

Recorded Justice or Procedural Justice? A Randomized Controlled Experiment of the Influence of Body Worn Cameras and Officer Behavior on Citizen Attitudes

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Abstract

We implement cluster randomization to test the impact of procedurally just and unjust police behavior during a hypothetical traffic stop (versus procedurally neutral behavior), in addition to the impact of body worn camera notification (versus BWC absence). The findings from post-vignette surveys administered to a random sample of 750 respondents indicate procedural justice has a pronounced effect on citizens' perceptions of the officer's behavior, their attitudes about the encounter, and their general views about police and the law within the context of the vignette. A BWC neither improved nor worsened attitudes, nor did it buffer the impact of procedurally unjust policing or enhance the impact of procedurally just policing in the eyes of citizens. Tests of asymmetry indicate procedurally unjust police behavior results in more negative judgments of the officer's behavior in the vignette than procedurally just behavior results in positive judgments. The results suggest that officer behavior matters much more than BWCs.

Keywords: experimental design, policing, procedural justice, body worn camera, citizen attitudes

Introduction

In response to what may be described as a crisis of police legitimacy, the President’s Task Force on 21st Century Policing (2015) urged the adoption of procedural justice as the “guiding principle” for trust-strengthening interactions with the community (see also National Academic of Sciences, Engineering, and Medicine, 2004). The Task Force further recommended implementation of body worn camera technology “best practices,” among other strategies, to improve oversight and enhance transparency. These recommendations have some empirical precedent, since procedural justice and (to a lesser extent) body worn cameras are two evidence-based policing strategies that have been widely studied and demonstrated to shape citizen attitudes—although not necessarily citizen behavior—with respect to the police in face-to-face encounters. A key ingredient to these interventions seems to be that they change for the better how police officers interact with the public.

Procedural justice and body worn cameras have been largely studied independently of one another, yet they are rooted in related mechanisms thought to be legitimacy-enhancing in encounters between police and the public. Procedural justice prioritizes fair treatment, whereas body worn cameras convey accountable decision-making, among other goals. Whether and how they work in combination to enhance police-citizen encounters has been understudied to date (see McClure et al., 2017; Sivasubramaniam et al., 2021). For example, BWCs could enhance the impact of procedurally just treatment on attitudes when the public believes they contribute to lawful conduct from police. Alternatively, they could buffer the impact of procedurally unjust treatment if the public trusts that such behavior will be sanctioned and corrected by supervisors.

The possibility that body worn cameras could moderate the impact of procedurally just policing, procedurally unjust policing, or both on attitudes also points to the utility of a

procedurally neutral reference point to formalize comparisons (see Johnson et al., 2017; Maguire et al., 2017). A neutral reference offers additional comparisons by testing for (a)symmetry in negative versus positive police encounters (relative to the neutral reference). Prior evidence suggests negative encounters are more consequential for certain policing attitudes than positive encounters. But to date, tests for this kind of asymmetry have been informal, and explanations of it have been ad hoc. We provide a description of the prospect theory of Kahneman and Tversky (1979) and use it to make predictions in a novel setting—police-citizen encounters. These theoretical predictions concern reference dependence, loss aversion, and diminishing sensitivity.

In the current study, we administered vignettes portraying a police-initiated traffic stop for a speed violation to a random sample of 750 community members. The vignettes were transcripts that randomly varied the behavior of the police officer toward the driver (three conditions: procedurally just vs. procedurally unjust vs. procedurally neutral) as well as the announced presence of a body worn camera (two conditions: present vs. absent). Post-vignette surveys measured respondents' perceptions of the police officer's behavior (procedural justice, effectiveness, lawfulness, legitimacy), their attitudes about the nature of the encounter (satisfaction with the encounter, compliance with police), and their generalized views about police and the law (satisfaction with police, police-community relations, cooperation with police, compliance with the law).

Background

Procedural justice and body worn cameras are viewed, in part, as legitimacy-enhancing strategies for law enforcement. As originally conceived by Max Weber (2019), police actions and directives have *legitimacy* when they possess “imperative validity,” most often stemming from the public's value-rational belief in legal authority as “the embodiment of ultimate, obligatory values”

(p. 111), and as underwritten by the coercive force of the state and its representatives. Fundamentally, legitimacy and related citizen attitudes are subjective and informed by personal or vicarious experiences with police. In our review below, we focus on survey research measuring citizen attitudes in the aftermath of police encounters involving body worn cameras and procedural justice, with priority given to research that randomizes some aspect of the encounter. We are especially interested in traffic stops since that is the context of the current study.

Body Worn Cameras and Citizen Attitudes

Body worn cameras (BWCs) serve many functions for law enforcement, but they are partly believed to enhance procedural justice and thus legitimacy. Because they provide an “objective” record of police encounters, BWCs are theorized to constrain the behavior of police officers in ways that channel them toward more restrained and lawful conduct (Ariel et al., 2015; Braga, Sousa, Coldren, and Rodriguez, 2018; Demir et al., 2020a, 2020b). When monitored by police managers, officers can be held internally accountable for their conduct, and moreover, can be targeted for enhanced training in departmental procedures that are violated in recorded encounters (e.g., Voigt et al., 2017).

Despite early indication of promise at reducing police officer use of force and citizen complaints (Ariel et al., 2015, 2016a, 2016b), meta-analytic reviews indicate BWCs do not have a consistent impact on the behavior of police officers or citizens during or following face-to-face encounters (for a recent evaluation, see Williams et al., 2021; see also Lum et al., 2020 and Braga et al., 2022).¹ Even though the jury is still out on the impact of BWCs on the behaviors measured administratively by law enforcement agencies, there is good reason to believe BWCs matter from the viewpoint of the public. Indeed, there is widespread support for outfitting police with BWCs

¹ Even with mixed findings across heterogeneous evaluations, however, BWCs appear to pass a benefit-cost test for their adoption, with a ratio approaching 5:1 (Williams et al., 2021).

to the degree they improve the interactional behavior of police officers (Crow et al., 2017; Demir, 2019; Williams, 2021; see also Demir, 2023b), and as mentioned, the President's Task Force on 21st Century Policing (2015) advocated for more widespread implementation of the technology partly on these grounds.

Compared with evaluations measuring behavioral outcomes, studies of the association between BWCs and citizen attitudes in face-to-face encounters with the police are revealing. McClure et al. (2017) found their survey respondents perceived more procedural justice when they interacted with officers who were randomly assigned to wear BWCs, and this was especially true with BWC officers who were further randomly assigned to administer a script disclosing that each encounter was recorded. One challenge revealed by this study was the difficulty respondents had recalling whether their encounter was recorded by BWC, suggesting that officer behavior was a more salient determinant of citizen attitudes than the mere presence of a BWC (see also McCluskey et al., 2019). The Police Executive Research Forum (2017) also randomly assigned police officers to BWCs, but did not detect differences in reports of professionalism, legitimacy, or satisfaction, whereas White et al. (2017) observed a difference in procedural justice perceptions linked to respondent awareness that the officer with whom they interacted had a BWC. Most recently, Braga et al. (2023) conducted a cluster randomized controlled trial in New York City and found no difference between BWC precincts and non-BWC precincts in citizen perceptions of procedural justice.

Of special interest are the studies tailored to police traffic stops. In Canada, Saulnier et al. (2020) found that drivers exposed to BWC notification during a sobriety checkpoint reported significantly better encounter-specific procedural justice perceptions, but did not differ from non-BWC drivers in their perceptions of police legitimacy. In Uruguay, Ariel et al. (2020) reported

better driver perceptions of procedural justice and more satisfaction with the encounter, but no difference in police lawfulness. In Turkey, Demir et al. (2020a) found that BWCs significantly improved perceptions of procedural justice and legitimacy among drivers at traffic checkpoints. In a path model, they additionally found the impact of BWCs on perceived legitimacy was entirely indirect through perceived procedural justice. Follow-up studies reported significantly improved compliance (both general and specific) and cooperation (Demir et al., 2020b), as well as satisfaction and general perceptions of police (Demir and Kule, 2022). Open-ended responses further supported the contention that BWCs were perceived to improve the quality of interactions between police and drivers, and to compel police officers to behave lawfully (Demir, 2019).

In a sample of US students, Demir (2023) failed to find differences in attitudes about police lawfulness, legitimacy, compliance, and cooperation when participants viewing a video of a police traffic stop were informed that the officer had a BWC (awareness) versus when they were advised that the encounter was recorded by BWC (notification).² Among US and Australian participants, Sivasubramaniam et al. (2021) similarly found that awareness-versus-notification conditions generally did not differ in reports of procedural justice, legitimacy, satisfaction, and cooperation. The additional manipulation of the police officer's behavior to be either respectful or disrespectful was the most salient determinant of respondent attitudes.

The foregoing studies thus provide mixed evidence, but on the whole, suggest BWCs do not strongly influence citizen attitudes independently of officer behavior (Police Executive Research Forum, 2017; Saulnier et al., 2020; Sivasubramaniam et al., 2021), at least in part because of inattentiveness and failure to recall the presence of a BWC (McClure et al., 2017; White et al.,

² In Demir's (2023) study, the group that was notified of the BWC did have more favorable perceptions of procedural justice, police lawfulness, and compliance with police compared to a control condition that did not view the traffic stop video.

2017). Concerning the latter, although the evidence is not strong, the impact of BWCs on citizen attitudes appears to be contingent on BWC awareness or notification, at least when perceived procedural justice is the outcome (Demir, 2023a; Sivasubramaniam et al., 2021; White et al., 2017). On the other hand, the evidence is more consistent that BWCs do increase procedurally just behavior of police officers (McClure et al., 2017; McCluskey et al., 2019). Even studies providing the strongest support for the contention that BWCs influence perceived legitimacy suggest the association is fully mediated by perceived procedural justice (Demir et al., 2020a, 2020b), suggesting behavioral differences in police officers that can be explained by the presence of BWCs.

Procedural Justice and Citizen Attitudes

Procedural justice is rooted in a process-based model of decision-making that concerns how decisions get made, in contrast to an outcome-based model that emphasizes what actual decisions are made (i.e., distributive justice). Procedural justice comprises two broad categories of police behavior: quality of treatment and quality of decision-making (Tyler and Blader, 2003; see also Jonathan-Zamir et al., 2015; Mazerolle et al., 2014; Reisig et al., 2007). *Quality of treatment* refers to the level of dignity, respect, transparency, and concern for wellbeing shown toward citizens. *Quality of decision-making* refers to decision processes that are deliberative, fair, neutral, fact-based, and give voice to citizens even if they are to be sanctioned. The means orientation of the process-based model implies fair treatment in the decision-making process can make people more cooperative, compliant, and even accepting of decisions, independent of outcomes (Tyler, 1988, 1990, 2003; Tyler and Wakslak, 2004). Importantly for policing, procedural justice is also closely linked with judgments about police legitimacy (Sunshine and Tyler, 2003).³

³ For a view of procedural justice as part and parcel of legitimacy, rather than a determinant of it, see Bottoms and Tankebe (2012; also Tankebe, 2013; Tankebe et al., 2016).

A number of studies ask whether isolated encounters with police officers can enhance perceptions of procedural justice and legitimacy, among other outcomes. In Australia, Mazerolle et al. (2012, 2013) reported that drivers stopped at sobriety checkpoints who were administered a procedural justice script perceived significantly more procedural justice during their encounter. They additionally reported an indirect influence on generalized procedural justice, legitimacy, satisfaction, and cooperation via experienced (encounter-specific) procedural justice. In Turkey, Sahin et al. (2017, 2023) replicated the impact of a procedural justice script on experienced procedural justice perceptions, but not on generalized procedural justice perceptions. In Scotland, on the other hand, MacQueen and Bradford (2015) found that procedurally just stops actually resulted in lower ratings of perceived procedural justice and satisfaction than business-as-usual stops. Recently, Terpstra and van Wijck (2023) conducted a field study at police traffic controls of moped drivers in the Netherlands to examine the effect of procedurally just police behavior, measured using systematic social observation, on drivers' perceived procedural justice, measured using surveys after the encounter, and found no evidence.

A number of other studies have innovated with the use of mock encounters between police and citizens. Using traffic stop vignettes in Australia, Barkworth and Murphy (2015) found that participants exposed to procedurally just stops rated higher on compliance than their peers exposed to procedurally unjust stops, with the effect fully mediated by differences in negative emotions (e.g., frustration, anger, anxiousness) elicited by the hypothetical encounter.⁴ In a US study of traffic stop vignettes by Reisig et al. (2018), respondents exposed to procedural injustice reported

⁴ “[I]n the procedural justice condition, the police officer is courteous, friendly, explains their decision to pull over the driver and allows the driver to explain the reason for their non-compliance with road rules. In the procedural injustice condition, in contrast, the police officer is condescending to the driver, uses rude and disrespectful language, does not explain to the driver the reason they were pulled over and does not give the driver the opportunity to explain their situation.” (Barkworth and Murphy, 2015, p. 265).

much lower encounter satisfaction and a wish for the encounter to have been handled differently. They also reported less willingness to comply with the police officer and lower acceptance of the decision. In mock traffic stops portrayed in a video simulation, Lowrey et al. (2016; see also Lowrey-Kinberg, 2018) found that procedurally just policing improved willingness to cooperate, obligation to obey, and trust and confidence relative to business-as-usual stops—all differences were in experienced attitudes rather than generalized attitudes. Using a similar design, Maguire et al. (2017; see also Johnson et al., 2017) found that respondents who viewed procedurally just encounters were more willing to cooperate, felt more obligation to obey, and reported more trust and confidence, relative to procedurally neutral encounters. The authors also incorporated a procedurally unjust condition, and reported worse outcomes than procedurally neutral encounters.

Taken together, the literature on traffic stops suggests procedurally just behavior of police officers has large effects on citizen attitudes in face-to-face encounters. This holds true when survey respondents report on their own experiences with officer behavior manipulated in the field (Mazerolle et al., 2012, 2013; Sahin et al., 2017, 2023; a notable exception is MacQueen and Bradford, 2015), as well as when they report on vicarious experiences with officer behavior manipulated in mock face-to-face encounters, either on video or in vignettes (Barkworth and Murphy, 2015; Johnson et al., 2017; Lowrey et al., 2016; Lowrey-Kinberg, 2018; Maguire et al., 2017; Reisig et al., 2018). The evidence also suggests that procedural justice impacts experienced (encounter-specific) attitudes, but singular encounters do not affect generalized attitudes about police, although there are exceptions suggesting procedural (in)justice can influence generalized attitudes in some contexts (Maguire et al., 2017; Mazerolle et al., 2012, 2013). With that said, some scholars argue that the effects of procedural justice on citizen attitudes and behaviors are mixed and debatable, and note especially the lack of correlation between actual treatment and

perceived treatment (Nagin and Telep, 2017, 2020; Terpstra and van Wijck, 2023; Worden and McLean, 2017, 2018).

Procedural Injustice, Attitudinal Asymmetry, and Loss Aversion

A distinct advantage of scenario-based research has been the ability to compare procedural justice to more than business-as-usual traffic stops, since the design allows manipulation of police behavior in a distinctly negative direction that would not be possible in field experiments (Barkworth and Murphy, 2015; Johnson et al., 2017; Maguire et al., 2017; Reisig et al., 2018). The studies by Johnson et al. (2017) and Maguire et al. (2017) are notable in this regard, since their designs allow tests of symmetry of procedurally just treatment versus procedurally unjust treatment, relative to a procedurally neutral reference point.⁵ Although they did not find a pattern of asymmetry with the experienced outcomes, Maguire et al. (2017) reported that the coefficients from a regression model predicting generalized attitudes were larger in absolute magnitude for procedural injustice compared to procedural justice, by a ratio of 2:1 or more. This suggested the impact of procedural (in)justice may be asymmetrical relative to the neutral reference point—negative encounters with police lead to more negative attitudes than positive encounters lead to positive attitudes. However, they did not perform a formal test of this possibility.

A number of non-experimental studies of police-citizen encounters uncover evidence of asymmetry, as well. Namely, unwelcome outcomes—unwelcome from the standpoint of police—follow negative encounters more so than wanted outcomes follow positive encounters. This seems to be true with respect to compliance with police requests (McCluskey et al., 1999), experienced procedural justice (Worden and McLean, 2017), experienced legitimacy (Thompson and Pickett,

⁵ To our knowledge, these are the only such experimental designs that exist, and are the inspiration for the current study. Barkworth and Murphy (2015) randomly assigned subjects to procedurally just and procedurally unjust conditions, but since they lacked a neutral reference point, they were not able to test for asymmetry.

2021), confidence in the police (Myhill and Bradford, 2012; Skogan, 2006), generalized procedural justice (Bradford et al., 2009; Jackson et al., 2012; Li et al., 2016; Oliveira et al., 2021; Slocum and Wiley, 2018), overall police effectiveness (Bradford et al., 2009; Jackson et al., 2012; Li et al., 2016), police engagement with the community (Bradford et al., 2009), and crime reporting intentions (Rengifo et al., 2019). In these studies, however, there is frequently either no clear reference point or else the reference point refers to no police contact.⁶ The existence of a neutral reference point, as in Maguire et al. (2017), would allow for a more complete test of asymmetry.

The inclusion of a neutral reference point also opens up theoretical explanations for attitudinal asymmetry in police-citizen encounters that have not been explicitly considered to date. We propose that *loss aversion* is one such explanation that might be uniquely applicable to police-citizen encounters. Originating in the prospect theory of Kahneman and Tversky (1979), loss aversion describes a situation in which the pain from a loss is felt more acutely than the pleasure from an equivalent-size gain. It is important for the theory that gains and losses are evaluated with respect to a reference point—this is referred to as *reference dependence*—which might be best characterized as a person’s expectations about the status quo. Formally, loss aversion occurs when a person’s “value function” is steeper in losses and shallower in gains, relative to the reference point.⁷ While the idea was proposed to explain certain anomalies in choice-making under conditions of uncertainty and risk, it has since been readily incorporated into perspectives on crime

⁶ Slocum and Wiley (2018) include self-report neutral police contact as a separate category, along with positive and negative police contacts. However, their test contrasts these three types of contact with a fourth, no-police-contact condition. They find positive contacts are uncorrelated with generalized perceptions of procedural justice, whereas negative contacts significantly worsen procedural justice perceptions. The pattern of results is in a direction suggesting asymmetry, but because they do not contrast the coefficients for positive and negative contacts, it is unclear if the difference in magnitude would be statistically significant.

⁷ Like a utility function in expected utility theory, a value function in prospect theory is concave in gains and convex in losses—in both cases, it is S-shaped. The difference is that gains and losses are asymmetrical in prospect theory, but symmetrical in expected utility theory. In other words, in prospect theory, the bottom half of the S drops lower and steeper than the upper half rises.

decision-making (see Pogarsky et al, 2018). Loss aversion is a quite general phenomenon (Kahneman et al., 1991; Tversky and Kahneman, 1991), and in our view, it also has promise for understanding potential asymmetry of negative and positive encounters with police.

If citizens are loss averse in face-to-face encounters with police, the improvement in attitudes from a procedurally just encounter should be smaller in absolute magnitude than the erosion in attitudes from a procedurally unjust encounter, when both are judged relative to a procedurally neutral reference point. In short, the expectation is that negative encounters with police are more “painful” (attitudinally speaking) than positive encounters are “pleasurable.” Notions of fairness are thought to underlie certain forms of loss aversion (Kahneman et al., 1986), and fairness judgments have obvious affinity with procedural justice in police-citizen encounters.⁸ On that point, recall the finding of Barkworth and Murphy (2015) that differences in frustration and anger fully accounted for differences in compliance in their vignette study of procedural (in)justice. Yet one unexpected finding is the asymmetries documented in prior procedural justice research have been observed for generalized outcomes rather than experienced outcomes. What makes this unexpected is the prediction from prospect theory of *diminishing sensitivity*, one interpretation of which is that loss aversion is stronger in proximate outcomes but weaker in distal outcomes (Tversky and Kahneman, 1991). If we conceptualize experienced attitudes as proximate and generalized attitudes as distal, the expectation would then be for asymmetries to be larger for experienced attitudes, which is the opposite of what Maguire et al. (2017) found in their study.

We recognize that integration of prospect theory with procedural justice theory could be viewed as a misguided attempt to reconcile different perspectives on human nature, especially considering that prospect theory is descended from utility theory. Utility theory conceives of a

⁸ Where resistance to unfair treatment is concerned, to put the matter crudely, it is not a stretch to believe what is true for profit-maximizing firms might be equally true for compliance-maximizing police officers.

rational economic agent who seeks the largest expected payoff under the circumstances—their behavior is outcome-oriented and shaped partly by the threat of punishment. Because behavior is driven by purely instrumental considerations, there is no room for sentiment or the good opinion of others in this theory.⁹ By comparison, procedural justice theory as proposed by Tyler (Tyler and Lind, 1992) assumes a relational actor motivated by fairness, justice, and the desire for positive standing in the eyes of others. In this case, fair treatment improves attitudes and strengthens compliance because it affirms one’s status as a member of a community worthy of dignity, even (and especially) if they have transgressed. In a rough manner of speaking, then, compliant behavior is *compelled* under utility theory but *impelled* under procedural justice theory, and it is quite natural to think of these as incompatible perspectives.

Prospect theory was proposed as a way to more accurately describe perceptions and behavior that are anomalous from the standpoint of utility maximization. It focuses on explaining the behavior of real-life people rather than hopelessly abstract “agents,” and lacks epistemological baggage stemming from the pure hedonism of utility theory. Prospect theory makes room for “irrational” moral and ethical considerations, as suggested by the relevance of “community standards of fairness” in the context of prices, rents, and wages (Kahneman et al., 1986). We see great value in conceptualizing social standing in the eyes of others as within the universe of gains and losses to which people are attuned in face-to-face encounters with police (possibly asymmetrically so), and our view is that prospect theory is well-positioned to accommodate these relational considerations and even make some testable predictions that might seem less obvious from procedural justice theory. Despite their different foundations, then, we view concepts from the two perspectives as complementary, although we acknowledge that view might not be widely

⁹ There technically is room for sentiments, but these are presumed to have been fully and stably incorporated into a decision-maker’s preference function.

shared.¹⁰ Whether integration of these two theories can be fully realized—and in truth, a formal integration is beyond the scope of the current study—remains to be seen. But their combination can perhaps shed some additional light on police-citizen encounters, where social standing and fairness are particularly salient.

Current Study

To better understand how BWCs and police behavior interact at the situational level, the current study adapts the methodology of Maguire et al. (2017) and Johnson et al. (2017) by employing a vignette-based randomized controlled experiment to jointly manipulate BWC presence and police procedural justice in the context of a hypothetical traffic stop. The BWC manipulation is dichotomous—BWC absence versus announced BWC presence. The procedural justice manipulation is trichotomous—procedurally unjust versus procedurally just versus procedurally neutral. The procedurally just vignette is designed to include key elements of procedural justice, for example, *voice* (“Do you have any questions?”), *neutral* and *fact-based* decisions (“I’ve stopped you this evening because the posted speed limit is 30 miles per hour, and you were going 48 miles per hour”), *dignity* and *respect* (repeated use of “sir”), and *concern for wellbeing* (“Listen, every year, people die on these roads from speeding and we’re just trying to keep that from happening.”). The vignette thus exhibits features of quality of treatment and quality of decision-making that have been prioritized in the measurement of procedural justice (Jonathan-Zamir et al., 2015; Mazerolle et al., 2014; Reisig et al., 2007). In a decidedly negative direction, the procedurally unjust condition conveys hostile and threatening treatment by the police officer.

¹⁰ Drawing from Max Weber (2019), one could argue that even the most orthodox version of utility theory is not truly incompatible with procedural justice theory. They represent not two fundamentally different views of human nature, merely two (among other) pure forms of rational social action—ends-oriented purposive rationality and means-oriented value rationality, respectively. Any given social action reflects some varying degree of purposive and value rationality, and “is very rarely oriented to solely one or the other type” (Weber, 2019, p. 103, emphasis removed).

The 3×2 design of this study is guided by questions of whether and how citizen attitudes are jointly affected by the procedural justice manipulation and the BWC manipulation. The use of multiple outcomes, shown in the conceptual model of Figure 1, which span the proximate (e.g., judgments about the police officer's behavior) to distal (e.g., general views about police and the law) continuum, allows tests of whether the manipulations are more strongly correlated with the former than the latter, as predicted by prospect theory.

*** Figure 1 about here ***

Study Design

The study was conducted in Plattsburgh, NY, a city of about 20,000 residents situated in the North Country region of New York State on the western shore of Lake Champlain. As of 2020, the majority of residents were 18 years or older (85.8%), female (52.8%), and most were White (87.6%), followed by Hispanic or Latino (4.4%), Black or African American (3.6%), and other race (Asian, American Indian or Alaska Native, and Native Hawaiian and Other Pacific Islander) (3.1%) (US Census Bureau, 2020). Data collection took place over three distinct time periods: fall 2019, fall 2021, and spring 2022. The gap between spring 2020 and fall 2021 was required by the institutional review board because of COVID-19 restrictions. To supplement the description of the study design below, a flow chart describing key features of the design is provided in Appendix A.

Participants were selected using cluster random sampling of households, and levels of the two main treatment conditions (encounter type, body worn camera) are experimentally manipulated via cluster random assignment. Clusters are 113 streets selected randomly from a sampling frame of 566 (Cartographic, n.d.). From these, households are randomly chosen and then all age-eligible residents are invited to participate in the study. The key design feature is that random assignment of the two main treatment conditions happens at the cluster level, which means

the unit of analysis is the street segment rather than the participant, although the latter may also be treated as the unit of analysis as long as standard errors are clustered at the street segment level. Doing so takes full advantage of the within-cluster variation that exists in survey outcomes.

Surveys were administered by undergraduate students, who were trained and instructed about the survey. During the administration of the survey, they wore their college ID, introduced themselves, provided brief information about the survey, emphasized that participation was anonymous and voluntary, and then kindly asked for participation. Participants were then administered a vignette portraying a police-initiated traffic stop for a speed violation. The vignettes varied along two dimensions: the behavior of the police officer toward the driver (procedurally neutral vs. procedurally unjust vs. procedurally just) and the presence of a body worn camera (present vs. absent). The study design is thus a 3×2 factorial experiment with fully crossed treatments. The factorial design allows comparisons between conditions as well as comparisons within conditions.¹¹

As participants read the vignette, they were instructed to imagine themselves as the driver in the scenario. They were then administered a questionnaire where they provide responses to questions that probe attitudes about the police officer's behavior and features of the encounter, as well assessments of their behavior in the vignette and their general views about the law and police. Vignettes in experimental designs have been used frequently in prior research studying police-citizen interactions in the context of a traffic stop or crime (e.g., Barkworth and Murphy, 2015; Brown and Reisig, 2020; Flippin et al., 2019; Hamm and Wolfe, 2020; Jackson et al., 2021; Nivette and Akoensi, 2019; Reisig et al., 2018).

¹¹ The vignettes were transcriptions of video clips from a stimulated traffic stop used in the study by Maguire et al. (2017) and were obtained from the lead author. The only difference in the current study was the inclusion of the following statement in the BWC condition: "The encounter is being recorded by body worn camera. (Officer points out the camera to the driver.)"

Data and Measures

The data are obtained from a questionnaire administered in person to participants, all of whom are city residents 18 years of age or older. Of 1,111 residents who were contacted, a total of 755 residents participated, yielding a response rate of 68%. There are slight differences in the response rates across the 3×2 treatment conditions, ranging from 65% to 71%. Five surveys are eliminated because of extensive non-response, yielding a final sample size of 750 for analysis. The demographics of the final sample reflected the demographics of the city residents. Specifically, the comparisons (sample vs. city population) were as follows: male (50.9% vs. 47.2%), White (83.6% vs. 87.6%), African American (7.5% vs. 3.6%), Hispanic or Latino (4.5% vs. 4.4%), other race (3.0% vs. 3.1%), and married (40.5% vs. 37% [15 years and older]).¹²

The independent variables of interest are the manipulated treatment levels for encounter type and BWC. The two treatments are fully crossed as part of the 3×2 design, so there are six treatment conditions. In the encounter type condition, the vignettes differ as to whether the police officer behaves toward the stopped driver in a (1) procedurally neutral, (2) procedurally unjust, or (3) procedurally just manner. In the BWC condition, the vignettes differ as to whether or not the police officer begins by stating the encounter is being recorded by BWC and pointing out the camera to the driver. The full transcripts for the vignettes are provided in Appendix B.

The outcome variables are ten measures commonly used in prior research, and their relations to each other are shown in Figure 1. Participants are asked to imagine they are the driver in the vignette, and then to respond to a number of statements assessing the police officer's behavior (procedural justice, effectiveness, lawfulness), perceived legitimacy, their attitudes about the encounter (satisfaction with the encounter, compliance with police), and their general attitudes

¹² It is important to note that city demographics cover the entire population, while the sample demographics cover those 18 years and older.

about the police and law (satisfaction with police, police-community relations, cooperation with police, compliance with the law) within the context of the vignette. All assessments are tied to the hypothetical encounter. All items have five-point Likert response scales, ranging either from strongly disagree (1) to strongly agree (5), or else from very unlikely (1) to very likely (5). Thus, higher values on all measures signify more favorable attitudes toward the encounter and the police. Descriptives are provided in Table 1.

*** Table 1 about here ***

Procedural justice is the principal component from five items (Cronbach's $\alpha = 0.91$): "the police treated me fairly," "the police treated me with dignity and respect," "the police made decision based upon facts, not personal opinions," "the police gave me a chance to express my views before making the decision," and "the police took account of my needs and concerns." These measure key facets of perceived procedural justice of the encounter, including fair/neutral treatment, respectful treatment, quality of decision making, participation (i.e., voice), and trustworthiness (Tyler, 1990; see also Murphy et al., 2017). The other two outcome variables assessing the police officer's behavior are measured from single items. *Police effectiveness* asks participants their level of agreement with the statement: "I would believe the police was doing the job well" (Tyler, 1990). *Police lawfulness* asks whether "the police treated me lawfully" (Jennings et al., 2014).

Police legitimacy is the principal component from four items (Cronbach's $\alpha = 0.93$): "I would trust the police," "I would have confidence in the police," "I would respect the police," and "I would believe the police was doing the right thing." There is not yet consensus on the conceptualization and measurement of police legitimacy (Bottoms and Tankebe, 2012; Cao and Graham, 2019; Jackson and Bradford, 2019; Johnson et al., 2014; Mazerolle et al., 2012; Nix et

al., 2019; Reisig and Trinkner, 2024; Tankebe, 2013), but the selected items have been used in prior research (Tyler, 1990; see also Demir, 2024; Demir et al., 2020a; Mazerolle et al., 2013; Saulnier et al., 2020). *Encounter satisfaction* is a single item inquiring about agreement with the statement: “I would be satisfied with the police during the encounter” (Demir and Kule, 2022; Mazerolle et al., 2012). *Compliance with police* is the principal component from three items (Cronbach’s $\alpha = 0.74$): “I would do what the police told me to do even if I disagreed with the decision,” “I would accept the officer’s decision to give me a ticket even if I did not agree with it,” and “I would feel compelled to do what the police said.” These have been used previously to measure compliance with police directives (Lowrey et al., 2016; Tyler, 1990).

General satisfaction asks participants their agreement with the statement: “I would be satisfied with the police” (Mazerolle et al., 2013). *Police-community relations* presents participants with the statement: “my relations with the police would improve” (Sousa et al., 2018). *Cooperation with police* is the principal component from five items (Cronbach’s $\alpha = 0.92$): “I would call the police to report a crime,” “I would report dangerous activities to the police,” “I would report suspicious activities to the police,” “I would help the police find a suspect by providing them with information,” and “I would be willing to assist the police if asked” (Murphy et al., 2008, 2017; Sunshine and Tyler, 2003; Tyler, 1990). *Compliance with the law* is the response to the statement: “I would feel compelled to follow the law” (Lowrey et al., 2016; Tyler, 1990).

Although control variables are technically unnecessary given random assignment of the vignette conditions, they are useful to judge post-randomization balance and to adjust treatment effect estimates in cases where assigned groups are imbalanced. These include age, sex (male vs. female), race (White vs. Non-White), and employment status (employed vs. not employed) as socio-demographic controls. They also include dummy variables measuring personal encounters

with the police, such as prior voluntary contact, prior involuntary contact, having been stopped, having been ticketed, and having encountered police wearing a BWC. The wave of data collection (fall 2019 vs. fall 2021 vs. spring 2020) is also included as a control variable.

Analytic Strategy

The first step in the analysis is to reduce the dimensionality of the outcomes, since four of them are measured from multiple items—procedural justice, police legitimacy, cooperation with police, and compliance with police. From confirmatory models, we extract the first principal component from each group of items. As they have Likert metric, a higher value on the principal component indicates a more favorable disposition toward the police. Results from the principal component analyses are provided in Appendix C. All other outcome variables are single items retained in Likert metric.¹³

The second step is analysis of variance and effect size estimation. Because the study design is a 3×2 factorial experiment with fully crossed treatments (encounter type, BWC), we perform factorial (two-way) analysis of variance. As treatment is assigned to clusters (street segments), the degrees of freedom are adjusted to reflect the number of clusters. This estimation strategy yields F-statistics on the additive effect for the levels of the two manipulations as well as on their interactive effect. We supplement the F-statistics with effect sizes (eta) for the main effects and interaction (Cohen, 1988).

¹³ Other confirmatory procedures (e.g., principal axis factor analysis, graded response modeling) yield latent variables that produce the same results reported below. Exploratory factor analyses of all measured items for the four hypothesized latent variables produced a very similar pattern of factor loadings, although the number of factors is less than four using traditional factor selection criteria (e.g., minimum eigenvalue > 1). The items measuring procedural justice and cooperation with police coalesce as intended, but there is overlap of the items measuring police legitimacy and compliance with police. Using principal component analysis with a minimum eigenvalue of 0.8 yields four latent factors, while principal axis factor analysis with a minimum eigenvalue of 0.9 yields three. Given that our items are drawn heavily from prior studies, we chose to proceed with the confirmatory approach instead of the exploratory one.

The third step in the analysis is linear regression to obtain point estimates of the impact of each level of the treatments. The models are estimated at the respondent level, employ cluster robust standard errors (clusters are street segments), and preserve the fully crossed design for the two treatments. Although it is technically unnecessary with random assignment, in follow-up models estimated for sensitivity purposes, we also adjust for potential imbalance in the control variables, using standard (parametric) linear regression and semi-parametric linear regression.¹⁴

The fourth and final step in the analysis is the test of (a)symmetry of the effects of procedural injustice and procedural justice relative to the procedurally neutral reference point. We test this in two ways. We first sum the regression coefficients for procedural injustice and procedural justice together and test that the sum is equal to zero. If it is not and the sum is negative, this would be consistent with asymmetry and thus loss aversion, since the negative impact of procedural injustice outweighs the positive impact of procedural justice. We then perform a test that the (absolute) ratio of the coefficient for procedural injustice to the coefficient for procedural justice is larger than unity. Because there is no straightforward standard error for a ratio of coefficients, we obtain 95% confidence bands from the cluster bootstrap with 500 replications. If these bands exclude one, it is evidence for asymmetry and thus loss aversion.

Results

We provide means and standard deviations of the outcome variables across the experimental manipulation in Appendix D. In Table 2, we report the findings from the factorial analysis of variance of the outcome variables measuring attitudes about police. The entries in the

¹⁴ In the standard approach, control variables are included in the model to obtain adjusted treatment effects for encounter type and BWC presence. The semi-parametric approach involves estimating separate linear regression models for each treatment group and then extrapolating predicted outcomes to other groups based on the model coefficients (Imbens, 2004). Since semi-parametric linear regression can only be performed one treatment variable at a time, we estimate the effect of one treatment variable while holding the other fixed along with the control variables.

table are F-statistics and effect sizes (eta) for each component of the ANOVA: the additive effect of encounter type, the additive effect of BWC, and the interactive effect between the two. Effect sizes at least 0.10 indicate differences in means that are practically significant (albeit small) even if they are not statistically significant (Cohen, 1988). Effect sizes at least 0.30 are regarded as medium-sized, and those at least 0.50 are considered to be large. As tests of balance, results from the same procedures for the control variables are provided in Appendix E.

*** Table 2 about here ***

The F-statistics show that encounter type is significantly correlated with all ten outcomes, and all but three of the effect sizes are larger than 0.30, while two are larger than 0.50. We thus find that differences in encounter type generate significant differences in policing attitudes, and these are notable for their magnitude. By comparison, neither BWC presence nor its interaction with encounter type are correlated with any outcome—the F-statistics all fall well short of statistical significance. Moreover, the effect sizes are well under 0.10, which strengthens the conclusion that BWC presence is not associated with the policing attitudes we measured, either alone or as an interaction with encounter type.

With ten sets of ANOVAs and effect sizes shown in the table, and as a further hedge against false discovery from multiple comparisons, we also stack the outcome variables in a global ANOVA. There are two pieces of information that may be culled from this type of test.¹⁵ First, we can obtain overall F-statistics for the additive and interactive effects of encounter type and BWC.

¹⁵ This procedure involves “stacking” the ten outcomes (i.e., appending the outcome variables into a single “long” dataset) and estimating a single ANOVA with a three-way interaction between outcome measure, encounter type, and BWC. The outcomes are standardized before stacking, so the means are placed on a common scale. An anonymous reviewer rightly notes that a MANOVA would also allow us to obtain F-tests for the joint effect of each intervention across all ten outcomes—what is reported in Table 2 as “Stacked: overall effect.” Indeed, when we perform MANOVA (which does not require stacking the outcomes), our overall conclusions are the same. However, MANOVA does not permit estimation of F-tests for variability in effects across outcomes, or what is reported in Table 2 as “Stacked: variable effects.”

These are F-statistics formed from averaging across all ten outcomes. As expected from the outcome-specific results, the overall F-statistic for the additive effect of encounter type is highly significant ($p < 0.0001$), but is not significant for BWC either additively ($p = 0.48$) or interactively ($p = 0.67$). The accompanying effect sizes are 0.38, 0.03, and 0.03, respectively, confirming the overall association between encounter type and policing perceptions is moderate in its practical significance. Second, we can obtain F-statistics for the two-way interactions of outcome measure with encounter type and with BWC, as well as the three-way interaction between outcome measure, encounter type, and BWC. This is a test for whether there is significant variability across outcomes. The results indicate there is significant variation in the additive effect of encounter type ($p < 0.0001$), but there is no significant variation for BWC additively ($p = 0.17$) or interactively ($p = 0.43$). The corresponding effect sizes for these components are 0.16, 0.04, and 0.06, respectively.

The findings thus far point to encounter type as having a pronounced association with policing attitudes, although there is variability across outcomes in the strength of association with encounter type. BWC presence, on the other hand, is not associated with policing attitudes, either additively or interactively with encounter type. The practical implication is that the association between encounter type and policing attitudes is the same irrespective of whether respondents are presented with a BWC or not. Policing attitudes in this sample are explained entirely by the police officer's behavior in the vignette, and not by the presence of a BWC.¹⁶

In Table 3, we present results from linear regression models that produce point estimates for the levels of the two treatment conditions. The models preserve the fully crossed design for the two treatment conditions, but instead of showing coefficients for the main effects and interaction

¹⁶ From this point forward, one might be tempted to eliminate the BWC manipulation from further analysis. However, since the encounter type \times BWC interaction was a key feature of the study design, we believe it appropriate to preserve the fully crossed manipulations at all later points.

effect, we show just the marginal effects of the two treatment conditions. These average over the interaction, but considering the ANOVA results, we do not show any interactive associations. Regression models that introduce the control variables in parametric and semi-parametric specifications are provided in Appendix F.

*** Table 3 about here ***

The coefficients for the levels of encounter type are in reference to the procedurally neutral vignette. Respondents administered the procedurally unjust vignette report significantly worse policing attitudes than their procedurally neutral peers for eight of the ten outcomes, and those administered the procedurally just vignette report significantly better policing attitudes for eight of the ten outcomes. Note that a Bonferroni-corrected p-value is just below 0.002 for 30 tests, and by that criterion, six outcomes are significant for procedurally unjust encounters, and five are significant for procedurally just encounters. Not shown is that tests of the difference in coefficients between the procedurally unjust and procedurally just conditions are highly significant for all ten outcomes. And as we have already seen, BWC presence is not associated with any outcome (either additively or interactively), so we do not discuss it further, other than to observe that BWCs do not appear to soften the impact of procedurally unjust policing, nor do they enhance the impact of procedurally just policing.

To consider one outcome in detail, take the effect of encounter type on procedural justice perceptions (e.g., fairness, dignity, voice), which had the largest effect size in the ANOVA table. As this is a latent variable it is standardized to have zero mean and unit variance, implying the regression slopes are in standard deviation metric. Respondents exposed to the procedurally unjust condition report worse procedural justice perceptions by a full standard deviation relative to their peers exposed to the procedurally neutral condition. Respondents in the procedurally just

condition, on the other hand, report improved procedural justice perceptions by one-half standard deviation compared to procedurally-neutral participants. It is important confirmation of a treatment signal of encounter type that the groups differ by such large margins in their procedural justice perceptions.

Two other attitudinal outcomes concerning the police officer's behavior—effectiveness and lawfulness—exhibit the same pattern as procedural justice. Participants exposed to the procedurally unjust vignette are significantly less likely to agree that the police were doing their job well and that they were treated lawfully, representing three-quarters to one full point on the Likert scale, on average. The procedurally just vignette yields the opposite—participants perceive significantly greater effectiveness and lawfulness than even the procedurally neutral vignette, by about one-third point on the Likert scale.

Participants' perceived legitimacy (e.g., trust, confidence, respect) and satisfaction with the encounter are also significantly worse in the procedurally unjust condition relative to the procedurally neutral condition. Specifically, legitimacy is lower by about two-thirds of a standard deviation and satisfaction is lower by one full point on the Likert scale. By comparison, the procedurally just vignette improves legitimacy by about one-third of a standard deviation and satisfaction by one-half point on the Likert scale, although the former is non-significant after Bonferroni correction. Despite their worse attitudes about the police officer and the encounter, participants in the procedurally unjust condition report they are as willing to comply with police directives (e.g., do what the police officer says, accept the officer's decision). Poor treatment by the police officer in the vignette, relative to the procedurally neutral condition, thus does not translate into non-compliant behavior. For that matter, neither does procedurally just behavior translate into compliant behavior.

When considering general views, participants exposed to a procedurally unjust police officer report they are less satisfied with the police in general (by three-quarters point on the Likert scale), and perceive worsened police-community relations (by one-half point on the Likert scale). But while they are as willing to cooperate with the police (e.g., report dangerous and suspicious activities), they feel less compelled to follow the law (by one-third point on the Likert scale). Although the procedurally just condition does not improve cooperation with police or compliance with the law over and above the procedurally neutral condition, as we saw above, general satisfaction and police-community relations are both better by almost one-half point on the Likert scale, although significance does not survive Bonferroni correction.

Table 4 provides output from the regression model to test for asymmetry. In the left panel is the sum of the marginal effects of procedurally just and procedurally unjust encounters. If this sum is negative and statistically significant, it supports loss aversion—the expectation from prospect theory that perceptions of negative encounters are more disapproving than perceptions of positive encounters are approving. In the right panel is the absolute ratio of the marginal effect of procedural injustice to the marginal effect of procedural justice. If the 95% confidence interval excludes one, it also supports loss aversion. Because the outcomes are listed in the table in a manner broadly consistent with a proximate/distal continuum, an informal test of diminishing sensitivity would be provided by larger asymmetries in (proximate) outcomes toward the top of the table than (distal) outcomes toward the bottom of the table.

*** Table 4 about here ***

When looking across the outcomes, the coefficients do reveal asymmetries in the impact of police officer behavior. For the first three outcomes listed—procedural justice, police effectiveness, and police lawfulness—both measures of asymmetry provide evidence that

procedurally unjust stops produce a more negative perceptual reaction (relative to a procedurally neutral stop) than procedurally just stops yield a positive reaction. This pattern is consistent with loss aversion. What is also notable is these outcomes concern perceptions of the police officer's behavior in the vignette, which along with the weaker asymmetries in police legitimacy and encounter satisfaction, we can think of as the most proximate outcomes in the study. For the other outcome variables, which measure general views about police and the law, we cannot rule out symmetry of negative and positive reactions. This pattern of results is broadly consistent with diminishing sensitivity—the expectation from prospect theory that loss aversion is more pronounced in proximate outcomes than in distal outcomes.

As a highly provisional sensitivity analysis, we explore the possibility that policing attitudes are impacted differently by encounter type and BWC presence following the murder of George Floyd in May 2020. This event sparked widespread protest against excessive police force and culminated in the conviction of Derek Chauvin in April 2021. As this all occurred when data collection was suspended during the COVID-19 pandemic, we can divide the sample into pre-Floyd (fall 2019) and post-Floyd (fall 2021, spring 2022) time periods, and test whether encounter type and body worn camera presence have similar effects on policing attitudes. To investigate, we estimate the linear regression model in Table 3, but add a post-Floyd dummy indicator as another interacting variable, where the three terms of interest are post-Floyd \times encounter type, post-Floyd \times body worn camera, and post-Floyd \times encounter type \times body worn camera. For interested readers, the relevant factorial ANOVAs are reported in Appendix G, and the period-specific marginal effects of encounter type and BWC presence are shown in Appendix H. Figure 2 provides a

graphical summary, and we focus our attention on associations that emerge as significantly different based on the output in the Appendix.¹⁷

*** Figure 2 about here ***

A prominent pattern revealed in Figure 2 is that the association between procedurally just encounters (relative to procedural neutrality) is by and large limited to post-Floyd time periods. Indeed, the association is weaker, and frequently non-significant, in the pre-Floyd time period. Procedurally unjust encounters, on the other hand, are largely consistent in their association with policing attitudes. While we are not able to disentangle the pandemic from George Floyd's murder, this provides some suggestion that the procedurally just vignette improves policing attitudes only in the aftermath of police brutality and social protest. We can also see from Figure 2 that BWC presence is significantly associated with some policing attitudes in the post-Floyd time periods (e.g., police-community relations, cooperation with police), while it has no association in the pre-Floyd time period, with one exception (e.g., compliance with police). The overall pattern is not one that is readily distilled, other than to note that these are more distal attitudes rather than more proximate attitudes that concern the encounter.

Summary and Discussion

The findings from this vignette-based randomized controlled experiment indicate that the kind of encounter participants experience in a simulated traffic stop (by way of a vignette) has a pronounced effect on their perceptions of the police officer's behavior (procedural justice, effectiveness, lawfulness, legitimacy), their attitudes about the nature of the encounter (satisfaction, compliance), and their general views about the law and police (satisfaction, police-community relations, cooperation with police, compliance with the law). Note that these

¹⁷ Although this is not shown, in linear models where we just regress each outcome on the post-Floyd dummy variable, it is not statistically significant for any outcome.

perceptions concern police officer behavior within the context of the vignette. Vignettes in which the police officer behaved in a procedurally unjust manner consistently elicited more disapproving attitudes than vignettes in which the police officer behaved in a procedurally neutral manner, while vignettes in which the police officer behaved in a procedurally just manner elicited more approving attitudes. These findings affirm a number of prior studies that police officer behavior can profoundly shape citizen attitudes, both in the field (Mazerolle et al., 2012, 2013; Sahin et al., 2017, 2023; but see MacQueen and Bradford, 2015) and in hypothetical scenarios (Barkworth and Murphy, 2015; Johnson et al., 2017; Lowrey-Kinberg et al., 2016, 2018; Maguire et al., 2017; Reisig et al., 2018).

A second condition that randomly varied the announced presence of a body worn camera (BWC) had no impact on policing attitudes. Instead, attitudes were explained entirely by the police officer's behavior in the vignette, and the presence of a BWC neither improved nor worsened attitudes under any procedural justice condition. Our results thus confirm the finding from Sivasubramaniam et al. (2021) that BWC presence does not influence attitudes about police-citizen interactions either independently of, or in combination with, a police officer's behavior (see also Police Executive Research Forum, 2017). They do, however, conflict with some research indicating BWCs can affect policing attitudes (Demir et al., 2020a, 2020b; McClure et al., 2017). Given that our participants obtained direct information from the vignette about police officer behavior as procedurally unjust or just or neutral, it stands to reason that BWCs might not significantly and independently alter their attitudes.

It is also possible that respondents presented with a BWC, despite notification of it by the police officer in the vignette, still failed to register it and were thus unaware of its presence. As the survey did not include an awareness check for the BWC, we are unable to rule this out. With that

said, the null result for the BWC condition does align with past research indicating null or weak effects of BWCs on policing attitudes, especially when people are unaware of their use (Demir, 2019, 2023; McClure et al., 2017; Sivasubramaniam et al., 2021; White et al., 2017). Past research does indicate that, to the extent BWCs alter the situational behavior of police officers, they do appear to improve citizen attitudes in the aftermath of a face-to-face encounter (McClure et al., 2017; McCluskey et al., 2019). But it is the behavior of the police officer to which citizens are attuned in these studies—not to the mere presence of a BWC.

What is thus not in doubt is the salience of police officer behavior as a driver of a wide variety of respondent attitudes in hypothetical encounters. In the procedurally just condition, the police officer exhibited high-quality treatment (dignity, respect, concern for wellbeing) and high-quality decision-making (voice, neutrality, fact-based), which are two procedural justice domains that have been widely studied (Jonathan-Zamir et al., 2015; Mazerolle et al., 2014; Reisig et al., 2007). In the procedurally unjust condition, the police officer was openly hostile and threatening, and in the procedurally neutral condition, the police officer engaged in standard compulsory dialogue (see also Johnson et al., 2017; Maguire et al., 2017).

The results also add to a growing body of evidence that negative encounters with police can be perceived as more unfavorable than positive encounters are perceived as favorable. This is especially true with respect to perceptions of police officer behavior (procedural justice, effectiveness, lawfulness). Drawing from the prospect theory of Kahneman and Tversky (1979), we proposed this could be understood as a form of *loss aversion* and *reference dependence* in police encounters, whereby the loss experienced from a procedurally unjust encounter is more subjectively consequential than the gain experienced from a procedurally just encounter, relative to a procedurally neutral reference point. In prospect theory, loss aversion is also expected to be

larger the more proximate the outcome is to the encounter, which is a form of *diminishing sensitivity*. This is the general pattern we observed. By comparison, distal attitudes exhibit attitudinal symmetry rather than asymmetry.

Police behavior plays a crucial role in shaping citizens' attitudes toward police officers, as well as their willingness to cooperate and comply. To the extent our findings have policy implications, they suggest that agency policies should be developed to enhance procedurally just police behaviors. This could be achieved through training, the use of technology such as BWCs for training purposes, and supervision (see Nagin and Telep, 2020, for discussion on procedural justice).

Limitations

One limitation of this study is that, although the sample size is large ($N = 750$), an important consideration for statistical power with cluster random assignment is the number of clusters ($K = 113$). This could explain the finding that the interaction between encounter type and BWC is not statistically significant for any outcome. However, this concern is mitigated by the observation that the effect sizes for the additive effect of BWC presence, as well as for the interactive effect of encounter type and BWC, were ruled out across ten outcomes as having even a small impact. Relatedly, randomization occurred across street segments rather than respondents, for both design and practical reasons. Because the sampling units were households, assignment of all household members to the same conditions avoided the possibility of contamination. And since the surveys were administered by a team of students dispatched to sampled street segments, it was further felt that we could minimize assignment error by assigning all selected households within a street segment to the same condition. That risks introducing unobserved correlation between segment

characteristics and the assigned conditions, although with a sufficiently large number of street segments (there are 113 in our study), we would expect potential biases to disappear.

A second limitation is the use of vignettes to probe citizen attitudes rather than the study of police-citizen interactions in real-world settings. Although we adapted the vignette transcripts from video clips of simulated traffic stops used in another study (Maguire et al., 2017), the design necessarily introduces some artificiality with respect to situational context. This necessarily glosses over many features of actual police stops, such as variation in the driver's demeanor, the presence of passengers, and the location of the stop, among many other examples. A "sanitized" vignette may thus yield unusually large effect sizes due to the lack of situational detail. Furthermore, participant compliance was based on self-report and thus lacks an external criterion for validation purposes. To the extent there is a discrepancy between what people say they would do in hypothetical situations, and what they would actually do in real-world situations, the results presented here would not necessarily generalize to real-world behavior. Furthermore, because the types of police behaviors were manipulated, this could have inadvertently directed the participants' attitudes in a particular direction. In other words, they could have merely responded in a way consistent with how the conditions were manipulated.

Finally, pre-test measures were not collected to minimize the burden on participants and to avoid potential biases arising from pre-test measurement. However, pre-test data might have provided a more comprehensive understanding of the baseline attitudes of participants, which would have allowed assessment of changes in attitudes attributable to the manipulations.

Conclusion

This study contributes to the body of evidence on the salience of procedural justice as a paradigm for modern policing, and to the mixed evidence concerning the efficacy of body worn

cameras. Body worn cameras had no impact on policing attitudes in mock traffic stops, whereas the situational behavior of police officers—the degree to which they treated stopped drivers in a procedurally just manner, a procedurally unjust manner, or a procedurally neutral manner—was highly relevant as a determinant of policing attitudes. Furthermore, for judgments about police officer behavior in the vignette (procedural justice, effectiveness, lawfulness), procedurally unjust encounters resulted in more negative assessments than procedurally just encounters resulted in positive assessments. This form of asymmetry is consistent with loss aversion in police-citizen encounters.

As to the question of recorded justice versus procedural justice, the results of this study point unambiguously to the latter. High-quality treatment and high-quality decision-making, the keystones of the process-based model, appear to significantly improve subjective assessments of the police in simulated traffic stops. Negative encounters, on the other hand, yield substantial setbacks for police.

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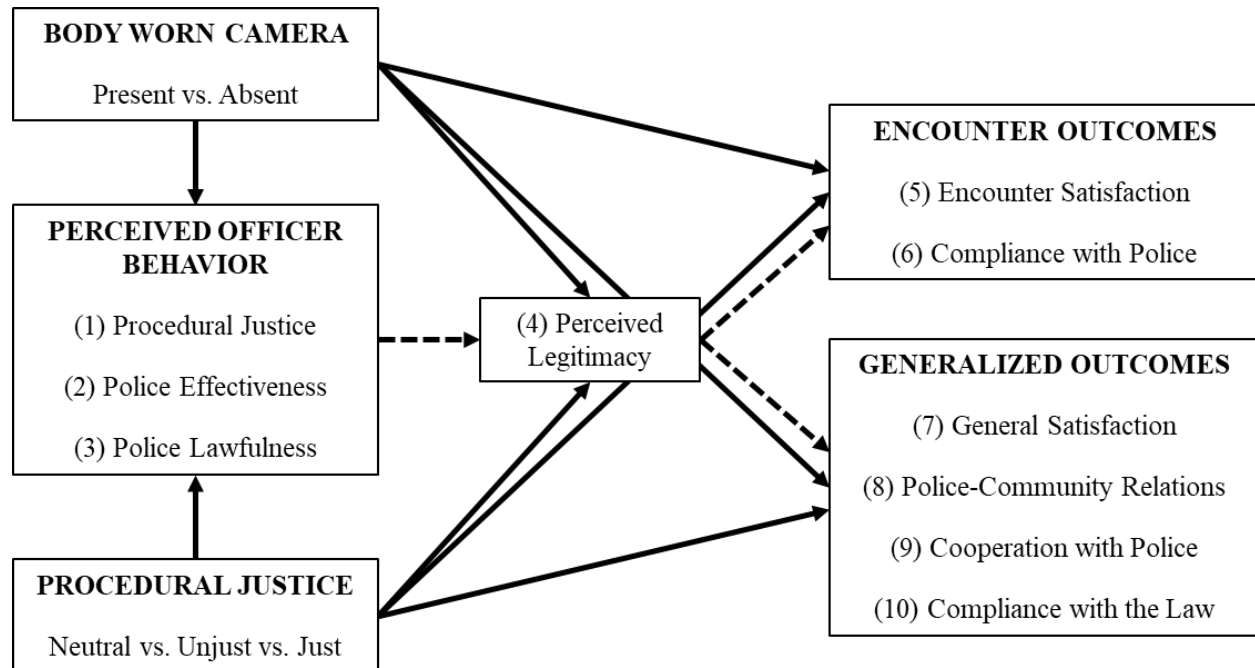
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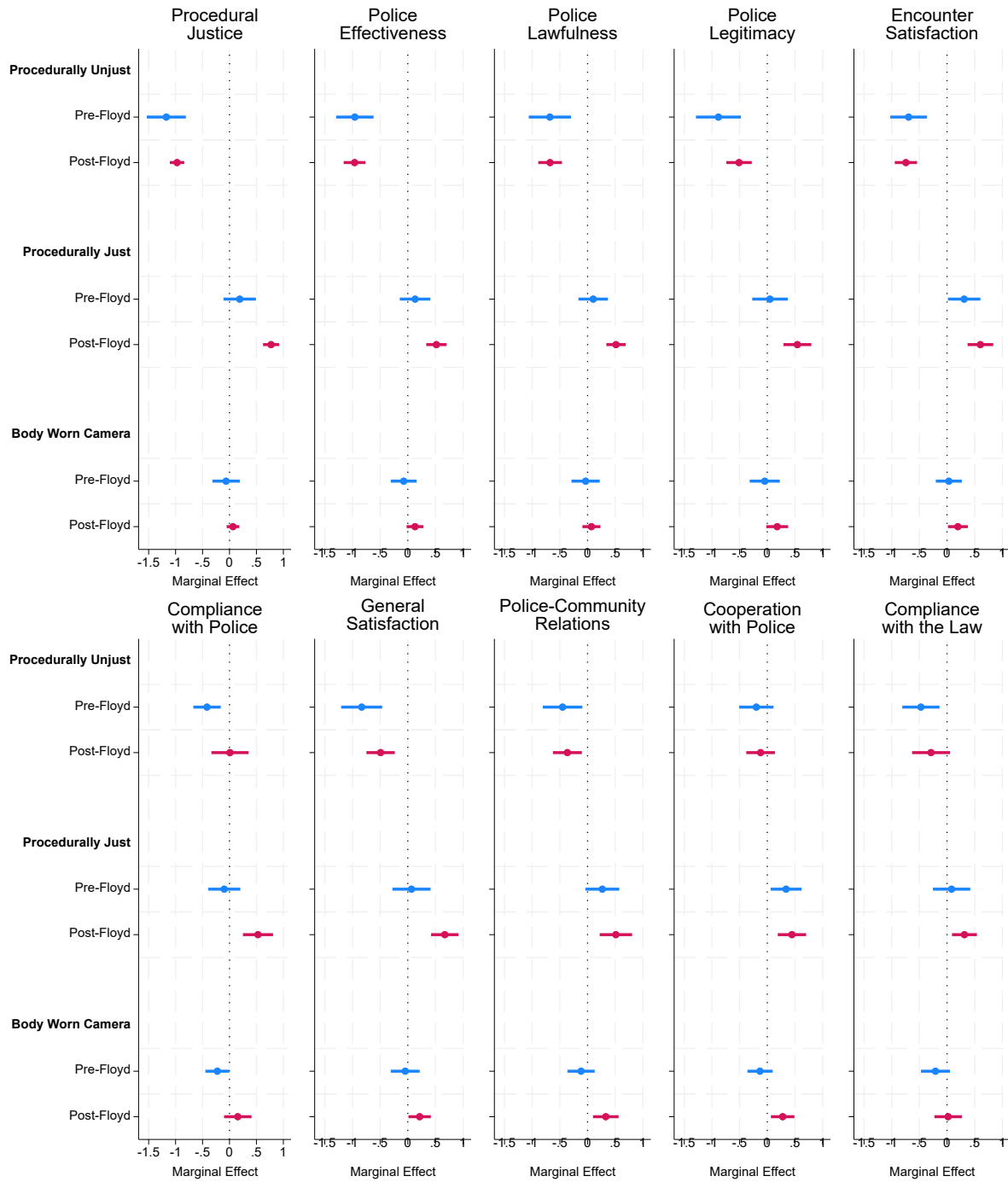
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Figure 1
Conceptual Model



Note: Dashed lines are shown to illustrate the theorized associations between the outcome variables. In the analyses, only the associations represented by the solid lines are estimated.

Figure 2
Marginal Effects of Encounter Type and Body Worn Camera, by Time Period



Note: Coefficients are period-specific marginal effects of the treatment levels, and outcomes are standardized to put them on a common scale. They are obtained from linear regression models that include a three-way interaction between encounter type, body worn camera presence, and a dummy indicator for post-Floyd data collection periods. Relevant F-tests are provided in the Appendix. Standard errors are cluster robust.

Table 1
Descriptive Statistics

Variable	N	Mean (SD)	Min, Max	ICC
Treatment variables				
Procedurally neutral, w/o BWC	750	21%	0 , 1	—
Procedurally unjust, w/o BWC	750	16%	0 , 1	—
Procedurally just, w/o BWC	750	18%	0 , 1	—
Procedurally neutral, w/ BWC	750	16%	0 , 1	—
Procedurally unjust, w/ BWC	750	13%	0 , 1	—
Procedurally just, w/ BWC	750	16%	0 , 1	—
Outcome variables				
Procedural justice (PCA)	750	0.00 (1.00)	-2 , 1	48%
Police effectiveness	750	3.58 (1.16)	1 , 5	37%
Police lawfulness	750	3.92 (1.05)	1 , 5	25%
Police legitimacy (PCA)	750	0.00 (1.00)	-3 , 1	31%
Encounter satisfaction	750	3.21 (1.28)	1 , 5	32%
Compliance with police (PCA)	750	0.00 (1.00)	-4 , 1	14%
General satisfaction	750	3.45 (1.14)	1 , 5	29%
Police-community relations	750	3.46 (1.23)	1 , 5	23%
Cooperation with police (PCA)	750	0.00 (1.00)	-3 , 1	15%
Compliance with the law	750	4.13 (0.86)	1 , 5	16%
Control variables				
Age	706	43.47 (18.12)	18 , 91	29%
Male	750	51%	0 , 1	1%
White	750	84%	0 , 1	10%
Employed	742	74%	0 , 1	11%
Prior voluntary contact	737	56%	0 , 1	8%
Prior involuntary contact	734	72%	0 , 1	12%
Prior stop	735	79%	0 , 1	9%
Prior ticket	732	69%	0 , 1	4%
Prior BWC encounter	750	23%	0 , 1	18%
Wave: Fall 2019	750	43%	0 , 1	—
Wave: Fall 2021	750	30%	0 , 1	—
Wave: Spring 2022	750	27%	0 , 1	—
Cluster size	113	6.64 (4.84)	1 , 21	—

PCA = principal component analysis used to construct variable.

ICC = intraclass correlation, indicating percentage of total variance that exists between clusters (K = 113 street segments).

Notes: Means of binary variables are shown as percentages. Details on the PCA are provided in the Appendix.

Table 2
Factorial Analysis of Variance and Effect Sizes

Outcome variable	Model	Additive effect: Encounter type			Additive effect: Body worn camera			Interactive effect: Encounter × BWC		
		F	η	p	F	η	p	F	η	p
	p									
Procedural justice (PCA)	0.0000	186.86	0.63	0.0000	0.00	0.00	0.9801	0.31	0.03	0.7319
Police effectiveness	0.0000	96.93	0.54	0.0000	0.13	0.01	0.7154	0.86	0.06	0.4243
Police lawfulness	0.0000	42.75	0.41	0.0000	0.05	0.01	0.8237	0.09	0.02	0.9098
Police legitimacy (PCA)	0.0000	43.70	0.40	0.0000	0.68	0.04	0.4111	0.29	0.04	0.7520
Encounter satisfaction	0.0000	86.89	0.48	0.0000	2.11	0.07	0.1496	0.08	0.02	0.9202
Compliance with police (PCA)	0.0236	6.31	0.16	0.0025	0.03	0.01	0.8676	0.03	0.01	0.9741
General satisfaction	0.0000	65.04	0.41	0.0000	1.02	0.05	0.3145	0.16	0.02	0.8523
Police-community relations	0.0000	40.42	0.32	0.0000	1.91	0.07	0.1696	0.45	0.05	0.6409
Cooperation with police (PCA)	0.0000	19.11	0.22	0.0000	1.96	0.06	0.1646	0.39	0.03	0.6756
Compliance with the law	0.0012	10.15	0.23	0.0001	0.43	0.04	0.5120	1.01	0.07	0.3674
Stacked: Overall effect	—	91.79	0.38	0.0000	0.50	0.03	0.4792	0.41	0.03	0.6667
Stacked: Variable effects	—	12.95	0.16	0.0000	1.45	0.04	0.1746	1.04	0.03	0.4265

Notes: N = 750 respondents; K = 113 clusters (street segments). In the main panel, each row is a separate factorial ANOVA following linear regression with adjustment for the number of clusters. Effect sizes for each component of the analysis of variance are eta, which is in the correlation family, and for which thresholds for practical significance are 0.1 (small), 0.3 (medium), and 0.5 (large). In the bottom panel, all ten outcomes are stacked and then a single ANOVA is estimated to investigate the overall effect across outcomes and the degree of variability in effects across outcomes. Prior to stacking, the outcomes are y-standardized to put them on a common scale.

Table 3
Linear Regression Models

Outcome variable	Encounter type: Procedurally unjust vs. neutral			Encounter type: Procedurally just vs. neutral			Body worn camera: Present vs. absent		
	b	se	p	b	se	p	b	se	p
Procedural justice (PCA)	-1.09	0.11	0.0000	0.49	0.10	0.0000	-0.00	0.08	0.9801
Police effectiveness	-1.16	0.13	0.0000	0.38	0.11	0.0014	0.03	0.10	0.7154
Police lawfulness	-0.75	0.13	0.0000	0.32	0.09	0.0010	0.02	0.09	0.8237
Police legitimacy (PCA)	-0.68	0.14	0.0000	0.31	0.12	0.0144	0.08	0.10	0.4111
Encounter satisfaction	-0.97	0.14	0.0000	0.58	0.13	0.0000	0.15	0.11	0.1496
Compliance with police (PCA)	-0.19	0.13	0.1630	0.23	0.13	0.0776	-0.02	0.10	0.8676
General satisfaction	-0.74	0.15	0.0000	0.43	0.15	0.0039	0.11	0.11	0.3145
Police-community relations	-0.51	0.16	0.0021	0.47	0.16	0.0036	0.17	0.12	0.1696
Cooperation with police (PCA)	-0.18	0.12	0.1324	0.36	0.11	0.0019	0.13	0.09	0.1646
Compliance with the law	-0.32	0.12	0.0076	0.17	0.09	0.0711	-0.06	0.09	0.5120

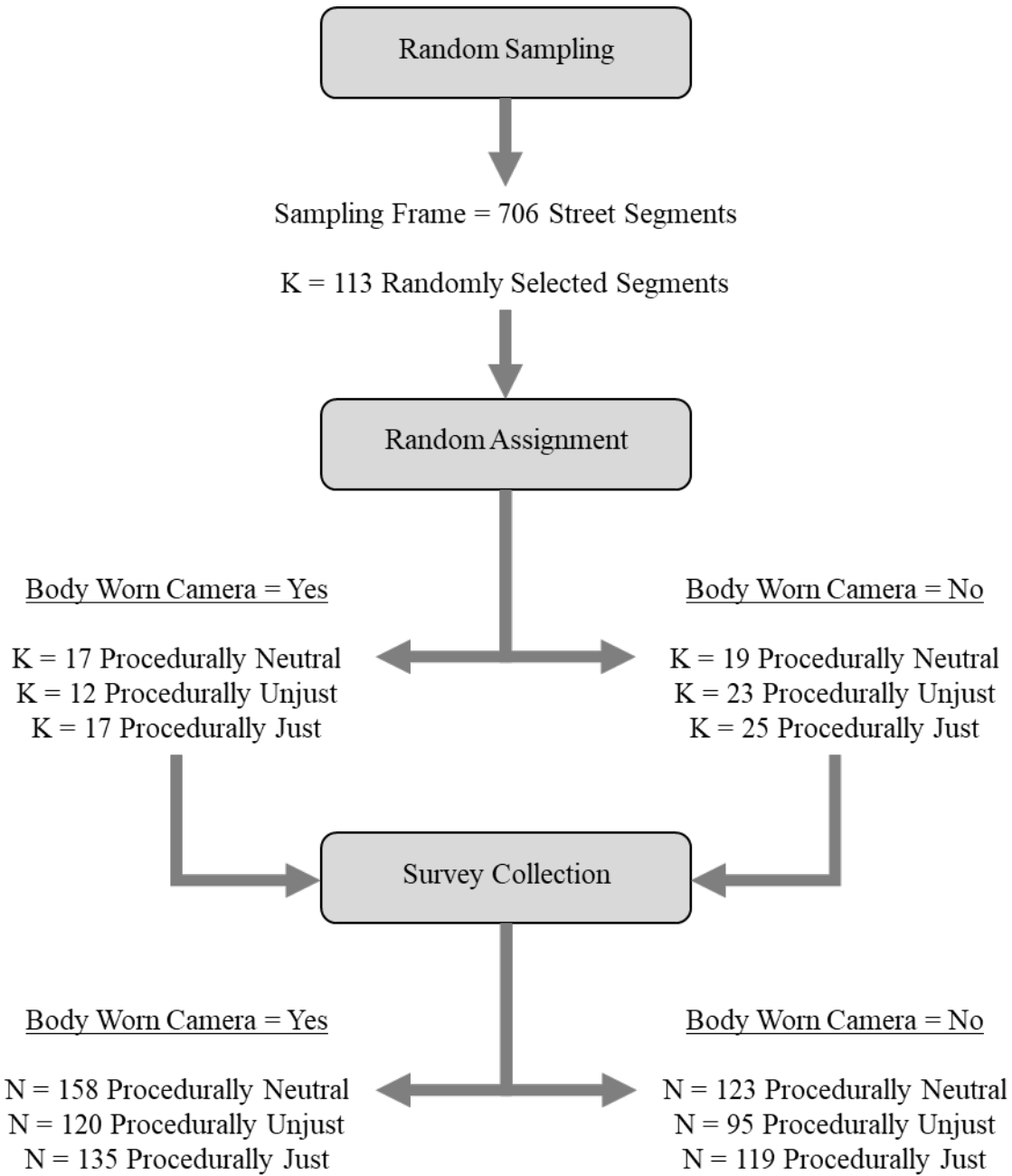
Notes: N = 750 respondents; K = 113 clusters (street segments). Each row is a separate regression model. Models preserve the fully crossed design with an interaction between encounter type and body worn camera presence, but only the marginal effects of the treatment levels are shown. Standard errors are cluster robust, and p-values are two-sided. A Bonferroni adjustment for 30 tests yields a 5% significance level of 0.0017.

Table 4
Tests of Asymmetry of Procedurally Unjust and Procedurally Just Encounters

Outcome variable	Sum of coefficients:			Ratio of coefficients:		
	est.	se	p	est.	lower	upper
Procedural justice	-0.60	0.19	0.0018	2.22	1.38	4.63
Police effectiveness	-0.78	0.22	0.0004	3.08	1.58	7.97
Police lawfulness	-0.43	0.19	0.0242	2.34	1.12	6.23
Police legitimacy	-0.37	0.24	0.1152	2.21	0.87	10.24
Encounter satisfaction	-0.39	0.25	0.1118	1.68	0.90	3.67
Compliance with police	0.04	0.23	0.8622	0.82	0.01	11.43
General satisfaction	-0.31	0.28	0.2560	1.73	0.69	6.51
Police-community relations	-0.05	0.30	0.8792	1.10	0.33	6.76
Cooperation with police	0.18	0.22	0.4143	0.51	0.02	3.22
Compliance with the law	-0.16	0.18	0.3883	1.94	0.45	44.41

Notes: N = 750 respondents; K = 113 clusters (street segments). For the sum of coefficients, the estimate is the post-estimation sum of the procedurally unjust and procedurally just marginal effects shown in Table 3. The p-value is for a test of the null hypothesis that the sum equals zero. For the ratio of coefficients, the estimate is the absolute value of the ratio of the procedurally unjust to procedurally just marginal effects shown in Table 3. The percentile-based and bias-corrected confidence bands (95%) are obtained via the cluster bootstrap with 500 replications, and provide a test of the null hypothesis that the ratio equals 1.

Appendix A
Design of the Cluster Randomized Experiment



Note: N refers to respondents, while K refers to street segments.

Appendix B

Description of Vignettes

Procedurally neutral condition	Procedurally unjust condition	Procedurally just condition
(Officer approaches the vehicle.) Officer: [<i>The encounter is being recorded by body worn camera. (Officer points out the camera to the driver.)</i>] You were going 48 in a 30. License and registration.	(Officer approaches the vehicle.) Officer: [<i>The encounter is being recorded by body worn camera. (Officer points out the camera to the driver.)</i>] Are you out of your damned mind driving like that? You were going 48 in a 30. What are you trying to kill somebody? Give me your license and registration.	(Officer approaches the vehicle.) Officer: Good evening, sir, my name is Officer Smith. [<i>The encounter is being recorded by body worn camera. (Officer points out the camera to the driver.)</i>] I've stopped you this evening because the posted speed limit is 30 miles per hour, and you were going 48 miles per hour. May I have your license and registration, please, sir?
Citizen: Here you go. Officer: Hang tight.	Citizen: Here you go. Officer: Don't move.	Citizen: Here you go. Officer: Okay, thanks so much. I'll be right back.
(Officer returns to patrol car to inspect paperwork.) Officer: Mr. XXX, I'm issuing you a ticket for speeding. Sign on the bottom line.	(Officer returns to patrol car to inspect paperwork.) Officer: Mr. XXX, you're getting a ticket for speeding. You're lucky I don't arrest you for reckless driving. Sign on the bottom line.	(Officer returns to patrol car to inspect paperwork.) Officer: Mr. XXX, I've decided to issue you a ticket for speeding. Instructions for how to pay the fine or challenge the ticket are on the back of the ticket. Make sure you take action within 30 days or it will double. Do you have any questions? Citizen: No, sir. Officer: Would you please sign on the ticket on the bottom line where it's indicated? Here is a pen for you, sir.
(Citizen signs the citation.) Officer: You're free to go.	(Citizen signs the citation.) Officer: Now, get out of here. I better never see you driving around here like that again.	(Citizen signs the citation.) Officer: Okay, thanks so much. Listen, every year, people die on these roads from speeding and we're just trying to keep that from happening. Our goal is to keep the roads safe by making sure people drive the speed limit. Do you have any questions? Citizen: I don't think so. Officer: I just want to finish off by thanking you for your time and attention, and please drive carefully, sir.

Note: Body worn camera manipulation (present vs. absent) is bracketed in italics.

Appendix C

Confirmatory Principal Component Analyses

Variable	N	Mean (SD)	Min, Max	Loading	Uniqueness
Procedural justice					
Eigenvalue of first principal component = 3.70					
Fair: "The police treated me fairly."	750	3.73 (1.23)	1 , 5	0.89	0.21
Respectful: "The police treated me with dignity and respect."	750	3.58 (1.31)	1 , 5	0.90	0.19
Factual: "The police made decision based upon facts, not personal opinions."	750	3.88 (1.08)	1 , 5	0.79	0.38
Voice: "The police gave me a chance to express my views before making the decision."	750	2.91 (1.38)	1 , 5	0.85	0.27
Considerate: "The police took account of my needs and concerns."	750	2.99 (1.31)	1 , 5	0.87	0.25
Police legitimacy					
Eigenvalue of first principal component = 3.28					
Trust: "I would trust the police."	750	3.69 (1.12)	1 , 5	0.93	0.13
Confidence: "I would have confidence in the police."	750	3.71 (1.09)	1 , 5	0.94	0.11
Respect: "I would respect the police."	750	3.91 (1.05)	1 , 5	0.88	0.22
Do right: "I would believe the police was doing the right thing."	750	3.67 (1.07)	1 , 5	0.86	0.26
Compliance with police					
Eigenvalue of first principal component = 1.97					
Comply: "I would do what the police told me to do even if I disagreed with the decision."	750	3.82 (0.97)	1 , 5	0.81	0.35
Accept: "I would accept the officer's decision to give me a ticket even if I did not agree with it."	750	3.83 (0.98)	1 , 5	0.81	0.34
Compelled: "I would feel compelled to do what the police said."	750	3.91 (0.95)	1 , 5	0.81	0.34
Cooperation with police					
Eigenvalue of first principal component = 3.79					
Crime: "I would call the police to report a crime."	750	3.79 (1.34)	1 , 5	0.81	0.35
Dangerous: "I would report dangerous activities to the police."	750	3.98 (1.12)	1 , 5	0.90	0.20
Suspicious: "I would report suspicious activities to the police."	750	3.79 (1.19)	1 , 5	0.89	0.21
Information: "I would help the police find a suspect by providing them with information."	750	3.78 (1.25)	1 , 5	0.90	0.19
Assistance: "I would be willing to assist the police if asked."	750	3.83 (1.18)	1 , 5	0.86	0.27

Appendix D

Outcome Descriptives by Experimental Manipulation

Outcome variable	By encounter type			By body worn camera	
	Procedurally neutral (N = 281)	Procedurally unjust (N = 215)	Procedurally just (N = 254)	No BWC (N = 413)	BWC (N = 337)
Procedural justice (PCA)	0.15 (0.82)	-0.95 (0.76)	0.64 (0.73)	-0.01 (1.02)	0.01 (0.98)
Police effectiveness	3.79 (1.00)	2.63 (1.12)	4.17 (0.83)	3.56 (1.19)	3.61 (1.13)
Police lawfulness	4.02 (0.93)	3.27 (1.15)	4.35 (0.79)	3.90 (1.06)	3.94 (1.03)
Police legitimacy (PCA)	0.09 (0.95)	-0.59 (1.01)	0.40 (0.79)	-0.04 (1.05)	0.05 (0.93)
Encounter satisfaction	3.29 (1.24)	2.32 (1.09)	3.87 (1.03)	3.13 (1.31)	3.31 (1.24)
Compliance with police (PCA)	-0.02 (1.07)	-0.21 (0.93)	0.20 (0.94)	0.00 (1.05)	-0.01 (0.93)
General satisfaction	3.52 (1.12)	2.78 (1.06)	3.95 (0.91)	3.39 (1.17)	3.53 (1.09)
Police-community relations	3.45 (1.21)	2.93 (1.22)	3.92 (1.06)	3.38 (1.22)	3.56 (1.22)
Cooperation with police (PCA)	-0.07 (1.06)	-0.25 (0.97)	0.30 (0.87)	-0.06 (1.00)	0.08 (1.00)
Compliance with the law	4.16 (0.89)	3.84 (0.95)	4.33 (0.65)	4.15 (0.87)	4.10 (0.85)

Outcome variable	By encounter type and body worn camera					
	Procedurally neutral w/o BWC (N = 158)	Procedurally unjust w/o BWC (N = 120)	Procedurally just w/o BWC (N = 135)	Procedurally neutral w/ BWC (N = 123)	Procedurally unjust w/ BWC (N = 95)	Procedurally just w/ BWC (N = 119)
Procedural justice (PCA)	0.17 (0.84)	-0.93 (0.85)	0.61 (0.74)	0.12 (0.79)	-0.97 (0.65)	0.68 (0.71)
Police effectiveness	3.74 (1.06)	2.70 (1.19)	4.11 (0.89)	3.85 (0.92)	2.55 (1.03)	4.23 (0.74)
Police lawfulness	4.04 (0.89)	3.25 (1.22)	4.33 (0.78)	4.01 (0.98)	3.31 (1.05)	4.37 (0.80)
Police legitimacy (PCA)	0.03 (1.01)	-0.58 (1.07)	0.35 (0.87)	0.16 (0.87)	-0.61 (0.93)	0.46 (0.68)
Encounter satisfaction	3.25 (1.26)	2.24 (1.12)	3.78 (1.10)	3.34 (1.22)	2.42 (1.05)	3.97 (0.93)
Compliance with police (PCA)	-0.03 (1.10)	-0.19 (1.00)	0.21 (1.00)	-0.02 (1.03)	-0.24 (0.82)	0.20 (0.88)
General satisfaction	3.49 (1.16)	2.74 (1.09)	3.87 (0.97)	3.56 (1.08)	2.82 (1.02)	4.05 (0.83)
Police-community relations	3.44 (1.21)	2.85 (1.19)	3.78 (1.10)	3.46 (1.21)	3.04 (1.25)	4.08 (0.99)
Cooperation with police (PCA)	-0.12 (1.04)	-0.28 (0.92)	0.20 (0.97)	-0.00 (1.09)	-0.23 (1.03)	0.40 (0.74)
Compliance with the law	4.20 (0.92)	3.93 (0.94)	4.29 (0.69)	4.12 (0.85)	3.73 (0.96)	4.38 (0.61)

PCA = principal component analysis used to construct variable.

Appendix E
Factorial Analysis of Variance and Effect Sizes for the Control Variables

Control variable (% missing)	Model p	Additive effect: Encounter type			Additive effect: Body worn camera			Interactive effect: Encounter × BWC		
		F	η	p	F	η	p	F	η	p
Age (5.9%)	0.2206	2.87	0.15	0.0607	1.79	0.09	0.1835	0.00	0.00	0.9982
Male (0.0%)	0.9869	0.23	0.03	0.7956	0.01	0.00	0.9191	0.10	0.02	0.9061
White (0.0%)	0.2913	0.91	0.06	0.4037	4.54	0.09	0.0354	0.07	0.02	0.9336
Employed (1.1%)	0.7492	0.38	0.04	0.6846	0.72	0.04	0.3965	0.89	0.06	0.4150
Prior voluntary contact (1.7%)	0.0110	3.31	0.11	0.0402	6.05	0.10	0.0154	1.27	0.07	0.2837
Prior involuntary contact (2.1%)	0.1929	1.99	0.11	0.1410	1.75	0.07	0.1891	0.28	0.03	0.7554
Prior stop (2.0%)	0.0705	3.34	0.10	0.0392	3.39	0.07	0.0683	2.01	0.08	0.1385
Prior ticket (2.4%)	0.1446	3.38	0.10	0.0374	0.84	0.04	0.3605	0.39	0.03	0.6758
Prior BWC encounter (0.0%)	0.8533	0.84	0.07	0.4327	0.09	0.01	0.7690	0.19	0.04	0.8275
Wave: Fall 2019	0.7692	0.36	0.11	0.7012	0.73	0.10	0.3949	0.46	0.11	0.6325
Wave: Fall 2021	0.5671	1.10	0.18	0.3379	0.27	0.07	0.6044	0.67	0.13	0.5150
Wave: Spring 2022	0.9117	0.64	0.12	0.5280	0.14	0.04	0.7083	0.01	0.01	0.9929
Stacked: Overall effect	—	3.44	0.05	0.0353	4.41	0.04	0.0380	0.89	0.03	0.4148
Stacked: Variable effects	—	2.06	0.08	0.0151	1.17	0.05	0.3205	0.95	0.04	0.5170

Notes: N = 750 respondents; K = 113 clusters (street segments). In the main panel, each row is a separate factorial ANOVA following linear regression with adjustment for the number of clusters. Effect sizes for each component of the analysis of variance are eta, which is in the correlation family, and for which thresholds for practical significance are 0.1 (small), 0.3 (medium), and 0.5 (large).

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Appendix F Parametric and Semi-Parametric Regression Adjustment

Outcome variable	Encounter type: Procedurally unjust vs. neutral			Encounter type: Procedurally just vs. neutral			Body worn camera: Present vs. absent		
	b	se	p	b	se	p	b	se	p
<u>Parametric</u>									
Procedural justice	-1.05	0.10	0.0000	0.48	0.10	0.0000	-0.01	0.08	0.8716
Police effectiveness	-1.10	0.13	0.0000	0.34	0.11	0.0026	-0.02	0.09	0.8666
Police lawfulness	-0.73	0.12	0.0000	0.30	0.10	0.0024	-0.01	0.09	0.9515
Police legitimacy	-0.60	0.12	0.0000	0.27	0.11	0.0170	0.01	0.09	0.8906
Encounter satisfaction	-0.90	0.14	0.0000	0.53	0.13	0.0001	0.11	0.11	0.2908
Compliance with police	-0.15	0.12	0.2314	0.18	0.12	0.1209	-0.10	0.09	0.3078
General satisfaction	-0.66	0.13	0.0000	0.43	0.12	0.0007	0.03	0.10	0.7453
Police-community relations	-0.45	0.15	0.0029	0.45	0.14	0.0017	0.14	0.11	0.2169
Cooperation with police	-0.15	0.10	0.1598	0.31	0.10	0.0033	0.04	0.08	0.5851
Compliance with the law	-0.30	0.09	0.0018	0.14	0.08	0.0951	-0.12	0.07	0.0822
<u>Semi-Parametric</u>									
Procedural justice	-1.07	0.10	0.0000	0.52	0.09	0.0000	-0.02	0.08	0.8265
Police effectiveness	-1.14	0.12	0.0000	0.39	0.11	0.0004	-0.02	0.10	0.8589
Police lawfulness	-0.70	0.12	0.0000	0.36	0.09	0.0000	-0.01	0.09	0.9208
Police legitimacy	-0.61	0.11	0.0000	0.30	0.11	0.0071	0.01	0.09	0.9195
Encounter satisfaction	-0.90	0.12	0.0000	0.59	0.13	0.0000	0.11	0.11	0.3172
Compliance with police	-0.17	0.10	0.0907	0.27	0.12	0.0297	-0.10	0.10	0.2990
General satisfaction	-0.68	0.12	0.0000	0.48	0.12	0.0001	0.03	0.10	0.7678
Police-community relations	-0.45	0.14	0.0009	0.52	0.14	0.0002	0.13	0.11	0.2412
Cooperation with police	-0.16	0.10	0.0959	0.32	0.11	0.0034	0.03	0.08	0.6693
Compliance with the law	-0.27	0.08	0.0010	0.19	0.08	0.0157	-0.13	0.07	0.0730

Notes: N = 694 respondents; K = 112 clusters (street segments). Each row is a separate regression model, with control variables included but not shown. Models preserve the fully crossed design with an interaction between encounter type and body worn camera presence, but only the marginal effects of the treatment levels are shown. Standard errors are cluster robust, and p-values are two-sided. A Bonferroni adjustment for 30 tests (not shown) yields a 5% significance level of 0.0017.

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Appendix G

Factorial Analysis of Variance Including Post-Floyd Dummy Variable

Outcome variable	Post-Floyd × Encounter type		Post-Floyd × Body worn camera		Post-Floyd × Encounter × BWC	
	F	p	F	p	F	p
Procedural justice	7.17	0.0012	0.81	0.3710	4.60	0.0120
Police effectiveness	3.82	0.0249	2.07	0.1535	3.55	0.0320
Police lawfulness	4.01	0.0208	0.46	0.4980	5.36	0.0060
Police legitimacy	2.87	0.0607	1.73	0.1909	2.25	0.1101
Encounter satisfaction	2.41	0.0945	1.19	0.2775	2.89	0.0596
Compliance with police	4.71	0.0109	4.89	0.0291	2.22	0.1139
General satisfaction	4.18	0.0178	2.39	0.1247	1.99	0.1420
Police-community relations	0.80	0.4531	6.78	0.0104	2.06	0.1322
Cooperation with police	0.15	0.8588	6.70	0.0109	5.29	0.0064
Compliance with the law	0.66	0.5211	1.55	0.2159	1.39	0.2541
Stacked: Overall effect	3.75	0.0266	4.28	0.0408	4.46	0.0136
Stacked: Variable effects	2.32	0.0040	0.90	0.5279	1.01	0.4494

Notes: N = 750 respondents; K = 113 clusters (street segments). In the main panel, each row is a separate factorial ANOVA following linear regression with adjustment for the number of clusters. In the bottom panel, all ten outcomes are stacked and then a single ANOVA is estimated to investigate the overall effect across outcomes and the degree of variability in effects across outcomes. Prior to stacking, the outcomes are y-standardized to put them on a common scale.

Recorded Justice or Procedural Justice?

Appendix H

Linear Regression Models Comparing Pre- and Post-Floyd Marginal Effects

Outcome variable	Procedurally unjust vs. neutral		Procedurally just vs. neutral		BWC present vs. absent	
	Pre-Floyd	Post-Floyd	Pre-Floyd	Post-Floyd	Pre-Floyd	Post-Floyd
Procedural justice	-1.17 ***	-0.97 ***	0.19	0.77 ***	-0.06	0.06
Police effectiveness	-1.11 ***	-1.12 ***	0.15	0.60 ***	-0.09	0.15 ^
Police lawfulness	-0.71 ***	-0.71 ***	0.11	0.54 ***	-0.04	0.07
Police legitimacy	-0.88 ***	-0.51 ***	0.05	0.54 ***	-0.05	0.18 ^
Encounter satisfaction	-0.89 ***	-0.95 ***	0.40 *	0.77 ***	0.04	0.25 *
Compliance with police	-0.42 **	0.01	-0.10	0.54 ***	-0.23 *	0.15
General satisfaction	-0.95 ***	-0.56 ***	0.08	0.76 ***	-0.05	0.24 *
Police-community relations	-0.55 *	-0.45 **	0.33 ^	0.63 ***	-0.14	0.40 **
Cooperation with police	-0.20	-0.12	0.34 *	0.45 ***	-0.13	0.28 *
Compliance with the law	-0.41 **	-0.25 ^	0.08	0.27 **	-0.18	0.02

Notes: N = 750 respondents; K = 113 clusters (street segments). Each row is a separate regression model. Models include a three-way interaction between a post-Floyd dummy indicator, encounter type, and body worn camera presence. Only the marginal effects of the treatment levels for pre- and post-Floyd time periods are shown. Standard errors are cluster robust, and p-values are two-sided.

^ p < 0.10; * p < 0.05; ** p < 0.01; *** p < 0.001 (two-sided tests).