). Even the manner in which outcomes are defined by researchers can vary. Below we explore three different methodological concerns that can influence the interpretation of study outcomes.

Statistical Rigor Using the Maryland Scale of Scientific Methods

Sherman et al. (1998) developed the Maryland Scale of Scientific Methods (MSSM) to standardize the evaluation of scholarly rigor (see Table 2). This scale ranks studies on a scale of 1 (weakest) to 5 (strongest) based primarily factors tied to internal validity. Evaluations of BWCs generally achieve a 3, 4, or 5 on the MSSM, ranging from a comparison of comparable units (one receiving the treatment or intervention and one not receiving it; Level 3) to randomized controlled trials with treatment and control groups (Level 5). For example, evaluations of the Denver, Edmonton, Tampa, and Toronto Police Departments/Services (Ariel, 2017; Edmonton Police Service, 2015; Jennings, Fridell, Lynch, Jetelina, & Reingle Gonzalez, 2017; Toronto Police Service, 2016) assessed BWCs using Level 3 research designs. Several evaluations have used Level 5 RCT designs, including the Boston, Las Vegas, Milwaukee, Rialto, Spokane, Tempe, and Washington (DC) Police Departments (Braga, Barao, McDevitt, & Zimmerman, 2018a; Braga, Sousa, Coldren, & Rodriguez, 2018b; Lawrence & Peterson, 2019; White, Gaub, & Todak, 2018a; White et al., 2018b; Yokum et al., 2017). That said, studies lower on the scale cannot claim causality like a Level 5 study.

Table 2 Maryland Scale of Scientific Methods Hierarchy

Scale	Description of Research Design	Explanation
5 ^a	Randomized controlled trial (RCT)	<i>Randomization, absent evidence of systematic bias or contamination</i>
4	Before/after across multiple sites <i>OR</i> quality longitudinal design	<i>No randomization, but relevant controls are included</i>
3	Before/after with one site/group and a control site/group	<i>Control group is demonstrably comparable on relevant factors</i>
2	Cross-sectional comparison of treatment and control <i>OR</i> before/after of treatment group only	<i>Control group is not demonstrably comparable, or is absent</i>
1	Cross-sectional study of treatment group only	<i>Correlation based on analysis of treatment group only</i>

a. More recent versions of this scale include a 5* version, in which systematic reviews and meta-analyses are placed above RCTs (Ratcliffe, 2019). The original version by Sherman et al. does not include the 5* level

They also may not detect smaller effect sizes and a significant-but-small effect could be mistaken as nonsignificant findings. Thus, not all studies are created equal, and the potential for significant impact is influenced by the rigor (or lack thereof) of the research design.

Randomization and Contamination

Even among Level 5 studies, variations in randomization procedures can complicate the comparison of results. For example, several studies have randomly assigned BWC treatment among participants, but those participants are either volunteers or a mixture of volunteers and “volun-tolds” (e.g., Braga et al., 2018b; Ready & Young, 2015). This is a fundamentally different method compared to an RCT among an entire group of officers (e.g., full patrol division) where every officer has an equal (and random) chance to receive a BWC (e.g., White, Gaub, & Todak, 2018a). Adding to this confusion is the unit of analysis in question. The majority of BWC RCT studies randomize at the officer level; in other words, *officers* are assigned to either use or not use BWCs (treatment/experimental and control groups). However, several studies use *shift* as the unit of analysis (see Ariel et al., 2016a, 2016b, 2015; Henstock & Ariel, 2017; Sutherland, Ariel, Farrar, & De Anda, 2017). Results must be described using the correct language and interpreted with the appropriate limitations in mind. In the former, *officers with BWCs (treatment) were involved in fewer use of force incidents*, whereas in the latter, *fewer use of force incidents occurred during BWC (treatment) shifts*. This distinction is rarely made in practice, but it is extremely important to understand how this difference in methodology can affect how we talk about outcomes. Moreover, there is a vigorous debate among BWC researchers about randomization, particularly about treatment contamination (or spillover) and the “stable-unit-treatment-value assumption” (SUTVA; Ariel, Sutherland, & Sherman, 2018).

All BWC RCTs have been forced to deal with the SUTVA question. In studies that randomize officers, contamination occurs when officers in one group interact with officers from another group (e.g., both a treatment officer and a control officer respond to the same call together). This can be referred to as *external contamination*. Studies

using this method have documented wide ranges of contamination, from 20% in Las Vegas (NV; Braga, Coldren, Sousa, Rodriguez, & Alper, 2017) to 70% in Washington (DC; Yokum et al. 2017). Conversely, studies that assess shift-level changes in outcomes experience what can be called *internal contamination*. Officers in these studies may wear a camera during their shift one week but not the following week. Contamination in a shift randomization study is 100%, as the officers in the study move back and forth between treatment and control groups (White & Malm, 2020). The equivalent in a pharmaceutical RCT is study patients sometimes receiving the experimental medication and sometimes receiving the placebo. Every patient gets at least some of the experimental medication.⁹

Defining Outcomes

Lastly, BWC studies likely apply inconsistent definitions of outcomes. Outcomes such as “use of force” do not mean the same thing in every jurisdiction (Alikhan, 2019; Arizona State University, 2018). For example, some agencies do not include officer-involved shootings in their use of force data, since these incidents automatically trigger a more in-depth investigation and are maintained in a separate database. Similarly, some agencies document the deployment of a canine (releasing the dog) as a use of force incident, whereas others may only document actual bites. Table 3 demonstrates this difference by showing inconsistencies across jurisdictions regarding “reportable” use of force. A study of use of force in the City of Los Angeles would not include the same types of incidents compared to a study from Las Vegas or New York. The inconsistencies in definitions complicate efforts to compare outcomes across studies because it is not always a true “apples to apples” comparison.

Similar definition problems occur when comparing other outcomes across BWC studies, including citizen complaints, citizen resistance, and even officer injury. For example, are complaints from citizens and internal complaints (from co-workers or supervisors) counted similarly across studies? Is verbal noncompliance by a citizen treated similarly across agencies? Or officer injuries? White et al. (2018a) assesses BWC impact on officer injuries using data compiled for the Spokane Police Department’s annual Law Enforcement Officers Killed and Assaulted (LEOKA) data submission. Thus, “injury” was defined using standard practice in the United States. Conversely, an international, multi-site evaluation of BWCs included a much wider range of “assaults,” including verbal assault (Ariel et al., 2016c). Comparison of the results of these two studies would require adjustment for definitional differences. Agency differences in definitions may lead to substantially different outcome definitions—and even different results, as is the case with White et al. (2018a) and Ariel et al. (2016c).

Recommendations

We propose several recommendations for researchers studying the impacts of BWCs. First, the field needs a meta-analysis or systematic review analyzing all relevant outcomes. Lum et al. (2019) mention they are contracted by the Campbell

⁹ See Ariel and colleagues (2018) for a defense of the shift-based randomization.

Table 3 Reportable Use of Force, by Jurisdiction

Department	Firm Grip	Baton/OC	TASER	Pointing Firearm	Vehicle PIT/Spike
Los Angeles (CA) Police	Complained of Injuries	Only if Contact	Only if Contact	No	No
Los Angeles (CA) County Sheriff	Any Resistance	All	All	No	No
Las Vegas (NV) Metro Police	Complained of Injuries	All	All	No	Yes
New York City Police	Complained of Injuries	All	Includes Spark Checks	No	No
Washington (DC) Metro Police	Complained of Injuries	All	All	Yes	No

Table used with permission from Arif Alikhan, Director of Constitutional Policing and Policy at the Los Angeles Police Department (see Alikhan, 2019)

Collaboration to conduct a systematic review for each area of research discussed in their article. Importantly, these reviews will address several of the aforementioned complicating factors, such as “the context and location of each research study to examine how relationships between study design, location, timing, and methodological approach contribute to the findings of BWC research” (Lum et al., 2019, p. 111, endnote 4). Additional reviews should also be conducted, and they need to be regularly updated to keep pace with the rapidly-evolving landscape of BWC scholarship.

Second, we need consistency in unit of analysis and definitions to compare “apples to apples.” Some of these limitations are beyond the control of researchers (e.g., how an agency defines use of force), and there is little the researcher can do beyond back-end adjustments to the analysis. This is simply the reality of dealing with administrative data (Gaub, Wallace, Todak, & White, 2018). But when department-related variation is combined with disparities in research-related issues, it becomes quite difficult to compare studies. This contention is also consistent with Malm’s (2019) argument that “to resolve these dilemmas, the future of BWC research may have to be a more thorough, and more standardized approach to documenting implementation, organizational culture, technical support, and frontline motivations” (p. 126). Scholars also need to come to agreement on metrics—including basics like independent and dependent variables—such that “the plethora of individual case studies from single departments could be better coalesced into a wider research framework” (Malm, 2019, p. 126).

From an interpretation perspective, it is important to note that we should not overlook or undervalue the substantive importance of findings that do not reach statistical significance. This is especially true for outcomes with notoriously low base rates. We must expect low base rates in some outcomes (e.g., use of force) and high base rates in others (e.g., citizen perceptions), such that there is little room for improvement in the statistical sense. But changes that are substantively meaningful, even if not statistically significant, still have value to police departments and other stakeholders in the community. In a small department, a decline from five complaints to two has enormous value in terms of time and money saved investigating those three unrealized complaints.

Conclusion

Academics, practitioners, and the public view research through different lenses, but seek to answer the question at the crux of nearly all BWC research: Are BWCs a worthwhile investment? None of the summaries provided here fully answer this question, but we disagree with Lum et al.'s (2019) gloomy outlook of the state of BWC research. When viewed holistically, the literature shows that departments can enjoy substantial benefits from BWCs. But BWCs are one tool among many at an officer's disposal, and like every tool, they have limitations. Good planning and implementation are difficult and time-intensive, and there are numerous pitfalls—some expected, others unforeseen. Departments have struggled with privacy legislation, the monetary and personnel costs required for redaction and retention, and balancing accountability and transparency priorities among the agency and the public. Positive outcomes are by no means guaranteed, but in our view, the early story on BWCs is ultimately bright. This is no small feat given the weighty issues at play, the often-significant pressure to deploy BWCs quickly, and the high level of difficulty associated with planning, implementing, and managing a BWC program.

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