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Body Worn Cameras, Procedural Justice, and Police Legitimacy: A Controlled Experimental Evaluation of Traffic Stops

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ABSTRACT

Police legitimacy is generally regarded as a view among community members that police departments play an appropriate role in implementing rules governing public conduct. Placing body worn cameras (BWCs) on police officers has been suggested as a potentially important response to police legitimacy crises. We use a rigorous controlled quasi-experimental evaluation to test the impact of BWCs on citizen perceptions of procedural justice and police legitimacy during traffic stops in Turkey. Relative to stops by officers without BWCs, we find that motorists stopped by officers with BWCs reported improved perceptions of procedural justice in the encounter and perceptions of legitimacy of traffic officers and the police more generally. Supplementary analyses suggest that the perceived improvements in police legitimacy were entirely driven by perceived enhancements in procedural justice during the traffic stop encounter. These findings suggest that body camera technology help ensure procedurally just encounters and improve public perceptions of police legitimacy.

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Introduction

Scholars have devoted a considerable amount of attention to enhancing our understanding of the conditions and means through which citizens create, bestow, deny, and rescind police legitimacy (Skogan & Frydl, 2004; Tyler, 2006). Legitimacy represents both public support and the voluntary willingness of citizens to recognize and comply with official authority (Beetham, 1991; Parsons, 1967; Sarat, 1977; Tankebe, Reisig, & Wang, 2016). To be effective in their duties, the police are highly dependent on public support and cooperation (Meares & Kahan, 1998; Skogan & Frydl, 2004). Citizen perceptions of police legitimacy are influenced by a number of factors such as procedural fairness, crime prevention efficacy, and the even distribution of law enforcement

resources (Bottoms & Tankebe, 2012; Jackson & Bradford, 2009; Reisig, Bratton, & Gertz, 2007). Citizens are more likely to support increases in officer discretion, comply with officer directions, and respect officer authority when the police are viewed as trustworthy (Reisig, 2010; Sunshine & Tyler, 2003; Tyler, 2004).

The processes through which the police enforce the law have been found to influence community member views on police legitimacy (Tyler, 2006). Procedurally-just encounters occur when police decision-making is viewed as fair and the police treat citizens with respect and dignity. Mounting scientific evidence suggests that procedurally just encounters improve community member perceptions of their specific encounters with the police and enhance their broader assessments of confidence and trust in police departments (Sunshine & Tyler, 2003; Tyler & Huo, 2002; Tyler & Wakslak, 2004). For instance, a seminal randomized experiment evaluated the impact of procedural justice on police–citizen interactions during drunk driving control operations and found that citizen perceptions of the specific encounter and the police in general improved relative to control conditions in Australia (Mazerolle, Antrobus, Bennett, & Tyler, 2013). However, replications with varying methodological approaches reported divergent results in Scotland (MacQueen & Bradford, 2015) and Turkey (Sahin, Braga, Apel, & Brunson, 2017).

Recent deadly police officer-involved shooting events in Baltimore, Charlotte, Chicago, Ferguson, and elsewhere in the United States have worsened concerning rifts in the relationships between the police and the communities they protect and serve. Outfitting police officers with body worn cameras (BWCs) has been suggested as one potentially powerful response to the current police legitimacy crisis underway in many U.S. cities (President’s Task Force on 21st Century Policing, 2015). Advocates suggest there are many benefits associated with placing BWCs on police officers (Lum, Koper, Merola, Scherer, & Reioux, 2015; Stanley, 2015; White, 2014). For example, BWCs have been suggested to increase transparency and citizen views of police legitimacy, improve police and citizen behaviors during encounters, enhance evidence collected for the resolution of citizen complaints and the arrest and prosecution of offenders, and provide improved opportunities for police training (Ariel, Farrar, & Sutherland, 2015; White, 2014). Despite claims of increased transparency and trust (Stanley, 2015), scant rigorous research exists that explicitly tests whether BWCs influence citizen perceptions of procedural justice and police legitimacy in their interactions with police officers (Lum et al., 2015; White, 2014; for an exception, see White, Todak, & Gaub, 2017).

In this study, we use a rigorous controlled quasi-experimental design to determine whether placing BWCs on officers during traffic stop encounters impacts pulled-over motorists’ perceptions of procedural justice in the Eskisehir Province of Turkey. Similar to police departments in the United States, the Turkish National Police (TNP) faced a legitimacy crisis driven by concerns over police officer behaviors.

The paper begins by providing a general overview of the police legitimacy and procedural justice literature. Theoretical perspectives on the mechanisms through which BWCs are suggested to impact officer and citizen behavior during encounters and the available empirical evidence on BWC impacts are presented. We then describe how the presence of BWCs is hypothesized to improve citizen views of police legitimacy

and procedural justice during encounters with the police. Subsequent sections present the design and implementation of the Eskisehir controlled experiment, analytical approach, and study results. The conclusion considers the theoretical and policy implications of the experimental results. While the study is not well positioned to unravel the specific theoretical mechanisms that may have influenced perceived improvements in procedural justice during traffic stops, our findings do offer strong support for the placement of BWCs on officers as one approach to improved public perceptions of procedural justice and some aspects of police legitimacy more broadly.

Literature review

Police legitimacy and procedural justice

The effective delivery of police services is far more challenging in the absence of strong public support. Legitimacy is conferred on legal authorities when citizens view these institutions and their individual actors as appropriately applying rules to govern public conduct (Sunshine & Tyler, 2003; Tyler & Huo, 2002). Studies have found that citizens are responsive to the ways in which police authority is exercised and public views of the police are powerfully shaped by police behavior (Tyler, 1990). Therefore, police legitimacy has been suggested to be centrally influenced by the procedural justice of police actions. Racially-biased policing practices and other unfair policing policies diminish citizen perceptions of police legitimacy (Fagan, 2002; Tyler & Wakslak, 2004) and, consequently, undermine policing in communities. Whites and minorities in the United States pay close attention to how the police exercise their authority when appraising police legitimacy during their specific encounters with officers (Tyler & Huo, 2002) and police departments more generally (Sunshine & Tyler, 2003). Studies in other countries report similar findings (e.g. Jonathan-Zamir & Weisburd, 2013; Tankebe, 2009, 2013). In essence, procedurally-just encounters can enhance community member cooperation and compliance; in turn, this improves the efficacy of police work in communities.

Research on police–citizen encounters suggests that citizen perceptions of the police are governed by their post-encounter perceptions of fairness. Four key elements of fairness in police–citizen encounters were identified by the National Research Council’s Committee to Review Police Policy and Practices (Skogan & Frydl, 2004). First, citizens should be allowed to have meaningful participation during encounters through opportunities to communicate and explain their actions to officers. Second, officers need to convey their neutrality to citizens by using objective indicators rather than personal views when assessing situations. Third, officers need to treat citizens with respect and dignity during encounters. Fourth, citizens need to feel that they trust the police officers involved in their interventions. Police establish trustworthiness by describing their decision-making processes and explaining to citizens how this shaped their conduct during encounters. Citizens view police procedures as fairer when they believe that the police care about their needs, concerns, and general wellbeing.

The process-based model of police legitimacy suggests that citizens’ assessments of the police are directly and measurably influenced by the way police treat citizens

(Tyler, 2003). Police are viewed as legitimate authorities when citizens perceive that the police treat citizens with respect and make their decisions to use authority fairly. When this occurs, citizens are more likely to cooperate with police instructions and comply with the law. As such, there are two associated elements used by citizens to determine procedural justice in police–citizen interactions: quality of decision-making (e.g. officer decisions were based on objective indicators) and quality of treatment (e.g. citizens were treated with dignity and respect) (Tyler, 2003; Reisig et al., 2007). The key components of the process-based model of police legitimacy have been supported by a number of nonexperimental studies (see e.g. Mastrofski, Snipes, & Supina, 1996; Paternoster et al., 1997; Sunshine & Tyler, 2003; Tyler & Fagan, 2008; Tyler & Wakslak, 2004).

Nagin and Telep (2017), however, suggest that the available empirical evidence is not strong enough to warrant causal inferences between procedural justice and citizen compliance with the law (see also Worden & McLean, 2017). The findings of research based on panel surveys show that, when prior attitudes are controlled, the impact of any one contact on attitudes towards the police are small (Brandl et al., 1994; Rosenbaum et al., 2005). Further, a recent study by Worden and McLean (2014) found weak associations between officers' actions and citizens' procedural justice assessments.

Experimental evidence

There are very few controlled experimental evaluations of interventions designed to enhance police legitimacy. A well-known Campbell Collaboration systematic review of police legitimacy programs supports this observation: “there is a clear lack of randomized experiments in the international research literature that specifically seek to isolate and test the component parts of a legitimacy policing intervention” (Mazerolle et al., 2013, p. 10). Nonexperimental evaluations consistently suggest that the police are viewed as legitimate authorities when they exercise their authority in a procedurally-just manner (Engel, 2005; Elliott et al., 2011; Gau & Brunson, 2010; Murphy et al., 2008; Reisig, et al., 2007). Unfortunately, citizen perceptions of the police are not directly linked to specific encounters with police officers in these observational studies (Mazerolle et al., 2013). Survey research studies infer long-run generalized views of legitimacy rather than conducting field tests that directly analyze the relationship between citizen appraisals of general and specific police legitimacy after actual police–citizen interactions (e.g. see Dai et al., 2011). As such, these studies are not able to adjust for confounding factors such as the role of the media, peer influences, and historical views of the police when making assessments of citizen perceptions of police legitimacy.

There are three randomized controlled trials of procedural justice policing that produce divergent findings on the impact of procedural justice on citizen perceptions of the police. These divergent findings may be influenced by variations in experimental design, officer script dialogues, and citizen response rates across the three studies. The seminal Queensland Community Engagement Trial (QCET) in Australia was the first randomized field experiment to test the direct and indirect outcomes of procedural justice policing (Mazerolle et al., 2013). The four key elements of procedural justice

(citizen participation, dignity and respect, neutrality, and trustworthy motives) were included in a script delivered as the treatment condition by police to drivers during police-initiated random breath testing traffic roadblocks (Mazerolle et al., 2013, p. 35). Routine random breath testing traffic operations served as the control condition. Survey responses mailed to drivers after the encounter were used to collect outcome measures. Procedurally-just encounters during the treatment roadblocks were found to directly improve citizen perceptions of the actual encounter and indirectly improve general views of the police relative to “business as usual” encounters during the control condition.

The Scotland Community Engagement Trial (ScotCET) was designed as a replication of the influential QCET study (MacQueen & Bradford, 2015). The evaluation team reported that procedurally-just police vehicle stops decreased citizen trust in police officers and reduced satisfaction with police conduct relative to comparison routine police vehicle stops. Further, procedurally-just vehicle stop encounters were not found to produce significant impacts on police legitimacy or improvement in general trust of the police relative to routine vehicle stop encounters. MacQueen and Bradford (2015) speculated that the impact of the procedural justice intervention may have been diminished by contextual factors such as generally favorable citizen opinions of the Scotland police when the experiment commenced.

The Adana, Turkey, randomized field experiment evaluated the effects of a procedural justice intervention during traffic stops for excessive speeding relative to business-as-usual police behavior during traffic stop for excessive speeding (Sahin et al., 2017). During this experiment, speed violators stopped by the police were randomly assigned to treatment and comparison conditions. Drivers in the control group experienced routine police behaviors during the traffic stop while drivers in the treatment group received the procedural justice treatment during the traffic stop. The procedural justice treatment did improve citizen perceptions of the police during the specific encounters when compared to the control conditions. Unfortunately, unlike the QCET experience, the procedural justice intervention did not produce noteworthy positive impacts on citizens’ general perceptions of the police.

Body worn cameras, police legitimacy, and procedural justice

Ariel et al. (2017b) identified two theories that support outfitting police officers with BWCs as a potentially important intervention to improve the civility of police–citizen encounters: deterrence (i.e. preventing inappropriate or illegal actions through the threat of video capture of such undesirable behaviors) and public self-awareness (i.e. video capture stimulating desirable behaviors such as treating others in a respectful manner). Deterrence posits that crimes can be prevented when offenders perceive the costs of committing illegal acts to exceed the benefits of such acts (Zimring & Hawkins, 1973; Gibbs, 1975). The available scientific evidence on the crime control efficacy of deterrence generally considers the impacts of modifying detection certainty, sanction swiftness, and punishment severity associated with the commission of specific crimes on the incidence of those crimes (e.g. Apel & Nagin, 2011; Blumstein et al., 1978; Cook, 1980; Paternoster, 1987). In his synthesis of the evidence, Nagin (2013)

suggests that offender perceptions of sanction risk and certainty ultimately determines the existence of deterrent effects.

The placement of BWCs on police officers has been argued to serve as a deterrent to undesirable behaviors during police–citizen interactions (Ariel, Farrar, & Sutherland, 2015). The introduction of BWCs to these encounters is suggested to influence the sanction risk perceptions of officers and citizens alike (Ariel et al., 2017a). Tilley (1993, p. 5) suggests that deterrence can be triggered by the presence of the cameras in social settings, arguing that a camera “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.” The threat that illegal and undesirable behaviors will be captured on video stimulates a deterrent effect by modifying officers’ and citizens’ perceptions that detection, apprehension, and punishment are more likely to occur.

Public self-awareness theory suggests that individuals evaluate and compare their immediate behavior to established internal social norms when they focus attention on themselves (Duval & Wicklund, 1972). Further, when people are being observed, they are more likely to change their behavior, follow social standards, comply with rules, and behave in more socially acceptable ways. When people are made self-aware and believe that they will be negatively affected by not complying with rules and standards, they are more likely to change their behavior to align with established norms (Duval & Wicklund, 1972). There are a variety of environmental cues, such as mirrors, audiences, and video and audio recording devices, that stimulate self-awareness (Silvia & Duval, 2001). Research suggests that, when individuals know that they are being observed, they are more likely to modify their behavior (e.g. Chartrand & Bargh, 1999; Munger & Harris, 1989; Paulhus, 1984).

BWCs stimulate officer and citizen self-awareness during interactions by making it clear that their behaviors are being recorded (Farrar & Ariel, 2013). BWCs may influence participants in police–citizen encounters to compare their behavior to social standards and norms; in turn, this introspection could stimulate socially-desirable behaviors during these interactions (Ariel, 2016b, 2017). Self-aware officers and citizens are more likely to detect differences between normative behavior and their actions and make the necessary adjustments during these encounters (Ariel, 2016a). As we describe later in this article, the procedurally-just treatment of citizens could be among the socially desirable behaviors stimulated by placing BWCs on police officers.¹ In closing, public self-awareness and deterrence theories suggest that BWCs may generate both intrinsic and extrinsic effects, respectively, on the social behavior of individuals who are being observed. Indeed, there is strong theoretical support for the argument that the placement of BWCs on police officers may improve the civility of police–citizen encounters.

A series of randomized field experiments and quasi-experiments suggest that BWCs reduce citizen complaints against officers and both excessive and non-excessive police

¹While officers obviously know whether they are wearing cameras, citizens need to be notified that the camera is present during the encounter in order to generate the hypothesized civilizing effect. Even with notifications, traumatized and/or intoxicated citizens may not internalize the message that the encounter is being recorded and make a rational appraisal of the costs and benefits of their behavior in the presence of the BWC. See White et al. (2017) for discussion of the influence of camera knowledge and rationality on the generation of a civilizing effect.

use of force. However, it remains unclear whether self-awareness, deterrence, or both theoretical perspectives generate the observed improvements in the civility of police–citizen encounters. The Rialto (CA) randomized controlled trial found that outfitting officers with BWCs during treatment shifts generated an 88% reduction in citizen complaints and a 59% reduction in officer use of force reports relative to comparison non-BWC shifts (Ariel, Farrar, & Sutherland, 2015). In Mesa (AZ), a quasi-experimental evaluation reported that BWCs were associated with a 40% decline in citizen complaints for officer misconduct and a 75% reduction in use of force complaints against treatment officers relative to their control counterparts (Mesa Police Department, 2013). The Orlando (FL) randomized controlled trial found that BWC treatment officers significantly reduced the incidence of response-to-resistance reports (incidents involving chemical agents, electronic control devices, impact weapons, and other non-lethal implements) and lowered incidence of serious external complaints when compared to non-BWC control officers (Jennings, Lynch, & Fridell, 2015). Lastly, a quasi-experimental test of BWCs revealed a 62% reduction in complaints made against treatment officers relative to control officers in Phoenix (AZ) (Hedberg et al., 2017).

Some evaluations, however, do not find that BWCs have meaningful impacts on undesirable behaviors during police–citizen encounters. For example, a randomized controlled trial did not find any statistically significant differences in complaints against London Metropolitan Police Service (UK) treatment officers who wore BWCs relative to complaints against control officers who did not wear BWCs (Grossmith et al., 2015). The evaluators acknowledged that the null effects may have been influenced by a treatment fidelity breakdown generated by a lack of BWCs available to the treatment officers during the study and the attrition of officers in the treatment group who were properly trained in the use of BWC. What is more, the camera activation policy provided great discretion to the treatment officers on when the BWCs were used during police–citizen interactions; this may have further undermined the treatment dosage in the experiment (Grossmith et al., 2015; Sutherland & Ariel, 2016). The modest numbers of officers in the treatment and control groups also raised concerns about the statistical power of the experimental design to detect treatment effects.

A multisite randomized experiment involving 2,122 officers in eight police departments reported no overall reduction in officer use of force and an increase in assaults on officers wearing BWCs during treatment shifts relative to officers not wearing BWCs during control shifts (Ariel et al., 2016a). In a re-analysis of the multisite randomized experiment data, Ariel et al. (2016b) show that use of force by treatment officers decreased by 37% in three sites with high compliance to a BWC policy that required officers to notify citizens that they were being recorded at the beginning of the encounter. Ariel et al. (2016a) also reported a 71% increase in officer use of force in sites with low compliance to the BWC policy. Based on these findings, the authors hypothesized that unchecked BWC discretion may increase use of force as camera activation during situations with escalating aggression may further increase aggression during these volatile situations (see also Ariel et al., 2017b). The authors also suggested that verbal notification of video recording by officers at the commencement of encounters may be helpful in deterring aggressive behavior and stimulating civil behavior before police–citizen interactions escalate in a negative direction. Finally, the

results of a randomized controlled trial involving 2,224 Metropolitan Police Department (MPD) officers in Washington, DC, found that the placement of BWCs on officers did not generate statistically significant reductions on use of force incidents and citizen complaints; however, these null effects may have been generated by extensive contamination of control conditions by the treatment officers (Yokum, Ravishanakar, & Coppock, 2017).

Hypothesized benefits of BWCs on police legitimacy and procedural justice

One perceived benefit of outfitting police officers with BWCs is increased transparency and improved citizen views of police legitimacy (President's Task Force on 21st Century Policing, 2015). Unfortunately, this claim has not been sufficiently tested (however, see Ariel 2016a; Culhane, Boman & Schweitzer, 2016). Drawing on deterrence theory, BWCs are assumed to increase the risk of suspension or dismissal for officers who do not comply with the law, procedures, and rules of conduct relative to officers not wearing cameras. Public self-awareness theories suggest that officers equipped with BWC become self-conscious of their behaviors and this increased awareness makes them fairer, more respectful and more courteous relative to officers without BWCs. As pointed out by Ariel (2016b), these behavioral changes for BWC officers represent key components of Tyler's (2003) process-based model of police legitimacy: public voice, neutrality in decision-making, trustworthiness, and treatment with dignity and respect. Citizens are suggested to respond to BWC officers' behavioral changes by viewing them as more legitimate and, in turn, become more willing to cooperate with the police.

Police legitimacy is substantially more complex than ensuring procedurally-just encounters with citizens (Bottoms & Tankebe, 2012; Tankebe, 2009) and the placement of BWCs on police officers may influence other dimensions of police legitimacy. The video recording of police-citizen encounters could enhance citizen perceptions of transparency and accountability in the day-to-day work of the police that serve them. Lawfulness certainly matters to citizens when they appraise police (Tankebe, 2013); putting BWCs on officers may increase their adherence to the rule of law when stopping, searching, frisking, and/or arresting citizens. BWC officers also might be less likely to allow extra-legal factors, such as race of suspected offenders, to influence their decision-making thereby improving distributive justice (Reisig et al., 2007). Finally, citizen perceptions of police effectiveness have been found to be related to assessments of police legitimacy (Bottoms & Tankebe, 2012; Tankebe, 2009). BWCs on officers could generate a deterrent effect on crime by increasing offenders' perceived risk of apprehension and punishment by capturing critical evidence on video that could be used against them in court proceedings—however this hypothesis was somewhat debunked (Ariel, 2016a).

Scant empirical evidence exists to determine whether placing BWCs on officers improves procedural justice in police-citizen encounters or influences these other key dimensions of police legitimacy. In an experiment examining the influence of BWCs on crime reporting in Denver (CO), Ariel (2016a) reported that BWCs led to greater willingness to report crimes to the police in low crime density level residential street segments, but no discernable crime reporting differences emerged in hot spot street

segments. Based on these findings, Ariel (2016a) concluded that the rise in crime reporting in some Denver street segments should be viewed as a sign of improved police–community relations, potentially as a result of changes of enhanced police accountability, legitimacy, and/or perceived effectiveness. Unfortunately, no citizens were interviewed or surveyed in his study.

A representative survey of Isle of Wight (UK) residents before and after the placement of BWCs on officers found that the public had high levels of confidence that the cameras would improve policing (Ellis, Jenkins, & Smith, 2015). However, the survey did not attempt to measure whether the launch of the BWC program influenced citizen perceptions of police legitimacy or, for those residents who did have encounters with BWC officers, whether they felt the cameras influenced the procedural fairness of the encounter. Other research indirectly questions that purported legitimacy-enhancing impacts of BWCs (Gaub et al., 2016; Jennings et al., 2015). For instance, in Phoenix (AZ), Katz et al. (2014) found that officers were disappointed with the impact of BWCs on the public's behavior, with their perceptions changing slightly toward being less positive over time. A recent Spokane (WA) telephone interview study of 249 citizens who had a recent encounter with a police officer found that, when citizens were aware of the presence of the BWC (only 28% were aware of the presence of the BWC), their perceptions of procedural justice in the encounter improved (White, Todak, & Gaub, 2017). However, these links are tenuous, given the lack of controlled settings to estimate these causal impacts. Rigorous empirical evidence explicitly designed to evaluate the influence of BWCs on citizen perceptions of procedural justice and police legitimacy is clearly needed.

Research design and methods

A two-group, post-test-only quasi-experimental design (Campbell & Stanley, 1966; Shadish, Cook, & Campbell, 2002) was used to examine whether citizen perceptions of procedural justice and police legitimacy were impacted by the presence of BWCs in traffic stop encounters in the Eskisehir province of Turkey. As will be described further below, treatment and control conditions were delivered by randomly-selected teams of officers after a “random start” to a balanced rotation of traffic control operation times and locations. The units of analysis were non-randomized traffic stop encounters during treatment and control operations. Citizen perception outcome measures were collected via survey instruments administered by research staff after traffic stops were completed. While non-randomized designs are suggested to have lower internal validity, well-executed quasi-experiments with balanced treatment and control groups can produce results that are of (asymptotically) similar quality to randomized controlled trials (Lipsey & Wilson, 1993). In general, the persuasiveness of causal inferences from quasi-experimental evaluations should be judged on a case-by-case basis (Weisburd et al., 2001).

Research setting

Police services are provided to Turkish citizens through the TNP. This organization has roughly 250,000 officers staffing some 834 Police Stations, 884 Security Directorates of

Towns affiliated with provinces, and 81 city police departments throughout Turkey as well as an assortment of centralized divisions covering counterterrorism, organized crime, and intelligence functions. A nationally-representative survey conducted in 2008 rated the citizen trust in the TNP a 6.4 on 0–10 scale (0 indicating “not at all trustworthy and 10 indicating “highly trustworthy”) with only the military (8.3) and the primary school system scoring higher (6.5) (Adaman, Carkoglu, & Senatarlar, 2009). However, the generally strong global trust that citizens have in the TNP may not apply equally across its varied divisions. Cao and Burton (2006) note that Turkish citizens may not extend this relatively high level of trust to patrol units and traffic divisions responsible for day-to-day police work; rather, these elevated levels of public trust may largely be generated by relatively-successful specialized units that investigate serious violent crimes, organized crime syndicates, and terrorist networks.

Traffic officers are among the most visible members of the TNP as a result of their prominence on the roads and their determined efforts to reduce motor vehicle accidents (Aytac, 2005; Kazu, 2003; Ozbaran, 2010). Unfortunately, survey evidence suggests that Turkish residents hold more negative attitudes toward and have diminished trust in TNP traffic officers relative to their non-traffic counterparts (Adaman, Carkoglu, & Senatarlar, 2005; Aytac, 2005). Rude behavior by traffic officers, alleged unfair practices shielding “privileged” citizens from traffic enforcement actions, and reports of corrupt traffic officers requesting bribes from motorists generated negative impacts on public perceptions of the traffic police (Yalcinkaya, 2012). In 2012, the TNP launched a pilot program to address these problematic perceptions of their traffic police officers. Pilot program was intended to shed insights on the technical, operational, and policy requirements needed to place BWCs on traffic officers. In 2014, the current study was launched to help inform the large scale adoption of BWCs by traffic officers as part of the nationwide “Transparency and Safety in Traffic” initiative (Adana Emniyet Mudurlugu, 2014).

Traffic stops account for the most frequent police–citizen encounters (Hoover et al., 1998) and, for some citizens, traffic stops may be their only direct interactions with police officers (Woodhull, 1994). In the United States, it has been estimated that police make over one hundred million traffic stops per year (Lichtenberg, 2002) and traffic stops represent roughly 60% of all public contact with the police (Eith & Durose, 2011). Unfortunately, systematic national data on the number of traffic stops made by Turkish police officers does not exist. However, relative to the number of tickets issued, it is estimated that there are two to three times the number of traffic stops (Bayley, 1994; Brown, 1981). Between 2012 and 2015, police averaged ten million traffic tickets issued to motorists per year in Turkey (Trafik Hizmetleri Baskanligi, 2016a, 2016b). As such, we estimate that, on average, between 20 and 30 million traffic-related stops are conducted annually in Turkey.

The Eskisehir Police Department (EPD) is comprised of roughly 3,000 police officers and serves a population of 685,727 residents in Turkey’s Eskisehir Province (TURKSAT, 2014). The EPD Regional Traffic Enforcement Unit (RTEU) is responsible for enforcing traffic law on 139 miles (223 km) of highways in this province (Eskisehir Emniyet Mudurlugu, n.d.). In 2014, the RTEU was staffed by 84 traffic police officers who patrolled in 16 officially-marked vehicles. RTEU officers work “8/24” shifts—i.e. eight hours on-duty followed by 24 hours off-duty. For each eight-hour shift, three teams

consisting of two RTEU officers each are responsible for carrying out traffic enforcement on Eskisehir Province highways. The Eskisehir Police did not participate in the TNP BWC pilot study.

Controlled quasi-experimental design

A controlled quasi-experimental design was used to test the impact of BWCs² on citizen perceptions of procedural justice and police legitimacy during traffic stop encounters with RTEU officers during traffic control operations over the 22-day period from November 20, 2014, to December 11, 2014. The units of analysis—police–citizen encounters during traffic stops—were not themselves randomly assigned to treatment and control groups. As such, the resulting study design was not a formal randomized controlled trial. We attempted to create balanced treatment and control groups of police–citizen encounters during traffic stops by ensuring through careful design that there were no systematic differences in the officers staffing the traffic control operations, the locations of the traffic control operations, and the days and shifts of the traffic control operations. As will be detailed below, randomization was used to select two specific locations to conduct speed control operations, two teams of RTEU officers to staff the administration of treatment and control conditions, and the location and timing of the first BWC treatment shift in a schedule that systematically rotated the BWC treatment across areas, shifts, and RTEU officer teams.

All experimental traffic control operations were conducted in two-hour blocks either between 9:30 AM and 11:30 AM or between 12:30 PM and 2:30 PM and were located in either one of two distinct areas. There were eight speed control checkpoint locations in Eskisehir. The research team randomly selected two locations to serve as the specific sites where treatment and control conditions were administered (A-8 representing an 8 kilometer section of highway enroute to Ankara; and B-3 representing a three kilometer section of highway enroute to Bursa). Three teams, each comprised of two RTEU officers, staff traffic control operations in the Eskisehir Province.³ The research team randomly selected two of these teams to participate in the evaluation. We then randomly selected one team of officers (team 4543) to first deliver the treatment conditions during their scheduled shift (i.e. wearing the cameras during the two-hour speed control operation) while the other (team 4545) was assigned to the control condition during their scheduled shift (i.e. not wearing the cameras during the two-hour speed control operation). The timing and location of the first delivery of the BWC treatment were then randomly selected.

²The type of BWC used in this study is model PD77G from Shenzhen AEE Technology. This BWC is designed to work under any weather conditions and features a 12 mega-pixel camera (Adana Emniyet Mudurlugu, 2014). Four BWCs were used in the study. Two cameras were provided to each police officer in the two-person teams administering the treatment. The second cameras served as backups to address any battery charge problems or other unexpected technological issues.

³It is worth noting here that 58 traffic officers staff the three teams responsible for 24-hour, 7-days per week traffic control coverage in Eskisehir Province. A total of 31 traffic officers (53.4% of 58) participated in the experiment. There were no noteworthy differences between participants and non-participants in terms of gender, age, marital status, education level, birth province, rank, total years of service, years of service in traffic, and years of service in the Eskisehir Province (see *reference removed for blind peer review*).

Following this “random start,” the two teams transitioned from treatment to control conditions depending on day and shift. The timing and location of the treatment were systematically rotated over the duration of the controlled study. This was necessary to achieve balance in the times, locations, and RTEU officer teams for the delivery of the BWC treatment conditions. All drivers stopped by the traffic police officers during a given shift received the same interaction; for instance, all drivers stopped by Group 4545 on Monday between 9:30 AM and 11:30 AM at location A-8 received a traffic stop encounter with a BWC officer. All interviewed drivers were stopped only once during the implementation of treatment and control conditions; as such, there were no repeatedly-stopped drivers included in the study.

The diffusion of treatment into the control group can threaten the internal validity of any controlled experiment (Campbell & Stanley, 1966; Shadish, Cook, & Campbell, 2002). In essence, counterfactual contrasts between treated and untreated subjects are weakened by contaminated control conditions. Rubin (1987) developed the stable unit treatment value assumption (SUTVA) which observes that the effect of a program on a particular subject should not be related to the assignment of treatment to other subjects (or observational units). Ariel and et al. (2015) noted possible diffusion of treatment into the control group via the randomization of BWCs by shift rather than by individual officer in the landmark Rialto (CA) BWC randomized controlled trial. In the Rialto study, the same officers wore the cameras during the treatment shifts and during the control shifts. This made it possible that the control shifts experienced a diffusion of the treatment effect. Ariel et al. (2015) reported that the BWC treatment generated statistically-significant reductions in citizen complaints and use-of-force incidents during treatment shifts as compared to control shifts. However, citizen complaints and use-of-force incidents were also reduced during control shifts. These control group reductions suggest a diffusion of treatment effect was present and this contamination of control group conditions may have undermined the establishment of the true treatment effect in the Rialto randomized experiment.

In the Eskisehir RTEU BWC study, the unit of analysis was the encounter as experienced through the view of the driver in the traffic stop. The delivery of treatment and control conditions at different times and at different locations minimized the possibility that the BWC treatment would somehow diffuse into the encounters experienced by drivers in the comparison group. Survey respondents in treatment and control conditions did not have the opportunity to interact with each other and the officers in the encounters either wore BWCs or did not wear BWCs. As described above, the 31 participating officers were not instructed to behave in a procedurally-just manner and did not know the content of the survey. The presence or absence of the BWC was the only difference between treatment and control encounters. This study also controlled for any potential confounding influences of specific RTEU officers in the delivery of the BWC treatment by design. Potential confounding officer-level factors, such as officer personality, experience, or personal attributes, were addressed through the participation of the same officers in the delivery of the treatment and control conditions.

Implementation

All procedures described in this section were subjected to a two-day pilot study conducted prior to the full implementation of the controlled experiment. This pilot was necessary to test the feasibility of the research, identify any issues that could possibly impact the integrity of treatment and control conditions, and make adjustments to the administration of the survey instrument to motorists. The only substantive problem noted was the negative impacts of rainy weather conditions on motorist survey participation. Fortunately, clear weather conditions were experienced during the actual implementation of the study. Prior to implementation, the 31 participating officers were trained on BWC equipment operations and policy as well as on the particulars of the evaluation design. It is important to note here that study officers were not explicitly trained in procedural justice principles or the application of the process-based model to police–citizen encounters during these sessions. The officers were also not informed about the specific purpose of the study. However, all RTEU officers had prior training that touched on the procedural justice framework and were generally familiar with the process-based model.

During treatment and control time periods, RTEU officers monitored drivers and their behaviors while on patrol in the selected traffic control areas. All RTEU officers patrolled as two-officer teams in the same marked patrol vehicle, and pulled over motorists for a variety of reasons ranging from subjective discretionary stops to check driver documents to objective violations of driving regulations and laws such as not wearing required safety belts and exceeding the posted speed limit. Treatment and control RTEU officers directed motorists to stop in a 200-foot-long traffic control operation area marked by orange cones, located on the side of the road. Officers were instructed to handle these encounters as “business-as-usual” traffic stop interactions for motorists in the control group. In business-as-usual traffic stops, one RTEU officer approaches the stopped vehicle, asks for the motorists for their driving documents (license and vehicle registration), and visually inspects the car; the other RTEU officer stays in the patrol car to create a record of the traffic stop, checks driving documents in the computer system, radio-calls the stop to a dispatcher, and writes a traffic violation a ticket, if necessary. During the application of treatment conditions, officers wearing BWCs specifically pointed out the device to drivers and notified them that the encounter was being recorded. To ensure compliance with the delivery of the treatment, supervisors reviewed the videos acquired by the BWC officers and compared the number and duration of videos to stop and self-initiated call records. No violations of treatment conditions were noted over the course of the experiment.

After the RTEU officers completed the treatment and control traffic stops, the drivers drove their vehicles away from the spot of the specific encounters. A researcher then stopped the drivers at a nearby location in the traffic control operations area that was at least 50 feet away from the traffic stop encounter. The researcher asked vehicle operators whether they were willing to participate in a brief survey. If the driver was willing to participate, the survey instrument was administered at this distal location to ensure that motorist responses could not be seen or heard by RTEU officers. The survey instrument collected measures of procedural justice, police legitimacy,

and demographic variables through questions used in previous studies (Mazerolle et al., 2013; Sahin et al., 2017; Tyler, 2004, 2006; Reisig et al., 2012).

The research team hired two undergraduate students to administer the survey in Turkish to stopped motorists during treatment and control traffic control operations. The research team trained the students regarding the study purpose, time schedule, expected sample size, survey questions, how to conduct the survey, how to approach the car, what to say, and possible problems that they might face during the study. Throughout the administration of the survey to the motorists, the student researchers wore picture IDs that displayed their photo, name, and “interviewer” title. Beyond the student surveyors, a senior member of the research team was present during all treatment and control operations to monitor the fidelity of the implementation of the controlled evaluation.⁴

In all, 860 drivers were invited to participate in the post-stop survey, comprising 423 experimental and 437 control subjects. Of these, 73% ($N = 624$) agreed to participate, with participation rates of 71% and 74% in the treatment ($N = 299$) and control ($N = 325$) groups, respectively, which is not a statistically significant difference. The most common reasons given for survey refusal include driving to an appointment (25%) and being late for work (20%), although none of the reasons differed by treatment group. For a two-tailed test with $p = 0.05$, this quasi-experimental evaluation had an estimated statistical power of 0.689 to detect a small standardized effect size of 0.20 and statistical power of 0.999 to detect a medium standardized effect size of 0.50 (Lipsey, 1990).

Analytical approach

All results reported herein are obtained using Stata/MP 14.2. The first step in preparation for the analysis involved confirming the dimensionality of the procedural justice and police legitimacy indicators. Confirmatory factor analyses were performed to extract the three latent variables which serve as the outcomes for the analysis, and the correlation matrices and factor loadings are shown in Appendixes 1 and 2.⁵ The first factor is labeled *procedural justice perceptions*, and includes the seven items (Cronbach’s $\alpha = 0.87$): (1) “Overall, the police officer was polite and treated me with respect during the interaction” (Sahin et al., 2017); (2) “The police officer was fair when making the decision to stop me” (Mazerolle et al., 2013); (3) “Overall, what the police officer did was based on the rules” (Tyler, 2004; Reisig et al., 2012); (4) “I felt the

⁴To make sure that the treatment was delivered with fidelity, two questions were included in the survey: “Were you notified that the encounter would be recorded through BWC?” and “Were you aware that the encounter was recorded through BWC?” The results indicated that only 3 participants in the experimental group reported that they were not notified and aware that the encounter would be recorded through BWC. These 3 participants were not excluded from the analysis. The results also indicated that none of the participants in the control group reported that they were notified or were aware that the encounter would be recorded through BWC. The results showed that the treatment condition was delivered with high fidelity.

⁵In an exploratory principal factor analysis of a polychoric correlation matrix including all indicators, procedural justice, traffic police legitimacy, and general police legitimacy coalesce into unique latent variables. The factor scores used as outcomes in the regression models then derive from (confirmatory) principal component analysis. Note that the principal factor and principal component predictions correlate higher than 0.99. Interestingly, the traffic and general police legitimacy factors do not coalesce into a second-order factor, and are therefore retained as unique outcomes.

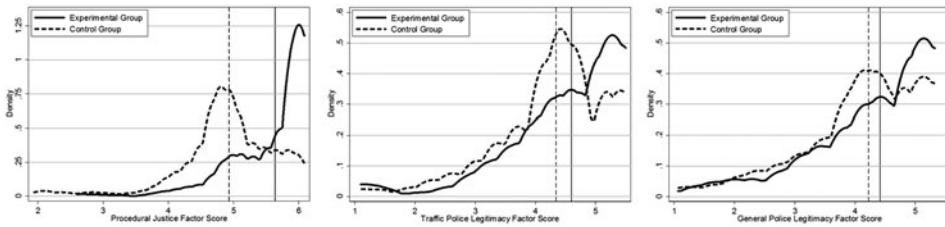


Figure 1. Distribution of procedural justice perceptions, by treatment assignment.

Note: $N=624$. These variables are the principal components extracted from polychoric correlation matrices. The experimental group was administered the body-worn camera intervention, whereas the control group experienced a business-as-usual interaction. The vertical lines identify the mean of each group.

police officer would do the same to anyone in my situation irrespective of his/her status” (Sahin et al., 2017); (5) “The police officer gave me opportunity to express my views during the interaction” (Mazerolle et al., 2013); (6) “The police officer listened to me during the interaction” (Mazerolle et al., 2013); (7) “I believe that what the police did is for my own safety” (Tyler, 2006). The second and third factors derive from three items each, with the factors labeled *traffic police legitimacy perceptions* (Cronbach’s $\alpha=0.86$) and *general police legitimacy perceptions* (Cronbach’s $\alpha=0.89$). The questions were worded the same but referenced either traffic police or police generally: (1) “I have respect for (traffic) police officers” (Tyler, 2006); (2) “I have confidence in (traffic) police officers” (Mazerolle et al., 2013; Tyler, 2006); (3) “I trust (traffic) police officers” (Mazerolle et al., 2013; Sahin et al., 2017).

Each of the foregoing items was measured on a scale from 1 (“strongly disagree”) to 5 (“strongly agree”). Higher values on the factor scores thus indicate more favorable judgments about procedural justice and police legitimacy. Figure 1 provides the distributions of these factor scores, stratified by experimental group assignment. Although the distributions exhibit mild censoring, least squares regression was employed for the main analysis. Supplementary models reported below indicate the results are insensitive to distributional assumptions, so we opted for the interpretability of least squares estimation. Because the factor scores were estimates rather than the true latent variables, the standard errors in all regression models derive from the bootstrap with 500 replications.

In a secondary analysis, a path model was estimated using procedural justice as a mediating variable for the impact of the BWC intervention on police legitimacy. Doing so allows decomposition of the effects of the BWC intervention on police legitimacy into their direct and indirect components. These results are to be interpreted provisionally, because we specify a structural (i.e. “causal”) relationship between procedural justice and police legitimacy despite lacking exogenous variation in the former aside from the BWC intervention. That being said, there are sound theoretical reasons to specify the relationships between officer behavior, perceived procedural justice, and perceived police legitimacy in this manner.

Results

Descriptive statistics for the variables used in the analysis are provided in Table 1. Of the 15 control variables, none differs significantly between the experimental and

Table 1. Descriptive statistics.

Measure	Mean	(SD)	Balance diagnostics		
			Mean diff.	<i>t</i>	<i>d</i>
Assignment variable					
Experimental group	47.9%		–	–	–
Dependent variables					
Procedural justice [§]	5.27	(0.78)	+0.72	13.03	1.04
Traffic police legitimacy	4.46	(1.00)	+0.25	3.19	0.26
General police legitimacy	4.31	(1.05)	+0.19	2.30	0.18
Control variables					
Male	96.2%		+0.01	0.62	0.05
Age [†]	40.76	(11.73)	+0.02	0.82	0.07
Married	78.5%		+0.01	0.24	0.02
Employed	83.2%		+0.00	0.07	0.01
Post-secondary	35.7%		+0.03	0.86	0.07
Monthly income (1,000) ^{††}	2.55	(2.25)	+0.09	1.50	0.12
Born in Eskisehir	27.9%		–0.05	1.50	0.12
City center resident	66.8%		+0.04	1.05	0.08
Police acquaintance	59.0%		+0.02	0.43	0.03
Prior police contact	36.1%		+0.05	1.37	0.11
Prior traffic stop	80.1%		–0.06	1.73	0.14
Prior traffic ticket	35.6%		–0.02	0.40	0.03
Ticketed during stop	7.7%		+0.01	0.60	0.05
A.M. stop time	56.7%		–0.00	0.10	0.01
A-8 stop location	48.4%		+0.01	0.21	0.02

Note: $N=624$. Means of binary variables are shown as percentages. Balance diagnostics are mean differences, an independent-samples t -test, and Cohen's d . A positive mean difference indicates that the experimental group possesses a higher mean than the control group. Control variable imbalance is exhibited by $|t|$ in excess of 1.96 or $|d|$ in excess of 0.20. For categorical control variables, balance is also confirmed from chi-square tests (not shown).

[§]The balance diagnostics are estimated assuming unequal variances.

[†]The untransformed mean is shown, but balance is tested with a log-transformed variable.

^{††}Valid $N=598$. Monthly income is in Turkish currency. \$1 was equal to 2.25 Turkish Lira at the time of the study.

control conditions; indicating that the quasi-experimental design achieved excellent balance. On the other hand, the treatment and control groups do significantly differ in their perceptions of procedural justice and police legitimacy. Specifically, independent-samples t -tests indicate that the groups differ in perceived procedural justice (experimental = 5.6; control = 4.9; $t = 13.03$; unequal variances $df = 606$; $p < 0.0001$), perceived traffic police legitimacy (experimental = 4.6; control = 4.3; $t = 3.19$; equal variances $df = 622$; $p < 0.01$), and perceived general police legitimacy (experimental = 4.4; control = 4.2; $t = 2.30$; equal variances $df = 622$; $p < 0.05$). The effect size estimates are 1.04, 0.26, and 0.18, respectively (see Cohen, 1988).⁶ The effect size for procedural justice is especially notable for its magnitude, indicating that the BWC intervention substantially improves respondents' impressions of the situational behavior of the traffic police officers. On the other hand, while the BWC intervention is significantly

⁶The effect size, Cohen's d , is the standardized difference between means estimated from two independent groups, and is frequently used to accompany the reporting of statistical significance:

$$d = \frac{\bar{y}_E - \bar{y}_C}{\sqrt{(s_E^2 + s_C^2)/2}}$$

where E references the experimental group and C references the control group. Cohen's d is routinely judged against 0.2 (small), 0.5 (medium), and 0.8 (large) with respect to substantive significance (Cohen, 1988). Effect sizes smaller than 0.2 are generally regarded as not worth mentioning, even if the difference in means is statistically significant.

correlated with police legitimacy, the small effect sizes suggest that the difference is not substantively meaningful.

In Table 2, we estimate linear regression models that introduce the control variables. Focusing first on the coefficients for the control variables, the cross-model results indicate that procedural justice and police legitimacy are jointly significantly correlated with income ($p < 0.05$), prior police contact ($p < 0.10$), and prior police ticket ($p < 0.05$).⁷ Concerning the latter two regressors, respondents who have interacted with the police in the past tend to have less favorable perceptions, on average and other things equal. Importantly, the control variables do not alter inference about assignment to the experimental condition. Indeed, the t -tests and effect sizes are barely different from those shown in Table 1. The BWC intervention is the only regressor that is highly jointly statistically significant in all three regression models ($p < 0.0001$). Although not shown, the 95% confidence intervals for the impact of the BWC intervention are 0.60–0.82 for procedural justice, 0.10–0.41 for traffic police legitimacy, and 0.06–0.37 for general police legitimacy.

In Appendix 3, we evaluate the sensitivity of the findings with respect to features of the model specification, and show only the coefficients for the BWC intervention to minimize extraneous output. The first model is a least squares regression in which the BWC intervention is interacted with every other regressor, and the average marginal effect reported. The second set of models comprise robust regression, which is an iterative procedure designed to limit the influence of outliers and high-leverage observations on inference (Andersen, 2008; Verardi & Croux, 2009). The third set of models comprises quantile regression, which estimates conditional quantiles (e.g. the median or 50th percentile, giving rise to least absolute deviations) rather than the conditional mean (Koenker, 2005). The fourth model is censored normal (i.e. tobit) regression, intended to address the mild right-censoring apparent in the distributions of procedural justice and police legitimacy perceptions (refer to Figure 1). The fifth model is censored least absolute deviations, which is robust to heteroscedasticity and non-normality that yield inconsistency in censored normal regression models (Powell, 1984). The sixth model is treatment effect regression (via regression adjustment), which produces regression-based extrapolations of the outcome conditional on experimental group assignment, less the extrapolated outcome conditional on control group assignment, and then averages over these effects (Imbens, 2004; Wooldridge, 2010). The seventh model derives from ordered logistic regression in which the individual indicators of procedural justice and police legitimacy perceptions are treated as outcomes, and then a single estimate is derived using the combination procedures outlined by Rubin (1987; Little & Rubin, 2002). The eighth model is a multilevel, ordered logistic regression in which the procedural justice and police legitimacy indicators are stacked and treated as level-1 measurements nested within respondents. The ninth and final model is a so-called MIMIC (multiple indicators, multiple causes) regression, estimated as a structural equation model where the link between the latent variables and indicators are ordered logit models (Skrondal & Rabe-Hesketh, 2004).

⁷The p -values reported in parentheses derive from seemingly unrelated regression models (see Zellner, 1962), following which a test is performed that the coefficients in all three models are jointly equal to zero.

Table 2. Least-squares regression models of procedural justice and police legitimacy.

Variable	Procedural justice perceptions			Traffic police legitimacy perceptions			General police legitimacy perceptions		
	Coeff.	(SE)		Coeff.	(SE)		Coeff.	(SE)	
Experimental Group	0.71	(0.06)	***	0.26	(0.08)	**	0.21	(0.08)	**
Male	-0.10	(0.12)		-0.01	(0.19)		-0.16	(0.21)	
Age (ln)	0.25	(0.11)	*	0.22	(0.18)		0.06	(0.18)	
Married	-0.00	(0.08)		0.16	(0.12)		0.28	(0.12)	*
Employed	-0.05	(0.08)		-0.08	(0.11)		0.01	(0.12)	
Post-secondary	-0.01	(0.07)		0.07	(0.10)		-0.11	(0.10)	
Monthly income [†]	0.11	(0.05)	*	-0.09	(0.08)		0.01	(0.08)	
Born in Eskisehir	0.06	(0.06)		0.15	(0.09)	+	0.17	(0.09)	+
City center resident	-0.03	(0.06)		-0.04	(0.09)		-0.17	(0.09)	+
Police acquaintance	0.02	(0.06)		-0.01	(0.08)		0.06	(0.08)	
Prior police contact	-0.05	(0.06)		-0.09	(0.09)		-0.24	(0.09)	**
Prior traffic stop	-0.02	(0.08)		-0.10	(0.10)		-0.09	(0.11)	
Prior traffic ticket	-0.17	(0.07)	*	-0.29	(0.10)	**	-0.17	(0.10)	+
Ticketed during stop	-0.10	(0.10)		0.10	(0.14)		-0.10	(0.14)	
A.M. stop time	0.05	(0.07)		-0.02	(0.08)		0.14	(0.09)	
A-8 stop location	-0.00	(0.07)		0.02	(0.09)		0.01	(0.10)	
R ²	0.25			0.09			0.07		

Note: $N = 624$. Robust standard errors are reported. Because the dependent variables are factor scores, the standard errors are obtained via the bootstrap with 500 replications. Included but not shown is a dummy variable that flags respondents with missing income. Sensitivity analysis of the experimental group coefficient is provided in Appendix 3.

[†]Variable is log-transformed.

⁺ $p < 0.10$.

^{*} $p < 0.05$.

^{**} $p < 0.01$.

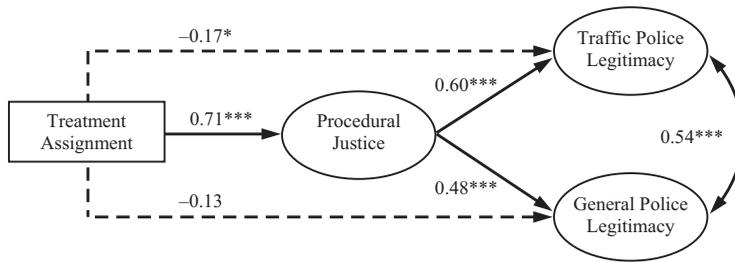
^{***} $p < 0.001$ (two-tailed tests).

In no model is inference perceptibly altered about the impact of the BWC intervention on procedural justice and police legitimacy. The results reported from the least squares regression model in Table 2 are therefore highly robust, and give us confidence that there are no distortions of the findings introduced by model choice.⁸

Path regression model results

Figure 2 provides the key results from a path regression in which procedural justice intermediates between the BWC intervention and police legitimacy. The results from this model are provisional and are to be interpreted with caution, since we lack

⁸One inferential threat stems from the fact that traffic officer identifiers were not available in the database, making it impossible to adjust standard errors for any officer-level clustering stemming from repeated stops. This was partly yet imperfectly remedied by estimation of robust standard errors. Yet it should be noted that any dependence would have to be substantial to inflate the standard errors enough to render results non-significant. For example, in the model for general police legitimacy perceptions in Table 1, the standard error would have to be over 30% larger to reduce the effect to non-significance. In the model for procedural justice perceptions, on the other hand, the standard error would have to be more than five times larger.



Impact of Treatment Assignment on Police Legitimacy

Traffic Police Legitimacy:	General Police Legitimacy:
Direct = -0.17 *	Direct = -0.13
Indirect = 0.43 ***	Indirect = 0.34 ***
Total = 0.26 ***	Total = 0.21 **

Figure 2. Path regression model results.

Note: $N = 624$. Robust standard errors are estimated but not shown. Control variables are also included but not shown. Path coefficients are unstandardized, while the term representing the correspondence between errors in the legitimacy equations is a correlation. Sensitivity analysis of the direct effects of treatment assignment on police legitimacy is provided in Appendix 4, and indicates that its apparent significance for traffic police legitimacy is an anomaly.

+ $p < 0.10$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$ (two-tailed tests).

exogenous variation in procedural justice, aside from experimental group assignment. Nevertheless, the model is specified in a way that harmonizes with theoretical expectations about the relationship between police officer behavior, perceived procedural justice, and perceived police legitimacy.

The first finding from Figure 2 is that the indirect effects of the BWC intervention on police legitimacy are positive and statistically significant, as expected. They are also sizable and at least 60% larger than their total effects. This follows readily from the magnitude of the impact of the BWC intervention on procedural justice, coupled with the magnitude of the relationship between procedural justice and police legitimacy. The indirect effect is simply the product of these two coefficients.⁹

The second finding from Figure 2 is that the direct effects of the BWC intervention on police legitimacy are surprisingly negative and, in one instance, statistically significant. While this seems counterintuitive, it is important to bear in mind that this negative direct effect is swamped by a very large positive indirect effect through procedural justice. Indeed, the net or total effect of the BWC intervention on police legitimacy is positive and statistically significant. It is also the case that the apparent negative direct effect of the BWC intervention on traffic police legitimacy does not withstand close scrutiny to model specification, as revealed in Appendix 4.¹⁰ We are

⁹Consistent with Sobel (1982, 1986), the product-of-coefficients method is used to obtain the standard error of the indirect effect. A more detailed review of methods for estimation and inference in mediation models is provided in MacKinnon et al. (2002).

¹⁰In comparisons of models including the BWC intervention and procedural justice (implying direct and indirect effects), with models including only procedural justice (implying indirect effects only), the Bayesian information criterion favors the indirect-effect-only specification (Schwarz, 1978). This is further confirmed from model averaging, in which the direct effect of BWC is questionable (De Luca & Magnus, 2011; Magnus et al., 2010). In sum, it can probably be ruled out that the BWC intervention has any direct effect on perceived police legitimacy, as the significant direct effect shown in Figure 1 is an anomaly. Instead, its effect is entirely indirect though perceived procedural justice.

consequently inclined to infer that the impact of BWC on perceived police legitimacy is indirect through perceived procedural justice.

Discussion and conclusion

This controlled quasi-experimental evaluation was designed to test whether placing BWCs on police officers during traffic stops impacted citizen views of the specific encounter and of the police more generally in the Eskisehir Province of Turkey. Some 860 police–citizen traffic encounters were allocated to balanced treatment and control groups. Subjects in both groups experienced business-as-usual traffic stops; the only designed difference in treatment group encounters was BWC presence coupled with an announcement at the outset of the traffic stop that the encounter was being recorded. The same officers delivered treatment and control conditions, and similar shares of treatment and control subjects received moving violation citations. Relative to drivers who experienced traffic stops without BWCs, our statistical analyses found that drivers stopped by officers with BWCs reported improved perceptions of procedural justice during the specific encounter and improved perceptions of the legitimacy of RTEU traffic police in particular and the TNP more generally. Exploratory path regression analyses suggest that the perceived improvements in police legitimacy were driven by perceived enhancements in procedural justice during the traffic stop encounter. These findings were robust to a variety of alternative model specifications and sensitivity tests.

Like other social science inquiries, the Eskisehir quasi-experimental evaluation had some limitations. Approaching motorists soon after traffic stops had the benefit of generating higher response rates. However, participant responses could possibly be affected by suspicions that interviewers were actually TNP officers or by the presence of traffic officers in areas proximate to the interview. It is worth noting that if participants were affected by these factors, treatment and control group members would likely be influenced in similar ways and, as such, this possible limitation may not have biased the estimation of the BWC treatment impact. While strong internal validity is a benefit of well-implemented controlled evaluations, experimental designs can have low levels of external validity (Shadish, Cook, & Campbell, 2002). The BWC treatment in this study was applied during a specific kind of police–citizen encounter setting—routine traffic enforcement. Therefore, it is questionable whether the findings of this evaluation can be generalized to other types of police–citizen encounters. Arguably, BWCs may be of limited value in generating civilizing effects during police–citizen encounters involving intoxicated and traumatized citizens (see White et al., 2017).

The generalizability of these findings may also be limited by the unique political, cultural, and religious setting of Eskisehir, an urban environment in the Middle East. It is uncertain whether the findings of this study are indeed transferrable to other contexts. However, police departments in Western countries do face similar legitimacy crises to those described here. TNP traffic officers were viewed by Turkish citizens as unfair in their policing practices, corrupt, and generally untrustworthy and rude during traffic stops. In many US cities, similar views are held by many residents, particularly

by minority citizens who reside in disadvantaged neighborhoods (Carr, Napolitano, & Keating, 2007; Brunson & Miller, 2006; Brunson & Weitzer, 2009). Therefore, the findings of this study may indeed be generalized to law enforcement agencies experiencing legitimacy crises in other settings.

The results of our controlled evaluation support the position that the presence of the cameras during traffic stops seem to enhance citizen perceptions of police legitimacy. The cameras may have stimulated officers to behave in a more procedurally-just manner and influenced the behaviors of pulled-over motorists and facilitated their receptiveness to police instructions and commands. It is also possible that the mere presence of the BWC during the interaction made the motorists feel more positive about the encounter independent of the officers' behaviors (Crow et al., 2017; ODS Consulting, 2011). Unfortunately, we did not ask specific questions to motorists and traffic officers to determine the theoretical mechanisms associated with observed behavioral changes influenced by the introduction of cameras to police-citizen encounters. Future research should attempt to do so.

From the standpoint of improving police-community relations, of course, the theoretical distinction between deterrence and public self-awareness is irrelevant; that is, it only matters whether an intervention "works" by improving individual behavior (for an argument to this effect, see Miles & Ludwig, 2007). However, determining whether deterrence, self-awareness, or some combination of the mechanisms associated with both theoretical perspectives would be important in developing further refinements to BWC deployments and policy. For instance, if officers noted that the initial announcement that the interaction was being recorded stimulated their self-awareness of procedural justice principles, it might be important to make such announcements mandatory for all police-citizen encounters where a BWC is present. Our study is generally supportive of policies requiring the initial announcement by BWC officers that the interaction is being recorded. Further, White et al. (2017) found that citizen awareness of BWC presence enhanced their perceptions of procedural justice during encounters with officers. Future studies should seek to provide stronger evaluation evidence to guide such policy decisions by perhaps randomizing BWC officers to a treatment group that make an initial announcement that the encounter is being recorded and to a control group that makes no initial announcement.

Some police executives suggest that the presence of BWCs may make it less likely that citizens submit false reports of poor treatment at the hands of the police (Police Executive Research Forum, 2014). The potential false reporting of problematic police behaviors was not examined in this study. With that caveat in mind, it is noteworthy that traffic stops involving officers with BWCs did not generate a single complaint while traffic stops involving the same officers without BWCs generated six complaints during the study time period. Coupled with our results suggesting that citizens do feel that police officers behave in a procedural just manner when outfitted with BWCs, this suggests that the cameras may indeed be generating a "civilizing" effect on police-citizen encounters rather than simply discouraging citizens from filing false reports. This conclusion is speculative, however, and requires further analysis of new data confirming or denying the veracity of the six complaints made by citizens during the control traffic stops.

The Eskisehir experience suggests that the placement of BWCs on officers could be helpful in ensuring procedurally-just encounters with citizens. This position is supported by mounting scientific evidence that adherence to procedural fairness norms helps criminal justice institutions establish and maintain legitimacy (Tyler, 2003, 2004, 2006). When criminal justice system actors exercise their authority in fair ways, citizen evaluations of the police and other legal institutions are enhanced. Both personal experiences with the police and general views of police activities are shaped by citizen judgments of procedural justice (e.g. Paternoster et al., 1997; Reisig & Chandek, 2001). The findings suggest that outfitting officers with BWCs may stimulate such improvements in the citizen perceptions of procedural fairness during encounters with the police. The placement of BWCs on police officers could be especially helpful in improving poor relations with minority residents. When police officers treat minority citizens in a respectful and polite manner, perceptions of racial profiling are reduced (Tyler & Wakslak, 2004). Moreover, more general feelings of satisfaction with the police are improved when officers make an effort to explain their reasons for enforcement actions to citizens (Skogan & Hartnett, 1997; Tyler & Huo, 2002).

While this study suggests that police legitimacy can be improved through the adoption of BWCs, it is worth noting that BWCs should not be regarded as a panacea for police departments affected by legitimacy problems. Other factors can be quite influential in establishing police legitimacy. For instance, survey research suggests increased citizen cooperation is associated with the fair distribution of police resources across people and communities in the U.S. (Reisig, Bratton, & Gertz, 2007). Police crime control efficacy was the primary factor associated with citizen cooperation in Ghana (Tankebe, 2009). Research in the UK suggests judgements of social cohesion and moral consensus exert a stronger influence on how citizens view local police when compared to perceptions of the risk of victimization (Jackson & Bradford, 2009). When people believe that the police act based on a shared moral purpose with citizens, they are more likely to accept the authority of the police to dictate appropriate behavior and to obey officer instructions (Jackson et al., 2012). Bottoms and Tankebe (2012) argue that the main components of police legitimacy in liberal democracies include lawfulness, effectiveness, procedural fairness, and distributive fairness (see also Beetham, 1991; Coicaud, 2002). Survey research in London suggests these main components were relevant to citizen appraisals of police legitimacy and had both direct and indirect influences on the readiness of citizens to cooperate with police (Tankebe, 2013).

Former President Obama's Task Force on 21st Century Policing (2015) urged law enforcement agencies to test strategic innovations and new technologies designed to build trust and legitimacy between officers and the communities they serve. Procedural justice during police-citizen encounters was highlighted as an important governing principle to improve police-community relations. BWCs were further suggested as a new technology that could improve police practices and build trust and legitimacy. Unfortunately, BWCs have been adopted by many police departments without much scientific evidence available to guide implementation, monitoring, and evaluation (Lum et al., 2015). This study provides some rigorous evidence in support of the Task Force's suggestion that BWCs could be helpful in improving perceptions of

police legitimacy, at least in the context considered here. However, not all BWC evaluations report desirable results with recent experiments, suggesting that policy guiding officer discretion on when to record encounters may be critical to ensuring positive impacts (Ariel et al., 2016a, 2016b). As the existing body of scientific evidence continues to develop, jurisdictions should implement BWCs with eternal vigilance to both intended and unintended consequences. Nevertheless, the results of this controlled evaluation suggest that BWCs could be an important tool to ensure procedurally-just encounters that, in turn, influence public perceptions of police legitimacy.

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Appendix 1.

Polychoric and Pearson's correlation matrices of procedural justice perceptions.

Measure	1	2	3	4	5	6	7
1 "Overall, the police officer was polite and treated me with respect during the interaction"	–	0.68	0.59	0.44	0.42	0.39	0.41
2 "The police officer was fair when making the decision to stop me"	0.78	–	0.73	0.51	0.46	0.44	0.48
3 "Overall, what the police officer did was based on the rules"	0.70	0.82	–	0.56	0.45	0.47	0.44
4 "I felt the police officer would do the same to anyone in my situation irrespective of his/her status"	0.53	0.61	0.69	–	0.48	0.50	0.41
5 "The police officer gave me opportunity to express my views during the interaction"	0.55	0.59	0.58	0.60	–	0.75	0.40
6 "The police officer listened to me during interaction"	0.52	0.58	0.60	0.60	0.84	–	0.41
7 "I believe that what the police did is for my own safety"	0.54	0.60	0.57	0.53	0.56	0.58	–
Factor loading	0.81	0.87	0.87	0.79	0.82	0.82	0.75
Uniqueness	0.35	0.24	0.24	0.37	0.33	0.33	0.43
Mean	4.5	4.5	4.5	4.1	4.2	4.1	4.4
SD	0.7	0.7	0.7	0.9	1.0	1.1	0.9
10th percentile	4	4	4	3	3	3	4
50th percentile	5	5	5	5	4	4	5
90th percentile	5	5	5	5	5	5	5

Note: $N = 624$. Correlations below the diagonal are polychoric correlations, whereas correlations above the diagonal are Pearson correlations. All correlations are statistically significant at $p < 0.0001$ (two-tailed). The factor loadings derive from principal components analysis of the polychoric correlation matrix.

Appendix 2.

Polychoric and Pearson's correlation matrices of police legitimacy perceptions.

Measure	Traffic police			General police		
	1	2	3	1	2	3
1 "I have respect for (traffic) police officers"	–	0.62	0.63	–	0.72	0.70
2 "I have confidence in (traffic) police officers"	0.78	–	0.74	0.60	–	0.80
3 "I trust (traffic) police officers"	0.70	0.82	–	0.60	0.84	–
Factor Loading	0.88	0.92	0.93	0.92	0.95	0.95
Uniqueness	0.23	0.15	0.14	0.15	0.10	0.11
Mean	4.2	3.9	4.0	4.3	3.9	4.0
SD	1.0	1.0	1.1	1.0	1.1	1.1
10th percentile	3	3	3	3	2	2
50th percentile	5	4	4	5	4	4
90th percentile	5	5	5	5	5	5

Note: $N=624$. The measures of traffic police legitimacy perceptions specifically reference the traffic police. Correlations below the diagonal are polychoric correlations, whereas correlations above the diagonal are Pearson correlations. All correlations are statistically significant at $p < 0.0001$ (two-tailed). The factor loadings derive from principal components analysis of the polychoric correlation matrix.

Appendix 3.

Sensitivity of effects of the BWC intervention to specification and model choice.

Statistical model	Procedural justice perceptions			Traffic police legitimacy perceptions			General police legitimacy perceptions		
	Coeff.	(SE)		Coeff.	(SE)		Coeff.	(SE)	
Fully inter-acted regression	0.71	(0.06)	***	0.26	(0.08)	**	0.21	(0.08)	**
Robust regression									
M-estimator	0.74	(0.06)	***	0.20	(0.08)	***	0.23	(0.07)	***
S-estimator	1.13	(0.04)	***	0.73	(0.17)	***	0.48	(0.20)	*
Quantile regression									
25th percentile	0.84	(0.08)	***	0.27	(0.12)	*	0.31	(0.15)	*
50th percentile	0.96	(0.06)	***	0.41	(0.11)	***	0.32	(0.12)	**
75th percentile	0.68	(0.06)	***	0.37	(0.12)	**	–	–	
Censored normal regression	0.95	(0.08)	***	0.41	(0.11)	***	0.37	(0.11)	***
Censored least absolute deviations	0.98	(0.06)	***	0.41	(0.11)	***	0.35	(0.14)	**
Treatment effect regression	0.71	(0.06)	***	0.26	(0.08)	***	0.21	(0.08)	**
Combined ordered logistic regression	1.80	(0.39)	***	0.60	(0.22)	**	0.47	(0.17)	**
Multilevel ordered logistic regression	2.72	(0.20)	***	1.19	(0.28)	***	1.27	(0.33)	***
MIMIC ordered logistic regression	2.90	(0.37)	***	0.91	(0.21)	***	0.98	(0.26)	***

Note: $N=624$. Each model is fully specified as in Table 2, but only the coefficient for experimental group assignment is shown. Robust standard errors are reported. In all models except ordered logistic regression, because the dependent variables are factor scores, the standard errors are obtained via the bootstrap with 500 replications.

⁺ $p < 0.10$.

* $p < 0.05$.

** $p < 0.01$.

*** $p < 0.001$ (two-tailed tests).

Appendix 4.

Sensitivity of direct effects of the BWC intervention, net of procedural justice, on police legitimacy to specification and model choice.

Statistical model	Traffic police legitimacy perceptions			General police legitimacy perceptions	
	Coeff.	(SE)		Coeff.	(SE)
Fully treatment interacted regression	-0.17	(0.09)	+	-0.11	(0.09)
Robust regression					
M-estimator	-0.13	(0.07)	+	-0.09	(0.08)
S-estimator	-0.08	(0.09)		-0.05	(0.12)
Quantile regression					
25th percentile	-0.11	(0.14)		-0.08	(0.16)
50th percentile	-0.12	(0.08)		0.01	(0.09)
75th percentile	-0.04	(0.06)		-0.09	(0.06)
Censored normal regression	-0.17	(0.11)		-0.12	(0.12)
Censored least absolute deviations	-0.10	(0.10)		0.01	(0.12)
Treatment effect regression	-0.17	(0.09)	+	-0.11	(0.09)
Combined ordered logistic regression	-0.16	(0.20)		-0.13	(0.18)
Multilevel ordered logistic regression	-0.39	(0.28)		-0.37	(0.34)
MIMIC ordered logistic regression	-0.27	(0.20)		-0.30	(0.26)

Note: $N = 624$. Each model is fully specified, but only the coefficient for experimental group assignment is shown, indicating its direct effect on police legitimacy net of procedural justice. Robust standard errors are reported. In all models except ordered logistic regression, because the dependent variables are factor scores, the standard errors are obtained via the bootstrap with 500 replications.

+ $p < 0.10$.

* $p < 0.05$.

** $p < 0.01$.

*** $p < 0.001$ (two-tailed tests).