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ORIGINAL PAPER

The Effect of Police Body-Worn Cameras on Use of Force and Citizens' Complaints Against the Police: A Randomized Controlled Trial

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Abstract
Objective Police use-of-force continues to be a major source of international concern, inviting interest from academics and practitioners alike. Whether justified or unnecessary/excessive, the exercise of power by the police can potentially tarnish their relationship with the community. Police misconduct can translate into complaints against the police, which carry large economic and social costs. The question we try to answer is: do body-worn-cameras reduce the prevalence of use-of-force and/or citizens' complaints against the police?
Methods We empirically tested the use of body-worn-cameras by measuring the effect of videotaping police–public encounters on incidents of police use-of-force and

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complaints, in randomized-controlled settings. Over 12 months, we randomly-assigned officers to "experimental-shifts" during which they were equipped with body-worn HD cameras that recorded all contacts with the public and to "control-shifts" without the cameras (n = 988). We nominally defined use-of-force, both unnecessary/excessive and reasonable, as a non-desirable response in police–public encounters. We estimate the causal effect of the use of body-worn-videos on the two outcome variables using both between-group differences using a Poisson regression model as well as before-after estimates using interrupted time-series analyses.

Results We found that the likelihood of force being used in control conditions were roughly twice those in experimental conditions. Similarly, a pre/post analysis of use-of-force and complaints data also support this result: the number of complaints filed against officers dropped from 0.7 complaints per 1,000 contacts to 0.07 per 1,000 contacts. We discuss the findings in terms of theory, research methods, policy and future avenues of research on body-worn-videos.

Keywords Technology · Deterrence theory · Use-of-force · Police · Randomized controlled field trial · Body-worn-cameras

Introduction

In recent years the use of police body-worn-cameras by police has received extensive media attention. These devices are commonly believed to achieve several aims, including: reducing police use-of-force and complaints against officers, enhancing police legitimacy and transparency, increasing prosecution rates and improving evidence capture by the police. The publicity has been so great that many go on to assume that cameras can fundamentally change 'flawed' police practices. This was epitomized in a 2013 Manhattan Federal District Court ruling that ordered officers in a precinct of New York Police Department with the highest volume of stop-and-frisk to wear body-worn-cameras in order to prevent racial profiling. In a similar vein across the Atlantic, the College of Policing in England and Wales identified body-worn-cameras as *the* mechanism through which "dented public confidence" could be restored (BBC, 10/24/2013).

Despite great promises, there is no research evidence on the benefits of body-worn-cameras. Other than anecdotal data captured in non-controlled conditions, without comparison groups and without systematic gathering of evidence, no causal estimates of the outcomes of these devices exist. In this paper we report on the first randomized controlled trial using body-worn-cameras, which tested the effect of body-worn-cameras in Rialto across 12 months. The study focused specifically on use-of-force and citizens' complaints, which were hypothesized to be affected by officers wearing cameras, given the possible deterrent effect of the devices on noncompliant behavior.

The paper begins with a review of the literature on police use-of-force and citizens' complaints against the police. These aspects of police behavior and police performance represent two burning issues in American policing. Mistrust and a lack of confidence may already characterize some communities' perception of their local police force. The use of unnecessary or excessive force by the police serves to further damage this relationship. Similarly, complaints filed against police officers are central to policing, not only because scholars consider them a proxy of police–public relations and police misconduct, but also

because of their organizational importance given the tremendous costs associated with these cases, particularly in an era of austerity where many agencies are on the verge of bankruptcy (New York Times, 12/28/2013).

We then move on to describe the theoretical grounds for the hypothesized effect of cameras. A rich body of evidence on perceived social-surveillance—self-awareness and socially-desirable-responding—proposes that people adhere to social norms and change their conduct *because* of the cognizance that someone else is watching. Elaborate research across several categories of human behavior has shown that when certainty of apprehension for wrongdoing is "high", socially and morally unacceptable acts are less likely to occur. Both force and complaints are assumed to be undesirable "negative" events—though admittedly both can be necessary consequences of volatile police–public encounters—which should be kept to minimum. The devices are thus hypothesized to decrease the tension in encounters and consequently reduce these outcomes.

The methodology used to evaluate the body-worn-cameras is described next. We conducted a randomized controlled trial, where nearly 1,000 officer shifts were randomized over a 12-month period to treatment and control conditions. During "treatment shifts" officers were required to wear and use body-worn-cameras when interacting with members of the public, while during "control shifts" officers were instructed not to carry or use the devices in any way. We observed the number of complaints, incidents of use-of-force, and the number of contacts between police officers and the public, in the years and months preceding the trial (in order to establish a baseline) and during the 12 months of the experiment. We used three statistical methods to analyze the outcomes: first, a Poisson regression model to estimate the causal effect of the cameras between the treatment and control conditions. Second, we also conducted an effect size analysis, in order to measure the magnitude of the difference between the groups in terms of the rate of incidents per 1,000 contacts between the police and the members of the public. Third, we employed interrupted time-series analysis to assess the city-wide impact of the trial, before and after the implementation of body-worn-cameras. Finally, we discuss the findings in terms of theory, police policy and research methods. We pay particular attention to the possible causal mechanism behind the effect of the cameras on the use-of-force and citizen complaints, and our concerns with violations of the stable unit treatment value assumption when using the shift as the unit of analysis. Future avenues of research in this area are also suggested.

Literature Review

Police Use-of-Force

Police use-of-force has received considerable attention in various disciplines. This scholastic interest reflects significant investment by practitioners and decision-makers in better understanding the ways in which law enforcement institutions exercise their power, and how such powers can be managed. In democratic civilizations, the police are expected by the public to use force when the situation justifies the use of "reasonable" power "necessary" to achieve "legitimate purposes" (Alpert and Smith 1994; Bittner 1970). In fact, a defining characteristic of the policing profession is that it requires potentially using "reasonable" and "necessary" force, including deadly force, in a variety of chaotic situations that may be both (un)desired and (un)expected by members of the public. (On the

conditional support for police use-of-force by race, gender and religion, see Halim and Stiles 2001).

Historically as well as contemporarily, police scholars have argued that there is a "social contract" between the police and the citizens they "protect and serve", an idea dating back to Hobbes (1651), Locke (1689), Rousseau (1762), and more recently Pettit (1997) and Shapiro (2003). Collectively, this line of theorization purports that the police are responsible for safeguarding and protecting the general social order, which includes protecting the safety of the citizens and other police officers. In exchange for granting police officers the right, power, and responsibility to use force, citizens expect police to only exercise force when it is necessary and to only use the amount of force that is "reasonable", "proportional" and "necessary" for that situation. The police are, therefore, entrusted with the legal and moral responsibility to maintain societal order and these imperatives are implemented through their legitimized use-of-force. So important is "that responsibility, that police use-of-force is believed to involve the execution of perhaps the essential function of the state and…because it affects the public's attitudes and behaviors toward the police and government more generally" (Friedrich 1980: 82).

Research in the area of use-of-force by the police has emphasized two distinct situations viewed as undesirable: the "use of excessive force" by the police (which is when an officer uses more force than is necessary/justifiable/reasonable in a situation where *some* force was justifiable) and/or the "unnecessary use-of-force" (which is when force is used by an officer but *no* force was necessary/justifiable/reasonable in the context). These two types of situation are argued to damage the relationship of the police with the community that they are expected "to protect and serve" (Reiss 1968; Skolnick and Fyfe 1993; Worden 1996). When the police undermine these expectations and violate their contract with the citizenry over the use-of-force, police–public tensions rise (King and Waddington 2004; Weitzer 2000, 2002). When these violations amount to outright "police violence", the core foundation of police legitimacy is undermined (Westley 1970).

Explaining Police Use-of-Force

In the present study we focus on three strands of research that purport to explain police use-of-force: situational, psychological and organizational. In terms of situational factors, one leading theory is based on the notion that police behavior is influenced by the social dynamics of police–citizen encounters. Black's (1976) sociology of law, for instance, suggests that the "quantity of law" is associated with the attributes of the parties. Sherman (1980) developed this point further, by laying out the situational factors that form essential cues officers use to make an assessment about how an incident should be handled. Most empirical research that followed suggested that the suspect's actions and resistance during a police–public encounter precipitate the force reaction of police officers (Alpert and Dunham 1997; Alpert et al. 2004; Crawford and Burns 2002; Terrill 2001). This is what some observers have termed the "demeanor hypothesis" (see review by Engel et al. 2000). Using self-report data, Garner et al. (2002) have shown that the link between characteristics of the police–public encounter and police use-of-force is significantly dependent on resisting arrest (see also Croft and Austin 1987).

Demeanor is just one aspect of the situational dynamics that elicit police use-of-force. Was the encounter part and parcel of routine police work? Was this a high-crime area or a known and dangerous offender with whom the police interacted? Was the officer alone and therefore more easily threatened? These and countless other situational factors and interactions-between-factors may ultimately lead to use-of-force (see Terrill and Mastrofski

2002; Wikström et al. 2012), but we should resist simply listing situational 'risk factors' for use-of-force as this does not aid explanation.

Psychological theories of use-of-force suggest that police officers with certain psychological traits are more likely to use excessive use-of-force or use force more broadly. For instance, the ability to "endure" some stressful situations was found to be associated with personality types, while some police officers tend to accept some forms of disrespect but not others (Engel et al. 2000). More broadly, Fabricatore et al. (1978) have shown that certain personality factors, as measured by the Sixteen Personality Factor (16PF) Questionnaire, revealed that "aggressive" and "tough-minded" characteristics were consistent predictors of use-of-force. Burke and Mikkelsen (2005) as well as Phillips and Sobol (2011) subsequently found that cynical officers held more favorable attitudes towards the use-of-force, while officers reporting higher levels of professional efficacy held more favorable attitudes towards the use of social skills to solve problems. We read this body of instructive literature as suggesting that some psychological variables are important in any study of police use-of-force.

Finally, police subculture in relation to use-of-force seems to play a role in accepting or allowing for "more force" to be applied in certain circumstances. Researchers who study police organizations have been claiming for years that use-of-force is a function of police officers' attitudinal commitment to certain institutional or organizational cultures around their roles in society and, more broadly, their view of power (Terrill et al. 2003; Lester 1996). Certain institutional and subcultural codes make police agencies particularly resistant to cultural changes; indeed, as Skolnick (2008: 37) observed, the "unrecorded code [of silence] has been noted as a feature of policing across continents, wherever commissions of inquiry have investigated police corruption". Feelings of loyalty sustain this code of silence and make it particularly difficult to investigate purported unnecessary, or excessive, use-of-force. Similarly, Baker (Baker 1985: 210–213) has shown that there is a hierarchy of wrongfulness for police misconduct, which sits well with how officers view excessive or unnecessary use-of-force: "dead wrong; wrong, but not bad; wrong but everybody does it". So in order to understand police use-of-force, it seems clear that one must include officers' individual predisposition to use force, the situational dynamics of police work and the broader context within which officers' operate.

Measuring Use-of-Force

How much force is 'too much'? One possible way to assess levels of police force was introduced in the "*use-of-force continuum*" (see review in Garner et al. 1995), and is utilized by many police agencies worldwide as a standardized tool for measuring responses to varied types of situations.[1] The continuum has several steps of "proportional dosage", all the way up to lethal response and down to aggressive verbal response. Any response that is proportional based on this tool (including no physical force at all), can potentially be deemed as 'necessary' or 'excessive'. However, there is room for interpretation. In fact, some would consider the mere presence of a police officer and the concomitant physical representation of authority as "some" force.

The inherent subjectivity of the use-of-force continuum signifies just how complex and inflammatory force can be: Just what exactly does a police officer have to do before they are deemed to have "used force"? How much of a grab or a hold, or even a "come-along

[1] Notably, many agencies are moving away from a use of force continuum, making the force determination even more ambiguous.

hold," is needed before it becomes "use-of-force"? Even more difficult to define is at what point the use-of-force is either "unnecessary" or "excessive" (see Atherley and Hickman 2014; Alpert and Smith 1994). Adams (1996: 53) cites a famous disagreement between a team of field researchers led by Reiss (1968) and a panel of experts from the President's Commission on Law Enforcement and Administration of Justice (1976), which aptly describes the measurement problem. The two teams could not agree on what constitutes "improper use-of-force" or "excessive or unnecessary use-of-force", even though they were both scrutinizing the same incidents. Though dated, the problem they encountered still persists today. These perceptual differences are important not least because they indicate the illusiveness of defining (excessive) use-of-force, but they also tend to suggest just how real the measurement problem is: error has cut through both academic and professional arenas for more than five decades. In many ways this is because rules and laws relating to police use-of-force are simply "too vague to be regarded as a comprehensive set of operational guidelines" (Fyfe 1988: 180; see also Henderson and Wilson 2008).

Yet at the same time, there seems to be widespread agreement that both the rate and frequency of use-of-force are low (Alpert and Dunham 2004; Croft 1985). Croft and Austin (1987), Friedrich (1980), and Fyfe (1988), for example, have shown that the rate of use-of-force is about 5–10 % of police contacts with suspects.[2] Bayley and Garofalo (1989) have shown that it is when transferring arrestees that the majority of use-of-force incidents occur, but such activities represent a small proportion of police work. Similarly, Reiss (1968) has found that the likelihood of an excessive use-of-force incident is far greater when the police come into contact with suspects, however the police are dramatically more likely to contact non-criminal-suspects in their daily routines—suggesting that the rate of use-of-force is very low as well. Garner et al. (1996) have also found that even in cases of arrest the police used 'some' physical force in only one of every five incidents. According to their data, no force, or only low levels of force, was used in most cases.

When officers do use force they are nearly always required to file an official report of such incidents, but work by Adams (1996: 62) suggests that use-of-force occurs "twice as often" as suggested by official reports, particularly when the incident is one of "low-level force" that does not amount to anything the police officer feels he or she needs to 'account' for. Some ethnographic work in this area (e.g., Hunt 1985; Rojek et al. 2012) suggests that what is construed as a "reportable incident of force" and how much force is appropriate is often predicated by a police department's organizational culture (as noted above). For example, placing one's hand on another's shoulder in an authoritative way or using aggressive language may be considered use-of-force in some instances and for some individuals, whereas for others they may not. Measuring "injury" or "assault" is also likely to be challenged in terms of definitional threshold, as it is open to interpretation when there are no clear signs of physical contact.

Whichever definition one would use for police use-of-force, the fact remains that *any* level of force can have detrimental effects on police–public relations. It may take just one or a handful of cases to damage the legitimacy of the police. The challenge is heightened if the three parties to the encounter—the officer, the suspect and the public—hold opposing views about the necessity, reasonableness and "amount" of force that the police apply. It is particularly the case when most police–public encounters, though they often occur in

[2] For a more systematic account of rates and prevalence, see Adams (1996:85–91), see also Hickman et al. (2009) who estimate, based on three dozen recent publications, that police use or threaten to use force in 1.7 % of all contacts and in 20.0 % of all arrests; but cf. Garner et al (2002) who found that prevalence can increase to more than 58 % of police–public encounters.

public (i.e. outside), are often away from the public eye. Therefore, any comprehensive and effective approach to reducing use-of-force must simultaneously address as many antecedents of police use-of-force as possible, including the suspect's demeanor, the officer's characteristics, and elements of police organizational culture that allow for such incidents to take place. Completely eradicating *illegitimate* use-of-force is unlikely as *some* force will always be required against *some* offenders in *some* circumstances. Likewise, any approach should still allow for legitimate use-of-force to be used in cases when it is required to protect the public, but for all other circumstances, a reductionist approach should aim to dramatically 'cool down' encounters.

Citizens Complaints Against the Police

One way to measure police (mis)conduct and how the public view police actions is through the analysis of complaints lodged against police officers—even though the rate of complaints is usually very low, compared to the number of interactions between the police and members of the public. Box and Russell (1975: 315) claim that while "complaints are a very minor aspect of the administration of justice, they nonetheless concern a very fundamental democratic right to have redress against 'deviants' in the police force". These complaints refer to allegations made by citizens regarding the conduct of officers, in both voluntary (e.g., requests for assistance) and involuntary contacts (e.g., traffic violations). Subsequently, complaint procedures were designed by most police agencies to investigate these complaints of officer misconduct and punish guilty officers—although Walker (1997) suggests that punishment is often not the goal of most complainants. Still, the number of complaints can be used as a measure for how people the police encounter evaluate their performance, with a lower rate of complaints being a marker of greater public satisfaction, although there can still be the case of fear or cynicism about future reporting to the police.

Researchers have also used complaint databases to assess various types of legitimacy and justice-related outcomes. Braga (2008), for example, analyzed police complaint data in Boston as a proxy of community complaints against the police.[3] Likewise, Greene (1999) showed that complaints can be used to measure the extent to which focused aggressive police enforcement strategies can result in police misconduct. Subsequently, as shown in Braga's (2008) review, grievances allow researchers to assess just how police legitimacy is influenced by whether community members perceive police–public encounters that they were treated fairly, with respect and dignity by police officers (Tyler 2001). Whether complaints are in fact justified and can be substantiated has always been a matter of contention. It is not uncommon for some offenders—especially experienced ones—to complain as a form of retaliation against the police (see Waters and Brown 2000; Prenzler et al. 2010). Yet it is difficult to defend the argument that *most* grievances are 'bogus' or erroneous. Furthermore, complaints are a source of public dissatisfaction: literature on the "complainants' experience" suggests that a substantial proportion of complainants remain dissatisfied with key aspects of the complaints process (Waters and Brown 2000; Brown 1998; Maguire and Corbett 1991). The implications for police legitimacy are substantial, which makes reducing the rate of complaints a major goal of a police complaints and discipline system (Liederbach et al. 2008).

[3] Though not without reservations about the utilization of complaint data as a single outcome measure, as complaints produce low substantiation rates—frequently 10 % or less (Liederbach et al. 2008).

Cameras as a Deterrence Stimulus to Manage Police Use-of-Force

Several lines of research across many disciplines suggest that most species alter their behavior once aware they are being observed (Chartrand and Bargh 1999; Dzieweczynski et al. 2006; Jones and Nisbett 1971). A rich body of evidence on perceived social-surveillance—self-awareness (Wicklund 1975) and socially-desirable-responding (Paulhus 1988)—proposes that people adhere to social norms and change their conduct *because* of that cognizance that someone else is watching (Munger and Shelby 1989). It seems that knowing with sufficient certainty that our behavior is being observed (or judged) affects various social cognitive processes: We experience public self-awareness (Gervais and Norenzayan 2012; Duval and Wicklund 1972), become more prone to socially-acceptable behavior (Sproull et al. 1996) and feel a heightened need to comply with rules (Milinski et al. 2002; Wedekind and Braithwaite 2002; Barclay 2004).

Getting caught breaking rules is often registered as behavior that can potentially lead to negative consequences such as sanctions, an outcome most individuals wish to avoid (Klepper and Nagin 2006; Nagin 2013). Whilst strict rationality in all decision-making is a rather strong assumption (Kahneman 2011), experimental evidence demonstrates that individuals work to avoid negative outcomes, and show that individuals react compliantly to even small cues indicating that somebody may be watching: Priming cues signaling how we ought to behave can range from reputational concerns (Bateson et al. 2006; Burnham and Johnson 2005; Haley and Fessler 2005; Fehr and Schneider 2010) and feelings of shame, to fear of punishment for noncompliance (Boyd et al. 2010). Paradigmatically, these cues are more broadly explored under deterrence theory.

The theoretical roots of deterrence theory are found in eighteenth century enlightenment philosophy (Beccaria 1995). An extensive body of recent rigorous research across several categories of human behavior has since shown that when certainty of apprehension for wrongdoing is "high" and when the severity of sanction is substantial, socially and morally-unacceptable acts are dramatically less likely to occur (Von Hirsch et al. 1999; Nagin 2013). Particularly around crime and disorder, when consequences of apprehension are perceived as harsh (imprisonment, fines, etc.), people simply do not want to get caught.

Theoretically, cameras are likely to deter people from noncompliance with rules of conduct. Tilley (1993: 3–5) rightly pointed out that the camera may "fire a number of mechanisms", but that one prominent preventative mechanism of a cameras is that it "reduces… [noncompliance] by deterring potential offenders who will not wish to risk apprehension and conviction by the evidence captured on videotape or observed by an operator on a screen on which their behavior is shown" (see also Wikström et al. 2012 on the conditional relevance of controls). Much like sentient observers, mirrors, or even pictures of eyes, cameras not only make us continuously conscious of the fact that we are being watched, but also drive us to compliance. If we become aware that a video-camera is recording our actions, we may also become more conscious that unacceptable behaviors will be captured on film, and that detection is perceived as certain. "Getting-away" with rule breaking is thus far less conceivable if one is being-videotaped. Cameras can therefore be viewed as "credible threats" (Jervis et al. 1989: 3) within the wider context of deterrence messages, which in both self-awareness studies and deterrence studies has largely been missing.

This conceptual appeal of the impact of cameras on human behavior and the possible ramifications of their use on social-control-policies, have led to two primary lines of rigorous research on their effect. These studies collectively seek to understand how cameras can potentially deter rule-breaking behaviors, but each has focused on a different

subtype of recording devices: CCTVs and speed-cameras. Both types are meant to trigger the perceptual mechanism of self-awareness. First, CCTV (passive) cameras are placed in public-spaces in order to increase the perceived likelihood of being apprehended by offenders. The available meta-analysis of the evidence from 44 studies on the use of public-area CCTV has shown that the mechanism "works" in principle, insofar as cameras caused a modest (16 %) decrease in crime in experimental areas compared with control areas. However, this overall result was largely driven by the effectiveness of CCTV schemes in car parks, which caused a 51 % decrease in crime (Welsh and Farrington 2009) and not in more serious or violence crimes as these tend not to be deliberative. Second, speed cameras were found to reduce the incidence of speeding, road traffic crashes, injuries and deaths. A meta-analysis of 35 rigorous studies has found that, compared with controls, the relative reduction in the proportion of vehicles speeding was up to 65 % and up to 44 % for fatal and serious injury crashes (Wilson et al. 2010). However, how *body-worn-cameras* may be used to affect behavior and—specifically—that of police officers, is as yet unknown.

Hypotheses

As the literature review suggests, the most ubiquitous type of camera—mobile cameras—have been virtually ignored. What is their effect on self-awareness? Could they promote socially desirable behavior? Can they be used as a social-control mechanism? Although theoretically compelling, research on the link between self-awareness and socially desirable behavior in the context of cameras and police use-of-force is virtually non-existent. The only parallels we can draw are research on how highly-publicized and videotaped police encounters are perceived by the public and the effect that videotaped negative encounters have on police reforms. Such studies indicate that videotaped arrests, for instance, have a negative impact on citizens' perceptions of force used by police during such arrest situations (Jefferis et al. 1997). Similarly, the Rodney King incident has led to significant reforms in the Los Angeles Police Department (Levenson 1993).

Notwithstanding the lack of direct research, we hypothesize that portable cameras would go beyond the limited impact that CCTV has had on expressive acts of public violence. We believe that the reason CCTV cameras were found to be weak modifiers of offenders' behavior is because the level of certainty of being apprehended necessary for the self-awareness mechanism to trigger, leading to socially-desirable behavior, is not high enough in CCTV. If cameras are expected to influence behavior and to serve as cues that social norms or legal rules must be followed, then the cue "dosage" of awareness must be high. Mobile cameras, and specifically body-worn-cameras, are likely to have this effect. They are directly observable by the parties to an encounter whilst conveying a straightforward, pragmatic message ("*you are being watched, videotaped and expected to follow the rules*"), and they can almost guarantee apprehension for socially undesirable behavior, if that behavior is recorded.

Perhaps equally important is that mobile cameras can work on both sides of the police–public interaction—the police officer and the suspect. Put differently, because the camera is actually worn by one of the actors in the exchange, it acts as a neutral third eye, impacting both players' psyches. Cameras are thus likely to have a "self-awareness effect" that would both deter the police officer from reacting with excessive or unnecessary force, and cool down the "aggressive demeanor" of the suspect (or deter the police from interpreting demeanor in this way). In part, this is due to the "announcement effects" of

surveillance (see Surette 2005). Signage advertising the presence of camera surveillance is a factor that constrains behavior, which is pertinent in CCTV, gunshot detection technologies and the red light traffic camera literature (see Ratcliffe 2007). Assuming such situations are conducted deliberatively (thoughtfully) at least some of the time, neither the police officer nor the suspect want to get caught engaging in a socially undesirable behavior that may have costly consequences.[4] Because the encounter is captured on tape, it makes both parties more accountable, which is likely to reduce the likelihood of unwarranted levels of force—including "use of excessive force", "unnecessary use-of-force", and certainly "abusive use-of-force"—indeed, arguably, *any* kind of force.

Therefore, cameras sit well (though not without some reservations, explored later on) with all three major approaches to explaining use-of-force. First, cameras confront situational dynamics that precipitate suspects' "negative actions" that could potentially lead to "force reaction" by police officers. Cameras also "force" the officer to endure stressful situations and arguably accept some forms of disrespect that without the cameras he or she would normally not. Lastly, even police subcultures of acceptable but illegitimate force responses are likely to be affected by the cameras, because misconduct cannot go undetected. In essence an external set of behavioral norms is being applied and enforced. Police–public encounters become more transparent and the curtain of silence that protects misconduct can more easily be unveiled, which makes misconduct less likely.

In summary, deterrence theory *presupposes* that effective deterrence requires self-consciousness of being observed. When the perceived probability-of-apprehension is high, unacceptable behavior is less likely to occur. But the actualization of this awareness has rarely been investigated (*cf.* Nagin 2013). Across various disciplines, research has yet to unravel the threshold of cognitive attentiveness under which socially-*un*desirable behavior will not take place. Body-worn-cameras offer a neat solution to this problem because the certainty of apprehension for such behavior is apparent when the cameras are on. It follows that we can directly measure deterrence when the certainty-of-getting-caught for non-compliance is greatly intensified, if not guaranteed. Cameras can sensitize individuals to being watched and can therefore elicit desirable behavior. Thus, cameras are hypothesized to reduce the number of incidents of use-of-force, as well as the number of citizens' complaints lodged against officers.

Methods

Research Settings

We tested the effect of body-worn-cameras on incidents of use-of-force and citizens' complaints against the police in a randomized-controlled field-trial in Rialto, California. Rialto Police is a mid-sized department that has jurisdiction over 28.5 square-miles and services a population of 100,000 residents. The department employs 115 sworn police officers and 42 non-sworn personnel who deal with approximately 3,000 property and 500 violent crimes per year. In 2009–2011, the department dealt with six to seven homicides per year, which is nearly 50 % higher than the US national rate per 100,000.

[4] It is worth noting recent research that suggests that both internal and external controls are conditionally relevant and depend, in part, on the extent to which individuals deliberate (see Wikström et al. 2012).

The entire population of Rialto Police Department frontline officers participated in the experiment (n = 54), though we consider the shift to be the unit-of-analysis (see below). Frontline officers work 7 days per week, in six shifts of 12 h per-day, or a total 2,038 officer shift-hours per week. Each shift consists of approximately ten armed officers who patrol the streets of Rialto and interact with offenders, victims, witnesses and members of the public. When officers were assigned to treatment conditions (see below), they were instructed to "wear" HD cameras, which would then record all of these interactions, both visually and aurally, throughout the entire shift.

"Police Shift" as the Unit of Analysis

The unit of analysis we have utilized in this study is the officer's shift. Our choice poses a great deal of reservation on a number of fronts, which deserve scrutiny. However, given the rule of *maximin,* our unit of interest poses the best possible option, given the circumstances.

Ideally, we would have randomly allocated half the officers to treatment (wearing the cameras) and half to control (not wearing the cameras). This approach would have made individual officers the unit of analysis. However, assigning individuals proved impossible for several practical as well as methodological reasons. First, Rialto officers patrol in revolving teams and whilst patrols are routinely 'solo', patrols also occur in pairs or teams. This means that throughout the lifecycle of the study we would have introduced uncontrollable noise that we believed we would not have been able to account for when analyzing the results. A related problem is that while the "combinations of officers" constantly changes, some officers have shifts they prefer to work. Some opt for late shifts while others prefer to work night shifts. We could have potentially randomly allocated individual officers within two statistical blocks of shift type, but there was no theory behind this procedure that would have made the blocking efficient (see Ariel and Farrington 2014). Moreover, these shift patterns change over the course of a year—particularly when new recruits join the force and more experienced officers are assigned to mentor them.

Methodologically, planning a treatment group of 27 experimental and 27 control officers would have resulted in an underpowered study. Statistical power was defined by Cohen (1988) as the probability of detecting an effect where one exists. Only if we were to estimate that the anticipated effect of cameras is large, around 0.8 in Cohen's terms (Cohen 1988), with an alpha of .05 and power at 80 %, would n = 54 suffice. Therefore, we were reluctant to "design a study doomed to failure" (Clarke and Weisburd 1994: 179), solely due to an insufficient sample size.

Instead, the unit of interest in this experiment is the patrol shift. Using the shift sits well with police routine operations because tasking, deployment and resourcing revolve around the shift. Shifts are also easy to administer in an experimental context because there are a set number of shifts in any given week, and the number of patrolling officers within every shift is stable and predictable. In most circumstances, the shift entails a new "set" of encounters that are normally unrelated to other encounters in other shifts, so we assumed that each shift is independent of all other shifts. Of course, this assumption is plausible only in terms of the "interactees", that is the members of the public that the police come in contact with, not in terms of the police officers,[5] who may 'carry the effect' into the control

[5] Similarly, officers regularly encounter 'the usual suspects' on patrol, meaning that there is some dependence between shifts in terms of "interactees". Other research (e.g. Wikström et al. 2012) would suggest that even with variations in 'actors', there may be stable environmental cues that are conducive to specific actions, but the use of force by police still depends on the interaction between individuals and their settings (the situation).

shifts as well. We return to these issues in the discussion, though we believe that the shifts add an element of randomness to the encounters as well. For example, if officers had been randomly allocated into pairs and then the pairs randomly allocated into experimental and control conditions, one might have asked whether or not there was something in the pair dynamic that might have influenced the outcome. With the switching pairs into shifts—as police officers often do—this dynamic between pairs is randomized as well, thus somewhat mitigating the potential spillover effect.

Procedure, Random Allocation and Statistical Power

Starting on February 13th 2012 and running for 12 months, the experiment consisted of randomly assigning all police shifts to either experimental or control conditions. "Experimental-shifts" consisted of shifts in which officers were assigned to wear high definition (HD) audio-visual recording apparatus (see below). "Control-shifts" consisted of shifts in which officers were instructed not to wear the HD cameras. Integrity of assignment was measured by both measuring the number of "footage-hours" against the assigned shifts as well as dip-sampling dates of footage and monitoring that the officers wore the cameras as assigned.

The experimental procedure is illustrated in Table 1 below. As shown, there are 19 shifts during any given week and the 54 frontline-officers patrol in six teams: Two teams work day shifts, three shifts work nights, and two shifts are cover shifts. Shifts were randomly allocated to treatment and control conditions, using the Cambridge Randomizer (Ariel et al. 2012), on a weekly basis.[6] In total, we assigned 988 shifts over 12 months into 489 treatment and 499 control conditions. Using *G*Power* (Faul et al. 2007), we estimated a priori that this sample size, with alpha at .05 and power at .80, would enable detection of a standardized-mean-difference of 0.2 (Cohen 1988).

One concern with experimental assignment is equivalence of treatment and control units. We were not able present an assessment of *baseline* balance, prior to random assignment, as the units of analysis—the shifts—were randomly assigned on a weekly basis over a course of 12 months. Still, in Table 2 we assessed the extent of balance between the number of shifts allocated and days of the week post-randomization, which were both statistically non-significant.

Apparatus

We collaborated with *Taser Inc.*© to provide all frontline officers with their HD body-worn-cameras. These body-mounted cameras captured video evidence from the officer's perspective. Weighing 108 g and small enough to place on the officer's shirt pocket, the camera systems were affixed to the collar and could always be seen by people who came into contact with the police—although in order to make sure people were aware of cameras, officers informed 'interactees' with that they were being videotaped. The units were water resistant, videoed in color, with a battery lasting for at least 12 h, making the apparatus ideal for the shift patterns of Rialto Police. The officers were instructed to have the cameras on during every encounter with members of the public, with the exception of incidents involving sexual assaults of minors and dealing with police informants. All data

[6] We acknowledge that prior knowledge of shift assignment might give rise to expectation effects—so that we would not know whether changes in behavior arise directly because of the presence of a camera, or anticipation of wearing a camera.

Table 1 Example of Rialto Police Department patrol patterns random assignments

	Mon	Tue	Wed	Thu	Fri	Sat	Sun
Day shift	Exp't	Exp't	Control	Exp't	Exp't	Control	Exp't
Night shift	Exp't	Control	Control	Exp't	Control	Exp't	Exp't
Cover shift		Exp't	Control	Control	Control	Control	

Table 2 Trial measures by treatment allocation

Day of week	Treatment	Control	Total	Shifts	Treatment	Control	Total
Sunday	63	72	135	Day	189	177	366
Monday	78	64	142	Night	169	197	366
Tuesday	58	81	139	Cover	141	125	256
Wednesday	75	75	150				
Thursday	72	60	132				
Friday	72	76	148				
Saturday	71	71	142				
Total	489	499	988		489	499	98
	$\chi^2(6) = 6.8845$; $p = .332$				$\chi^2(2) = 2.5752$; $p = .276$		

from the cameras were collated using a web-based computerized video management system developed by *evidence.com*©. The software tracked and inventoried all *Taser Inc.*© video cameras evidence. The system automatically uploaded the officers' videos at the end of their shifts and the research team was granted full access to these rich data.

Measures

Use-of-Force

Rialto Police Department used a system called *Blue Team* to track "recorded" use-of-force incidents. This standardized tracking system enabled us to count how many reported incidents had occurred during the experimental period in both experimental and control shifts, and to verify the details of the incidents, such as time, date, location, and whether the officer or the suspect initiated the incident. Rialto Police Department records instances of use-of-force, which encompasses physical force that is greater than basic control or "compliance holds'—including the use of (a) OC spray, (b) baton (c) Taser, (d) canine bite or (e) firearm". These are the *types* of force responses that we considered as eligible use-of-force incidents. We operationalized the "use-of-force" dependent variable as whether or not force was used in a given shift.

We acknowledge that police software cannot "measure" the use-of-force, and that it is nearly always up to the individual officer to account for those incidents where force was used. Given the subjectivity of this variable and the measurement problems we reviewed

above, we therefore relied on these official written reports, but not without hesitation.[7] Specifically in our study, our dependent variable only indicates whether or not force was used, but it does not say "how much" force was used. The "amount" of force used is also up to the officer to write down, as he or she recollects it. For instance, if three police officers use force on one suspect in one event, it would be registered as "one use-of-force." Because the prevalence data are binary, even if there were one officer but two persons that the one officer used force on, it would still be counted as "one use-of-force" incident. Likewise, the variable does not say for how long the person was stunned with a Taser gun, or how many shots were fired against an aggressive suspect, or how many times he or she was beaten with a baton before lying down on the ground and being handcuffed.

Another limitation is that we did not know from the data which party instigated the use-of-force, which seems to be an important aspect of use-of-force (Engel et al. 2000). For this information, we relied on what the officers had written down (again, in *Blue Team*), but this is not necessarily an objective measure. We were also able to capture information on this question from the videotaped footage, but of course this only covers the experimental arm, not the control shifts. An alternative would have been to systematically observe *all* police–public encounters with research assistants ("ride-a-longs"), but this option went well beyond our research budget.

Citizen Complaints

In some ways, complaints compliment data on use-of-force (Pate et al. 1993). It is common practice for virtually all police agencies to have clear guidelines for citizens to file complaints against officers, though the rates of complaints vary dramatically between different forces. Nevertheless, analysis of departmental and citizens' complaints against police officers was shown to provide somewhat reliable estimates of use-of-force (McCluskey and Terrill 2005: 513). If this is the case, then we ought to use citizens' complaints as a proxy for incidents of use-of-force—though they can also be used as a measure of police behavior more generally. True, citizens can be very poor judges of what constitutes "force" and particularly so when it comes to excessive force, but these complaints do provide a glimpse into what the public perceives as "force".

Rialto Police Department tracked complaints against officers with software called *IA-Pro*. Formally, the system records citizens' complaints where the reporting party has filed a grievance for alleged misconduct or what they perceive as poor performance. We used the data captured on this system to count the number of complaints (of any kind) filed against Rialto police officers.

Contacts with the Public

We measured the total number of contacts between the police and the public in each arm. Any non-casual interaction with the public was recorded on the Department's computer-aided dispatch system (CAD). These included attending calls-for-service, formal advices given to individuals, collecting evidence and statements during any type of investigation

[7] As noted by Adams (1996:65), "although there are many attractive reasons for using official records in research on [use of] force, the strategy is not without limitations…some concerns are based on practical issues of how the data are collected…the quality of data (e.g., accuracy, dependability, and coverage)…can influence counts dramatically…more significant problem is that of missing data or information that should be available in record-keeping systems but is not."

and the like. With this variable we were able to compute the rate of incidents per 1,000 police–public contacts.

Baseline Data

Table 3 below lists the outcome variables at baseline, up to 3 years prior to the experiment. As shown, use-of-force is a relatively rare event, with approximately 65 incidents per year, or 1.46 incidents for every 1,000 police–public contacts. Similarly, complaints lodged by citizens against police-officers are very infrequent, with 24 grievances filed against officers in the year prior to the experiment (about 0.7 for every 1,000 contacts). Police–public contacts data show that, on average, police officers interacted with members of the public about 3,600 times-per-month, or approximately 42 recorded contacts per shift.

Statistical Procedure

We employed three analytical approaches to analyze the outcomes. First, we used a Poisson model to assess differences between experimental and control groups. Group assignment ("experimental shifts" [0]/"control shifts" [1]) was set as an explanatory variable, and the dependent variable was whether or not use-of-force occurred.[8] Second, for each outcome variable, we assessed the standardized mean difference for the rates of use-of-force incidents per shift. Third, we observed the number of use-of-force incidents and citizens' complaints that were recorded prior to the experiment and compared them to the figures during the year of the experiment, in order to enrich the analysis. This quasi-experimental approach was used in order to indicate how the *entire* police organization responded to wearing the cameras; assessing the city-wide impact of the trial by comparing the data before and after the implementation of body-worn-cameras.

Results

Use-of-Force

During the experimental period a total of 25 incidents of police use-of-force were recorded by Rialto Police Department, of which 17 occurred during control shifts and 8 during experimental shifts. These represent a mean rate of 0.78 and 0.33 incidents per 1,000 police–public contacts, respectively. Results from the Poisson model suggest a treatment effect on use-of-force {IRR = 2.08; [95 % CI .91–4.78]}[9] meaning that the incident rate in the control condition is roughly twice that of the control condition (Table 4). Similarly, when we measure the magnitude of the difference in terms of rates per 1,000 encounters (dividing the number of incidents by the total number of contacts in each arm of the experiment), the effect size was statistically significant {SMD = 0.140; [95 % CI .015–.265]}.

[8] Poisson is appropriate here because each event has a small probability in each shift, and there are many shifts.

[9] Note that we have reverse coded the treatment conditions so that 1 = control and 0 = treatment, meaning that ratios reflect the incident rate of the outcome occurring for the control condition versus the incident rate of the outcome occurring in the treatment condition.

Table 3 Use-of-force, citizens complaints and police–public raw figures—baseline and experimental raw data

	2009–2010	2010–2011	2011–2012	2012–2013[a]
Use-of-force	70	65	67	25[b]
Complaints	36	51	24	3[c]
Police–public contacts	–[d]	–[d]	45,104	43,289

[a] Experimental period

[b] 8 during experimental shifts, 17 during control shifts (n = 499)

[c] 2 during experimental shifts, 1 during control shifts (n = 489)

[d] Data automatically collected starting in 2011

Table 4 Generalized linear model with Poisson distribution and log link for use-of-force (n = 988)

	Parameter estimates			
	IRR	Robust SE	95 % CI	
			Lower	Upper
Phase	2.082[†]	.883	0.907	2.082
(*Intercept*)	0.016	.006	0.008	0.016

[†] $p \leq .10$; * $p \leq .05$; ** $p \leq .01$; *** $p \leq .001$

We have also detected large before-after reductions in prevalence of incidents of use-of-force force (see Table 3; Fig. 1): 64.3 % reduction from 2009, 61.5 % from 2010, and 58.3 % from 2011, with a significant before-after effect based on the interrupted time series model (Table 5) (the ARIMA model parameter for the phase of intervention is −3.50 (SE = 0.689); $p \leq .001$).

Citizens' Complaints

In terms of complaints against officers, the between-groups treatment effect was not statistically significant,[10] largely because of the overall low occurrence in *both* treatment and control conditions. We observed only three complaints in total—one complaint lodged for an incident that occurred during control conditions and two for incidents that occurred during treatment conditions (all three occurred in August and September). We did, however, observe a significant, overall reduction of citizens' complaints, from 24 complaints filed in the 12 months before the trial to three during the trial period. The raw year-to-year reductions (Fig. 2) suggest 92 % fewer cases compared to 2009, 94 % compared to 2010, and 88 % compared to 2011—or 0.7 complaints per 1,000 contacts to 0.07 per 1,000 contacts. These reductions are mirrored by the interrupted time series model (Table 6), which resulted in a significant estimated parameter for the experimental phase of (−1.750; SE = .665; $p < 0.01$).

[10] Results not shown in tabular form, but given here: {B = −0.713; [95 % CI −3.112 to 1.685]; p .560}.

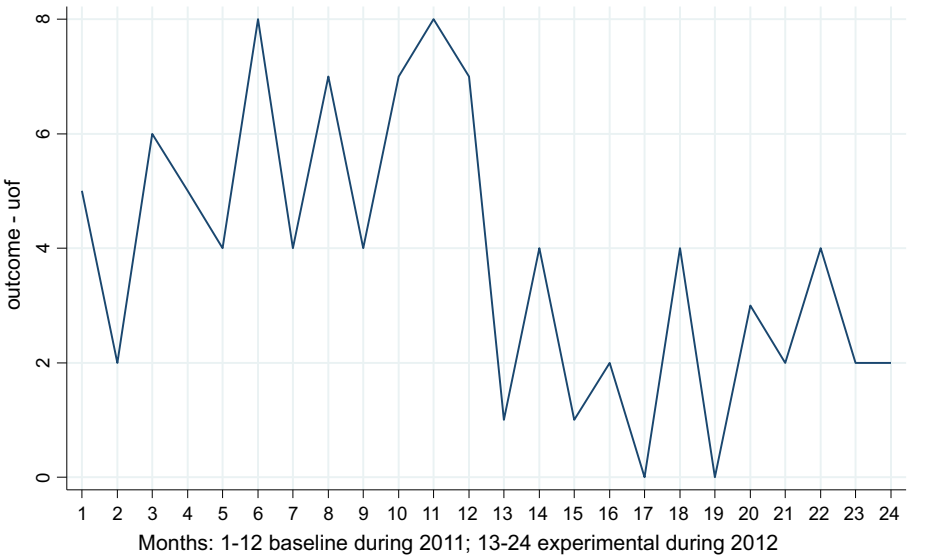

Fig. 1 Time series *line plot* for number of use-of-force incidents over 24 months (12 months pre-intervention; 12 months during intervention)

Table 5 Time series ARIMA model for use-of-force before/during experimental period ($t = 24$)

	Parameter estimates			
	B	OPG SE	95 % CI	
			Lower	Upper
Phase	−3.5***	.689	−4.850	−2.150
(*Intercept*)	5.583	.427	4.747	6.420

* $p \leq .05$; ** $p \leq .01$; *** $p \leq .001$

Discussion

In this experiment we tested, for the first time, the effect of mobile cameras on police use-of-force and citizens' complaints. The outcomes suggest a reduction in the total number of incidents of use-of-force in experimental conditions compared to control conditions. We have also observed nearly ten times more citizens' complaints in the 12-months prior to the experiment, compared to any of the 3 years prior to the experiment. In practical terms, the study provides law enforcement agencies with a methodology that may substantially reduce force responses, as well as reducing the incidence of complaints. This behavioral modification may be of real practical significance to the police, especially given the cost-to-benefit ratios (which we will present below). We therefore envisage that body-worn-cameras may noticeably affect police–public encounters. We acknowledge that this may pose ethical concerns, which we discuss below, but we believe that, on average, the benefits of using body-worn-cameras may outweigh the costs. (Issues that warrant further attention are whether using cameras reduces the likelihood of victims actually reporting crimes, and broader questions about victims' rights and procedural practice.)

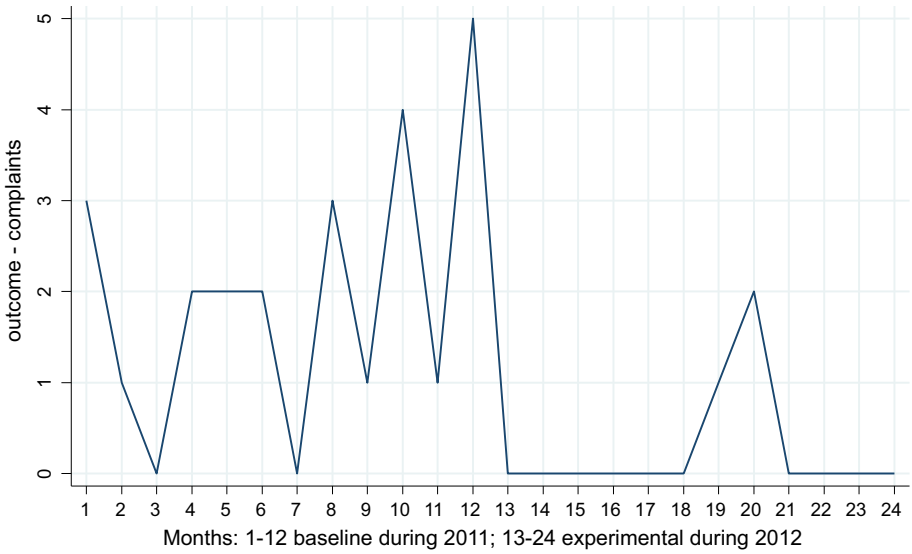

Fig. 2 Time series *line plot* for number of complaints over 24 months (12 months pre-intervention; 12 months during intervention)

Table 6 Time series ARIMA Model for complaints before/during experimental period ($t = 24$)

	Parameter estimates			
	B	OPG SE	95 % CI	
			Lower	Upper
Phase	−1.75**	.665	−3.053	−.447
(*Intercept*)	2	.265	1.481	2.519

* $p \leq .05$; ** $p \leq .01$; *** $p \leq .001$

The findings have implications, more broadly, for deterrence theory. Generally speaking, the proposition that videotaped police–public interactions "experience" fewer incidents of use-of-force seems to be supported by the evidence. We interpret this to reflect a fundamental tendency of humans to exhibit more desirable behaviors when they know they are under surveillance and subject to rules, but we acknowledge that even this will vary depending on the situation (see Wikström et al. 2012).

Our study, as far as we can tell, is the first to use the shift as the unit of experimental analysis. Using police shifts has its disadvantages, but on the other hand the benefits should not be overlooked. There are clearly more shifts than police officers, which increases the statistical power of any test. Here, instead of 54 officers split into treatment and control conditions, we have had 988 shifts, which we estimated using power analyses to be sufficient in order to detect even relatively small effects (Cohen 1988; Faul et al. 2007). To be sure, many studies in criminology are believed to be underpowered, thus potentially concluding that treatments do not work when in in fact they do work (Weisburd et al. 2001; Ariel 2009). Therefore, using shifts allows researchers to increase power without prolonging the study period or increasing the number of cases.

What deserves consideration, however, is the potential spillover effect that cameras have had on officers' behavior during *control shifts*. The reduction in use-of-force, coupled with a reduction in citizens' complaints, was registered across both study arms, which suggests that the effect of being observed during experimental shifts diffused to control shifts. These findings present conceptual as well as methodological challenges: How should the spillover be handled, and what is the right way to interpret these patterns? Answering these questions may also provide a better understanding of what future research avenues in this area might look like.

Just Another Hawthorne Effect?

Somewhat crudely, we could argue that it is difficult to attribute the reduction in both use-of-force and complaints to anything *but* the effect of the cameras. We would argue that cameras have modified the perception of individuals about what socially acceptable behavior *should* be in police–public interactions and, in turn, they have changed their responses even when officers did not wear the cameras. Critically, however, we must accept the possibility that the effect may have followed from either Hawthorne/John Henry (Saretsky 1972) effects or reporting artifacts. At least for reporting bias, it seems that we do not have a way to address this problem under the current research settings. One method would be to contact *every* individual that has interacted with the police during the study period, and survey his or her views on the encounter. But this approach only minimally addresses the (non-) reporting bias, as we have no objective way to ascertain that the recorded police–public encounters represent the entire population of encounters.

In terms of the Hawthorne and John Henry effects, we acknowledge that interference risks potentially characterize our study. As laid out by Sampson (2010: 492), Rubin (1990: 282), Cox (1958) Holland (1986) and others, the "stable-unit-treatment-value assumption" (SUTVA) may be a real threat to experiments and specifically for the reach of our conclusions. SUTVA refers to situations where dependency exists between the units in an experiment. In other words, units (and their outcomes) are not independent of one another. Violations of SUTVA create difficulties in making causal claims about the relationship between the manipulation and the dependent variable. If ignored, SUTVA violations have the possibility of adding bias to estimated treatment effects, and the bias can go in either a positive or negative direction (Alvarez and Sinclair 2009: 3). Here, the same participants, all of whom were participating in the same program, experienced both treatment and control conditions. This means that we cannot rule out interference and there may be a spillover of treatment effect to control and within treatment units. In fact, a large body of knowledge in criminology is clearly suggestive of social processes that could explain the reduction in use-of-force beyond the manipulation—peer pressure, social desirability, deterrence, leadership, perceptions of danger and crowd influence are only some of the micro-social elements that could lead officers to control or not control themselves.

On the one hand, treatment contamination such as in the case of SUTVA 'simply' makes it more difficult to detect a significant effect. If control cases are also treated (or, vice versa, when treatment cases are not treated), then in practice the crossover merely would require the treatment to exert a stronger effect in order to be observed, above and beyond the noise created by the violation. Therefore, if a significant outcome is detected even with SUTVA violations, than essentially it can be argued that the treatment is nevertheless still effective. The trouble is that there is currently no statistical fix for SUTVA violations (Berk 2005: 7; Sampson 2010) and we cannot "solve" the SUTVA problem with statistical modeling (Berk 2005). However, we can try to specify how it actually occurs and supplement the main analyses with additional observations that can, to some extent, address

the SUTVA violations in the context of the cameras treatment effect. Let us first go back to our research question: what is the effect of cameras on use-of-force? Does being observed (with a camera) elicit socially acceptable responses? Now let us return to the Hawthorne bias: changing participants' behavior because they are being observed, despite any treatment effect (work environment, etc.). When considered this way, it looks like the hypothesis and the observer bias are very similar. If the causal mechanism behind the body-worn-cameras *is* the observer effect, then more than anything else, we manipulated a "Hawthorne treatment" under controlled settings, thus concurring with previous research on the effect of focused attention on outcomes. Similarly, the John Henry effect may actually be construed as a positive outcome under these settings, as well. "Members" of the control group were fully cognizant of their status as members and were able to compare their performance with that of the treatment group, and it seems that they attempted to overcome the "disadvantage" of being in the control group by "behaving" themselves while *not* on film. Therefore, if our interpretation is reasonable, the study provides direct evidence on how repeated and systematic exposure to a stimulus that elicits deterrence can change behavior, even when the stimulus has vanished. Put differently, this is learning: it seems that people learn, by their exposure to observation, of what normative or appropriate reactions are, *even when they are not under surveillance anymore*. That both John Henry and Hawthorne effects may be in play simultaneously and we still find differences suggests that the effect of body-worn cameras may be much stronger under 'cleaner' randomisation designs.

Still, SUTVA is not just about observer's bias and there may be other mechanisms in place that cause interference, some of which we have listed above. Because the unit of analysis was the shift, we can think of a number of officer-based variables, for example, that might explain the change in behavior. Some officers have "thick skins" and would not respond with "too much force" to a resisting suspect. Others are more sensitive and would subdue such a suspect with "more force". The "amount of response" to such demeanor seems largely dependent on the cognitive and emotional capacities, as well as the training and experience of the officer (see Paoline and Terrill 2011). Therefore, if the same officer is in the habit of responding with a particular "response dosage" to certain police–public interactions, then it is likely that he or she would spill-over such reaction between the study units—that is, from one encounter to the next.[11] We invite future research to look more closely into this possibility.

Estimating the Costs and Benefits of Body-Worn-Cameras

Combining costs data from the experiment with figures from Finn (2001), the Minneapolis Civilian Review Authority (1997), Walker et al. (2002) and Metropolitan Police court settlements (BBC, 11th May 2012) we have crudely estimated the dollar benefit-cost ratio to be approximately 4:1 (details available as a supplement to this article).[12] That said, there are wider social and ethical costs to using these cameras. If body-worn-cameras become

[11] As Aristotle observed: "*We are what we* repeatedly *do*".

[12] One benefit which we have overlooked but should be closely observed in the future is the "training potential" of body-worn-videos. Rialto officers downloaded their own footage in order to view their interactions on a routine basis. Much like surgery, football or acting, the footage recorded by police body-worn-videos can be used to "coach" police officers, about how they conduct themselves. We envisage future police training to incorporate one-on-one sessions in which junior officers train with their own footage, about police conduct and potentially improve their demeanor when dealing with suspects, victims and witnesses. The benefits associated with such an impactful evidence-based approach to training through digital coaching, for procedural justice, distributive justice and police conduct more generally, should be an area of future investigations.

common practice, it means more electronic surveillance, more digitized tagging of individuals, and more challenges to privacy rights. This was certainly the argument against CCTV, as there are clear ethical considerations to having a data storage policy that routinely collects data on citizens in the public domain (Brey 2004; Spinello and Tavani 2004; Duff and Marshall 2000). CCTV surveillance captures the routine behavior of citizens whose consent is not obtained prior to their being observed and is now so much a fabric of life as to be 'banal' (Goold et al. 2013). Whilst the moral argument against CCTV is not of the same scope and magnitude when it comes to police body-worn-cameras, it is an open question as to whether police–public encounters should be routinely filmed and what threats to rights this practice might represent.[13] Victims and witnesses might expect that their communication with officers of the law is well-documented (beyond contemporaneous note-taking by police). Either way, one area that body-worn-cameras might be a tool to potentially improve the quality of interaction is when police encounter members of minority groups, particularly if officers are more mindful of the need procedural fairness and to be respectful (Tyler 2001). If the "legitimacy benefits" associated with wearing cameras—economically, socially, and culturally—exceed the "costs" of the cameras, we sense that body-worn-videos will become increasingly popular amongst officers.

On the other hand, future research should be mindful of at least two "prices" that are presently unclear. First, what are the direct and indirect costs of storing, sharing and managing digital evidence? The velocity and volume of data accumulating in police departments—even if only a fraction of the number of recorded events turn into "downloadable" recordings for evidentiary purposes—will exponentially grow over time. User licenses, storage space, "security costs", maintenance and system upgrades can potentially translate into billions of dollars worldwide (see Grossman 2009; Nambiar et al. 2014).

Second, the cost of *not* having video footage may have direct implications on decisions to prosecute or criminal proceedings more generally. Historically, evidence given by police officers in court against defendants—particularly testimonies of response officers—carried tremendous weight. The officer was able to characterize the suspect's demeanor, explain what was in the scene of crime and provide overall crucial details pertaining to the case. To a large degree, the assumption of credibility is generally made by the courts, unless challenged by the defendant. Yet it is very likely that defense attorneys, judges, the jury and the public as a whole would steadfastly assign more weight to digital evidence, arguably even more than officers' testimonies. This may be viewed as a "good" thing, yet it has indirect but important costs on policing: would district attorneys in domestic violence cases be reluctant to prosecute when there is no evidence from body-worn-videos to corroborate the testimony of the officer (or even the victim)? Would cases be dismissed if arrests or stop-and-frisk were conducted without a body-worn-video, given the possible violation of human rights (i.e., a similar approach as the Miranda warning)? Will officers' credibility in court be assumed to be violated *ex ante* when police–public encounters are not recorded? These are substantial effects that future research should be mindful to explore, that can potentially offset the benefits of body-worn-videos.

Research Limitations

Thus far we have ignored how cameras affected the citizens the police came into contact with, meaning that our analyses do not directly address the demeanor hypothesis. However,

[13] If we assume that all members of public encountered by the police are 'criminals' then it might well be justifiable, but this is obviously untrue.

we have found that, at least as officially recorded by the police (bearing in mind caveats associated with this data source), it is nearly always the case that officers *responded* with force and did not initiate the force response. We have also found that alcohol is a factor in more than half of all cases of use-of-force, which merits further attention in future research (see supplementary material). More broadly, we do not know on which party in an encounter the cameras have had an effect on, or how the two effects—on officers and on suspects—interact. This means that the estimated causal effect on officers' use-of-force conflates these mechanisms: Do cameras affect the conduct of suspects, which then moderates the need of officers to react with force to such behavior? Or do cameras affect the conduct of officers, who might have otherwise acted with unnecessary or excessive force *regardless* of the suspects' demeanor? Does it have a double effect?

Nevertheless, while it is difficult to isolate the mechanism in play, we can at least suggest that cameras have affected the overall result of police–citizen encounters. Whether police use-of-force—justified or unwarranted, excessive or proportional, reasonable or unreasonable—is a function of suspects' demeanor, or whether it is caused by unprofessional or inexperienced officers, the *circumstances* in which use-of-force occurred have changed, and resulted in what can be interpreted as a socially desirable response: force-free police–public encounters. "Human beings are norm users," MacCormick (2007: 20) reminds us, "whose interactions with each other depend on mutually recognizable patterns that can be articulated in terms of right versus wrong conduct, or of what one ought to do in certain settings." Simply put, the cameras communicated the deterrence message, through self-awareness of being observed, that the acceptable behavioral response in a given situation was not one of force. In short, whether they affected officers, citizens, or both, body-worn-cameras resulted in less force.

On the other hand, our unique research settings cannot be overlooked. Rialto, after all, is a small force with a dedicated Chief who has directly managed the experiment. This model may work well in a relatively limited force and when the "pracademic" (practitioner-academic) involved in the study is the director/chief of police, but the effect of body-worn videos may not work in the same way when the pracademic is less influential in the organization. More research is therefore needed to replicate our design in larger forces and different organizational frameworks, when a middle-level manager is directly involved in the daily affairs of the experiment (see Strang 2012)—which is a more likely context in larger police departments involved in randomized controlled trials or field research more broadly.

Finally, there is something to be said about treatment fidelity. For the purposes of this experiment *every* crime type and virtually all encounters between the police and the public were assigned to recording as well as to a verbal notification by officers that the encounter is videotaped. Yet we do not know how well the requirements were implemented, and it is difficult to estimate the fidelity of the intervention. There are three areas however that future research should focus on in order to assess the implementation of treatments with finer integrity: first, by measuring the number of video uploads/downloads during *control* condition, researchers would be able to ascertain whether violations of control conditions were made, and in which cases. As far as we can tell, contemporary back-office digital storage systems (cloud-based or stand-alone) can enumerate all recorded events by time-stamps, and so violations of control conditions can be accurately and systematically measured—hopefully *during* the experimental period in order to confront these violations and deal with them as they occur, not *ex post facto* in the analysis stage. However, estimating the violations of treatment assignment—that is, when cameras are assigned but are *not used*—is trickier and proxies should be used instead. Ride-a-longs, surprise visits and dip sample interviews with victims or suspects, asking them whether they recall the

officers wearing body-worn-videos or not, are equally useful, but they would provide estimates rather than a comprehensive approach to tracking random assignment fidelity (see Sherman 2013). Nevertheless, measuring the implementation is undoubtedly crucial in order to understand the effect of wearing body-cameras—especially in studies that would provide officers the power of discretion about when to use the devices.[14]

Conclusion

Regardless of the reason for the contact—initiated by the member of the public, or involuntary and initiated by the police—when members of the public sense they have had a "bad experience" during the encounter, they are nearly 15 times more likely to evaluate the police negatively—and this negative attitude translates into complaints (Skogan 2006; see also Rosenbaum et al. 2005; Hinds 2009). Similarly, the use-of-force by police, particularly if excessive, has a lasting effect on public perceptions of police and police-community relations. Finding ways to ameliorate these two negative outcomes was the driver for the present study. We have reported results from the first trial in the world to assess the effects of police body-worn-cameras on use-of-force and complaints against the police. To handle the small number of officers in Rialto Police, we took the innovative approach of randomizing police shifts to treatment and control conditions. Based on evidence collected in this randomized controlled field trial, our findings suggest that police body-worn-cameras reduce the prevalence of use-of-force by the police as well as the incidence of citizens' complaints against the police. However, this is but one experiment and before this policy is considered more widely, police forces, governments and researchers should invest further time and effort in replicating these findings.

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[14] We argue that experiments that allow treatment-providers full discretion about when to give or not to deliver the treatment(s)—and with what dosage levels—are generally poor designs. If the study results in non-significant outcomes, then it would be very difficult to interpret the findings—are they due to fidelity failure or that the treatment 'actually' do not work in the hypothesized direction? Moreover, even if the study results in significant results, the magnitude of the treatment compared to control conditions would be either inflated or deflated and therefore misleading, depending on how the treatment-providers decided to contaminate the treatment delivery. These scenarios may have adverse impacts for any attempt of conducting reliable cost-benefit analyses, or at the very least force researchers to dabble in conversions, transformations and statistical corrections which may or may not work—but anyway take away from the 'cleanliness' of controlled experimental design. We are cognizant that in real-life, non-experimental settings police officers may end up owning the power of discretion when to use or not use body-worn-videos, yet at this stage of our knowledge on the potential effect of these novel devices, experimentalists should encourage the use of strict protocols with as little discretionary powers as possible, before making policy recommendations.

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