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Evaluating the Relative Impact of Positive and Negative Encounters with Police: A Randomized Experiment

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EVALUATING THE RELATIVE IMPACT OF POSITIVE AND NEGATIVE ENCOUNTERS WITH POLICE: A RANDOMIZED EXPERIMENT

ABSTRACT

Objectives: Examines the influence of positive, negative, and neutral police behavior during traffic stops on citizen perceptions of police.

Methods: Participants were randomly assigned to view a video clip of a simulated traffic stop in which the officer communicates with the driver in a positive (procedurally just), negative (procedurally unjust), or neutral manner. After viewing the video, participants completed a survey about their perceptions of police, including their level of trust in police, obligation to obey police orders, and willingness to cooperate with police.

Results: Observing positive interactions with police enhanced people's self-reported willingness to cooperate with police, obligation to obey police and the law, and trust and confidence in police, whereas observing negative interactions undermined these outcomes. The effects of these interactions were much stronger for encounter-specific outcomes than for more general outcomes.

Conclusions: The results from this randomized experiment confirm that procedural justice can enhance people's prosocial attitudes toward police, whereas procedural injustice can undermine these attitudes. While positive (procedurally just) interactions tend to have weaker effects than negative (procedurally unjust) interactions, this study finds little support for the notion that only negative experiences shape people's views about the police.

INTRODUCTION

Communication between police officers and the public is often regarded as one of the core technologies of policing (Manning 1996; Sklansky 2011).[1] The recent explosion of research on procedural justice and legitimacy in criminology reinforces the idea that how police officers talk to people matters (Gau and Brunson 2010; Tyler 2006; Tyler and Huo 2002). Research suggests that people's perceptions of procedural justice during interactions with police have a significant influence on their trust and confidence in the police, their perceived obligation to obey the police (and the law more generally), and their willingness to cooperate with the police. This body of research is becoming increasingly influential among policy makers. For instance, the President's Task Force on 21st Century Policing (2015: 6) in the United States recently issued a series of recommendations endorsing the importance of procedural justice, characterizing it as "a foundational necessity in building public trust."

Although existing research has helped to clarify the nature and effects of procedural justice, numerous questions remain to be answered. This study seeks to make two contributions to the research on procedural justice and its effects. First, questions have been raised about the relative effects of negative and positive encounters with police. Some research suggests that the effects of negative (procedurally unjust) encounters may be stronger than the effects of positive (procedurally just) encounters (Skogan 2006). Studies on whether negative and positive encounters have asymmetric effects have reached contradictory findings and the question remains unresolved (Bradford et al. 2009; Skogan 2012). Thus, we seek to test the relative effects of positive and negative encounters with police on people's trust and confidence in police, obligation to obey the police, and willingness to cooperate with police.

Second, most of the research on procedural justice and legitimacy relies on multivariate analyses of cross-sectional survey data, making it difficult to draw causal inferences. To address this concern, scholars have begun to rely on randomized field trials to test the effects of procedural justice interventions, usually in the context of police traffic stops (MacQueen and Bradford 2015; Mazerolle et al. 2013; Sahin 2014). Randomized field trials are ideal for testing the effects of *positive* interventions such as having police officers talk to people in a helpful, thoughtful, or procedurally just manner. However, they are unsuitable for testing the effects of *negative* interventions such as having police officers talk to people in a rude, disrespectful, or threatening manner. Police leaders would be unlikely to endorse any study design in which their officers are required to behave in a negative manner toward the public.

While randomized *field* trials may not be suitable to test the relative effects of negative and positive encounters with police, other types of experimental methods are well-suited for addressing this issue. Here we use a randomized laboratory-style experiment to test the effects of positive, negative, and neutral encounters with police in a mock traffic stop. Research participants viewed a brief video of a simulated traffic stop that appears to be shot from the perspective of a police body camera. Participants were randomly assigned to view a video with either a positive (procedurally just) condition, a negative (procedurally unjust) condition, or a neutral (control) condition. Since participants observed someone else being stopped by police, the study examined the effects of vicarious exposure to encounters with police. This methodology enabled us to test the hypothesis that the effects of positive and negative vicarious encounters with police are asymmetric.

BACKGROUND

A large body of research examines citizen perceptions of police, with a particular emphasis on understanding the antecedents and consequences of positive or negative attitudes toward police (e.g., Brandl et al. 1994; Frank et al. 2005; Gallagher et al. 2001; Maguire and Johnson 2010). Much of the recent scholarship in this area focuses on the role of procedural justice for fostering positive attitudes toward the police, as well as cooperation and compliance with legal authorities (e.g., Johnson et al. 2014; Sunshine and Tyler 2003; Tyler 2006; Tyler and Huo 2002). Based in social psychology, procedural justice theory emphasizes the primacy of fairness in the decision-making *process*, as opposed to fairness in the distribution of *outcomes* (Thibaut and Walker 1975). According to the group-value model of procedural justice, people place great importance on the feeling they derive from belonging to social groupings such as organizations or communities. Fair decision-making processes by authority figures convey a sense of an individual's worth within a group (Lind and Tyler 1988; Tyler 1989). Unfair treatment by authority figures may be perceived as a form of group exclusion or ostracism, sending the message that a person is not a valuable member of a group (Tyler and Lind 1992; van Prooijen et al. 2004).

Procedural justice is a central component of Tyler's process-based model of regulation, which serves as the intellectual foundation for much of the scholarship in this genre (Sunshine and Tyler 2003; Tyler 2006; Tyler and Huo 2002). In the process-based model, procedural justice is typically conceptualized as having two components: the quality of treatment and the quality of decision-making by authority figures. *Quality of treatment* focuses on the nature of the interpersonal interaction between an authority figure and an individual. For instance, police officers can choose to treat people in a polite and respectful manner, even when delivering a distasteful or disagreeable outcome (such as a citation or arrest). Tyler and Blader (2003: 351-

352) note that procedural justice is "strongly linked to quality of treatment issues, such as treating people with politeness and dignity in social interactions." *Quality of decision-making* focuses on the extent to which authority figures rely on fair and neutral processes in making decisions. For instance, if a police officer's decision to stop and search an individual is based on extra-legal factors like race, sex, or the type of clothing a person is wearing, then the public is likely to view the officer as behaving in a procedurally unjust manner (Gau and Brunson 2010).

According to Tyler's three-step model, people make judgments about the procedural justice of legal authorities, these judgments influence their general assessments of the legitimacy of the law and legal institutions, and these assessments, in turn, influence people's willingness to obey the law or comply with the directives of legal authorities (Sunshine and Tyler 2003; Tankebe 2009; Tyler 2006; Tyler and Fagan 2008; Tyler and Huo 2002). These normative process-based judgments are thought to be more influential than instrumental judgments about outcome favorability, police performance, or risk of sanctions in structuring people's sense of obligation to obey and their willingness to cooperate or comply with legal authorities (Sunshine and Tyler 2003; Tyler 2005). One of the implications of this body of theory and research is the notion that people who are arrested or cited by the police may be more likely to accept the decision and view the police as fair and impartial if officers treated them with dignity and respect and relied upon fair and unbiased decision-making procedures. By incorporating the principles of procedural justice into their interactions with the public, police can preserve or enhance their moral authority and seek to ensure that the people they encounter (including those who receive a disagreeable outcome like an arrest, citation, or use of force) are able to maintain their dignity. This approach will help to reduce the likelihood that in the aftermath of an encounter with police, people "will feel a sense of indignation, anger, or both" (Gau et al. 2012: 334).

Experimental Evidence

Although there is now a significant body of empirical research on procedural justice and its effects, much of it is based on cross-sectional data and correlational designs and therefore provides a weak basis for drawing inferences about cause and effect.[2] However, several recent studies have relied on randomized experiments to discern the effects of procedural justice interventions during police traffic stops.[3] For example, the Queensland Community Engagement Trial (QCET) in Australia tested the effects of a procedural justice intervention on drivers who were stopped by the police for random breath tests (Mazerolle et al. 2013). In the experimental condition, officers relied on a procedural justice script during their interactions with drivers. In the control condition, officers were instructed to follow standard police procedure. The study found that the intervention had a significant, positive, *direct* effect on encounter-specific perceptions of procedural justice during the stop. The intervention also had significant *indirect* effects on general perceptions of procedural justice, perceptions of police legitimacy, satisfaction with police, and cooperation with police through the encounter-specific perceptions (Mazerolle et al. 2012; Mazerolle et al. 2013). Other analyses of data from the QCET found that the intervention exerted a direct effect on a measure of encounter-specific trust and confidence and indirect effects (through trust and confidence) on willingness to cooperate and obligation to obey (Murphy, Mazerolle, & Bennett 2014).

A similar study tested the effects of a procedural justice intervention during routine traffic stops for speeding in Adana, Turkey (Sahin 2014). The study found that incorporating the principles of procedural justice into the language used by police officers during a traffic stop had a significant effect on drivers' perceptions of procedural justice during the encounter. However, the intervention did not have a significant effect on drivers' more general perceptions of police

procedural justice. Comments from participating drivers suggest that their general views of the police are "mainly shaped by accumulated individual police experiences" and therefore it may take more than a single procedurally just contact to alter their general attitudes toward police (Sahin 2014: 164-165).

Another randomized field experiment tested a procedural justice policing intervention in Scotland (MacQueen and Bradford 2015). Consistent with the methodology used by Mazerolle et al. (2013) and Sahin (2014), the researchers tested an intervention in which police officers incorporated key elements of procedural justice into their interactions with drivers during traffic stops. The officers also provided drivers with a leaflet that reinforced these same themes. The control condition consisted of standard "business as usual" policing practices (MacQueen and Bradford 2015: 425). Contrary to expectations, the researchers found that the procedural justice intervention reduced encounter-specific perceptions of procedural fairness and satisfaction with police among drivers, but had no effect on trust in the officer. Moreover, the procedural justice intervention had no effect on more generalized assessments of police fairness, effectiveness, or legitimacy. These results differ somewhat from those of the previous two randomized field trials. According to the authors, these unexpected findings "challenge the notion that public perceptions may be improved through a simple, additive approach to the delivery and communication of procedural justice" (MacQueen and Bradford 2015:419).

A more recent randomized laboratory-type experiment examined the effects of procedural justice in the context of a simulated traffic stop in the U.S. (Lowrey, Maguire, and Bennett 2016). Study participants watched a brief video of a simulated traffic stop recorded from the perspective of an officer's body-worn camera. Participants were randomly assigned to view a video featuring one of three experimental conditions: procedurally just, neutral (control), and

overaccommodative. The latter condition was based on the concept of overaccommodation from the study of linguistics and communications.[4] After watching the video, participants answered survey questions about their perceptions of the officer in the specific encounter as well as their more general attitudes toward police. Participants exposed to the procedural justice condition reported a greater feeling of obligation to obey the officer, and greater trust and confidence in the officer, relative to those exposed to the neutral (control) condition. The effects of overaccommodation on the encounter-specific outcomes were not significantly different from those of the neutral condition. Furthermore, none of the experimental conditions exerted significant effects on more general perceptions of police.

Finally, Barkworth and Murphy (2015) conducted a laboratory-style randomized experiment in which participants (first year university students) were exposed to vignettes describing a police traffic stop encounter. Participants were instructed to "imagine themselves being stopped by a police officer for exceeding the speed limit by 5km per hour" (p. 265). Participants were randomly assigned to either a procedural justice or a procedural injustice condition. The findings revealed that students exposed to the procedural injustice condition were significantly more likely to report experiencing negative affect, including feelings of frustration, anger, and anxiety. Students exposed to the procedural injustice condition were also significantly less likely to comply with the law. Barkworth and Murphy (2015) concluded that negative emotional responses mediate the effect of procedural justice on compliance. Based on this finding, they called for more research on the role of emotion and affect in research on procedural justice, legitimacy, and compliance.

To our knowledge, the experimental evidence on the effects of procedural justice during traffic stops consists of these five studies: three field trials and two laboratory-style experiments.

In four of these experiments, procedural justice interventions administered during actual or simulated traffic stops appear to have generated significant effects on people's judgments and perceptions about the police and the law. In three of these four studies, the evidence suggests that the interventions had different effects on participants' attitudes about the officers involved in a specific encounter than on their views about the police more generally (Barkworth and Murphy [2015] did not examine the difference between encounter-specific and general outcomes).

Taken together, these studies make an important contribution to the literature by improving internal validity and helping to delineate the effects of procedural justice and injustice. While the evidence from randomized *field* trials is useful for comparing the effects of procedural justice interaction styles with those of conventional interaction styles, the interventions that have been tested in these studies so far represent only one segment on a continuum of police behavior. The three field experiments we reviewed compared a control condition with little or no valence to a treatment condition with a positive valence; none of them tested the effects of a condition with a *negative* valence. While studying the effects of procedural *justice* is important, it is equally important to test the effects of procedural *injustice*. To date, only one experiment (Barkworth and Murphy's laboratory study) has examined the effects of procedurally unjust police behaviors, and none have addressed the relative effects of negative and positive encounters with police on outcomes like trust and confidence, obligation to obey the law, and willingness to cooperate or comply with authorities.

Contrasting the Effects of Positive and Negative Encounters

Although experimental research has not yet addressed this issue in depth, evidence from cross-sectional data suggests that positive and negative police encounters may have asymmetric effects. Using survey data from Chicago, Skogan (2006) compared the effects of positive and

negative police encounters on citizen confidence in police. Confidence in police was measured using survey items gauging respondents' perceptions about police effectiveness and community engagement.[5] Those respondents who had experienced a contact with the police in the last year were asked follow-up questions about their overall satisfaction with the police as well as police responsiveness, attentiveness, explanations, politeness, fairness, and helpfulness. The responses to these follow-up questions were used to classify each contact as either positive or negative. Contacts were classified as positive if half or more of these ratings were positive. If less than half of these ratings were positive, the encounter was classified as negative. Each encounter was also classified as either police-initiated or citizen-initiated. Cross-classifying these two variables resulted in four conditions: police-initiated positive or negative, and citizen-initiated positive or negative. Each condition was then compared against having no contact with police.

Using linear regression, Skogan found that the impact of a positive encounter (relative to no encounter) was not statistically significant; having a positive experience with police did not significantly impact citizen confidence in police. In contrast, a negative encounter had a significant (negative) impact on confidence, one that was four to fourteen times as great as that of a positive encounter. This asymmetry is in contradiction to much of the procedural justice literature, and suggests that even if police are polite, fair, attentive and helpful, these positive behaviors may not lead to increased confidence in the police. It may only be the negative encounters that make an impression. Skogan replicated this finding in seven additional locations: Great Britain (England and Wales), Indianapolis, New York, St. Petersburg (Florida), St. Petersburg (Russia), Seattle, and Washington, DC. These results are consistent with a more general "negativity bias" which has previously been reported in the psychology literature (Baumeister et al. 2001; Taylor 1991). Skogan's (2006: 99) findings led him to conclude that

"the police may get essentially no credit for delivering professional service, while bad experiences can deeply influence peoples' views of their performance and even legitimacy."

Using survey data from London, Bradford et al. (2009) tested Skogan's findings. Unlike Skogan (2006), they treated "public confidence" in police as consisting of three independent perceptual dimensions: effectiveness in dealing with crime, fairness, and community engagement. Consistent with Skogan, they divided encounters into four categories by cross classifying positive versus negative encounters and citizen-initiated versus police-initiated encounters. Their findings revealed that the impact of positive and negative encounters, whether citizen or police initiated, varied across these three dimensions. Perceptions of police effectiveness decreased for all four contact types (relative to no contact). Thus, any contact with police, whether positive or negative, reduced perceptions of police effectiveness. This finding is roughly consistent with Skogan's findings. However, the findings with regard to perceptions of fairness and community engagement were somewhat different. Respondents who experienced a positive self-initiated contact with police perceived police to be fairer and more engaged than those who experienced no contact with police. Respondents who experienced a negative contact, whether self-initiated or police-initiated, perceived police as less fair and less engaged relative to those who experienced no contact. Respondents who experienced a positive police-initiated contact perceived the same levels of fairness and community engagement as those who had no contact with police. Bradford and his colleagues concluded that "while opinions about police effectiveness may be challenged by any contact – whether it is satisfactory or unsatisfactory – ideas about fairness and community engagement appear to be amenable to change in either a positive or negative direction" (Bradford et al. 2009: 41).

Finally, using panel data from the UK, Myhill and Bradford (2011) explored the effects of pre-existing opinions about police on satisfaction with individual encounters and the effects of satisfaction on public confidence in police. They found that "those with low levels of confidence in the police were more likely to judge subsequent contacts negatively, while those with high levels of confidence were not more likely to judge them positively" (Myhill and Bradford 2011:10). Further, the researchers found that, holding confidence in police at Wave 1 constant, individual *police-initiated* unsatisfactory contacts significantly decreased confidence in police at Wave 2. Satisfactory police-initiated contacts, however, did not positively impact confidence. These findings are only partially consistent with Skogan's (2006) findings on the asymmetry between positive and negative contacts with police. In contrast to previous findings about asymmetry, satisfactory *citizen-initiated* contact showed a small but statistically significant and positive association with confidence in police.

The Present Study

Using a randomized experimental design, the current study examines the absolute and relative effects of procedural justice and procedural injustice on trust and confidence in police, obligation to obey police and the law, and willingness to cooperate with the police.[6] Participants were randomly assigned to view one of three video clips containing footage of a simulated traffic stop, and then completed a survey about their views of the police and the law. The present study builds upon the previous research reviewed above and expands our knowledge in several ways. For example, it is only the second experimental study to explore the effects of negative (or procedurally unjust) police behavior, and the first to examine the *relative* effects of positive and negative encounters with police using experimental methods. Moreover, this study is one of only

two that incorporates a neutral condition into the experimental design to allow for a more nuanced exploration of the effects of procedural justice and injustice. In addition, this is only the second randomized experiment conducted in the United States, thus expanding on the external validity of this line of experimental research.

METHODOLOGY

Participants

Participants were college students in undergraduate criminology and criminal justice classes at American University (AU), a private institution in Washington, DC, and George Mason University (Mason) in Fairfax, VA, the largest public university in the state. Respondents received credit toward their class participation grade for participating in the study. The experiment was administered in November and December, 2015. In total, 546 students participated (178 from AU and 368 from Mason). The analyses reported here are based on a subsample of 266 respondents (87 from AU and 179 from Mason).[7] Table 1 summarizes the demographic characteristics of the samples from both universities as well as the full sample, including age, sex, race, ethnicity, and birthplace. The Mason sample is more diverse in terms of race and age than the AU sample, which is a pattern that generally reflects the differences between the two universities. Although this college sample is more educated than the U.S. population, the use of a student sample is appropriate given the focus of this study, which is to assess whether attitudes toward police and the law vary *across* the experimental conditions (and not, for example, to measure the absolute level of cooperation or trust in police expressed by the respondents).

[Insert Table 1 about here]

Procedures

Each participant was randomly assigned to either the positive (procedural justice), negative (procedural injustice), or neutral (control) condition, watched the corresponding video clip, and completed an online survey using Qualtrics survey software. Sample sizes for the three conditions were: positive (n=90), negative (n=90), and neutral (n=86). Survey questions focused on respondents' perceptions of the degree of respect, bias, and citizen voice in the encounter; their trust, obligation to obey, and willingness to cooperate with the officer in the video; and their trust, obligation to obey, and willingness to cooperate with the police more generally. Participants answered demographic questions and were given the opportunity to respond to an open-ended question inviting them to share any further thoughts about the video they had watched. Finally, participants were debriefed about the study's purpose.

Treatments

The three experimental conditions consisted of video footage of a mock traffic stop in which the officer treats the driver in either a positive (procedurally just), neutral, or negative (procedurally unjust) manner. Each video was shot from the perspective of a body-worn camera, showing the driver but not the officer, in order to limit any possible confounding effects of police officer characteristics, and to focus on the effects of *verbal* communication across the three treatment conditions. The driver in each video is a teenage white male whose dialogue is minimal and does not vary across conditions. The role of the officer was played by a researcher with previous experience working as a police officer.

The basic procedure for each traffic stop video is the same, and begins with the officer approaching a stopped vehicle, speaking to the driver, obtaining the driver's license and registration, and then walking back toward his own vehicle. The video then cuts to the officer returning from his vehicle and issuing the driver a speeding ticket. Both the infraction and the punishment are held constant across the experimental conditions. In each case the officer informs the driver that he was speeding (48 miles per hour in a 30 mile per hour zone) and issues a citation.

In the neutral (control) video, the procedure merely consists of the officer telling the driver that he was speeding, requesting the driver's documentation, and then telling the driver he is being issued a ticket for speeding. The negative condition contains the same basic ingredients as the control video, but the officer speaks rudely to the driver. The officer opens the interaction by admonishing the driver for exceeding the speed limit ("Are you out of your damned mind driving like that? You were going 48 in a 30. What, are you trying to kill somebody?"). The officer then demands the driver's documents ("Give me your license and registration!"). When the officer returns, he issues the speeding ticket and tells the driver "You're lucky I don't arrest you for reckless driving!" The officer orders the driver to sign the ticket and the interaction ends with the officer saying: "Now get out of here, I better never see you driving in this neighborhood like that again."

The positive condition incorporates the key aspects of procedural justice (Sunshine and Tyler 2003). Upon approaching the vehicle the officer greets the driver, introduces himself by name, politely asks for the driver's documents ("May I have your license and registration, please, sir?"), and thanks the driver for providing the requested documents. After returning from his vehicle the officer issues the ticket, explains that the instructions are on the back of the ticket,

warns the driver that the fine will double if it is not paid, and asks the driver if he has any questions. The interaction ends with the officer explaining the importance of road safety and asking the driver to drive carefully: "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." Elements of procedural justice in this interaction include polite dialogue (respect/fair treatment), an invitation for the driver to ask questions (citizen voice/fair treatment), and an explanation for why the officer issued the ticket (fair decision-making, trustworthy motives).

It is worth emphasizing that study participants were exposed to a *vicarious* contact with police, not a direct contact. Much of the procedural justice literature focuses on people's direct experiences with authority figures. However, research evidence suggests that vicarious exposure to police can also have powerful effects on people's attitudes and behaviors (e.g., Augustyn, 2016; Rosenbaum et al., 2005; Weitzer & Brunson, 2009). For instance, Rosenbaum and his colleagues (2005) found that vicarious experiences with police, whether positive or negative, had stronger effects than direct contacts on attitudes toward police. In spite of the seeming importance of vicarious experiences with police, Augustyn (2016: 261) notes that the effects of vicarious contacts on perceptions of law and legal authorities are "vastly understudied relative to direct contacts." Therefore this study helps to fill an important gap in the research.

Measures

We measured six dimensions of respondents' views toward police and the law, including three at the encounter-specific level and three at a more general or global level (Brandl, et al., 1994). The encounter-specific measures focus on respondents' feelings about the officer in the video they watched, including their willingness to cooperate with the officer, obligation to obey the officer, and trust and confidence in the officer. The more general questions focus on respondents' global views toward police and the law, including their willingness to cooperate with police, obligation to obey the police and the law, and trust and confidence in the police. Each outcome was treated as a latent variable and measured using multiple indicators. Confirmatory factor analysis (CFA) was used to estimate a measurement model linking the six latent variables with 20 indicators thought to reflect the underlying concepts. The use of CFA was intended to improve construct validity and reduce the influence of measurement error, which often goes undiagnosed in research on procedural justice (Gau, 2014; Johnson, Maguire, and Kuhns, 2014; Maguire and Johnson, 2010). The measurement model fit the data well according to multiple fit indices (RMSEA=.06; CFI=.994; TLI=.992; WRMR=0.68).[8] All indicators are listed in Table 2 together with their means and CFA factor loadings for the full sample. The loadings are uniformly strong and positive, ranging from approximately .72 to .97, with a mean of .89 and a median of.93. The six latent variables are all reliably measured; composite reliabilities range from .84 to .94, with a mean and median of .89.[9]

[Insert Table 2 about here]

Analyses

The three experimental groups represent the positive (procedural justice), negative (procedural injustice), and neutral (control) conditions. We first performed manipulation checks,

which confirmed that the procedural justice treatment conditions influenced participants' perceptions of procedural justice during the encounter in the expected manner. We computed a procedural justice scale based on participants' responses to nine survey questions intended to measure different facets of procedural justice (α=.949). Scores on each item ranged from 1 to 5 for a total possible scale score of 9 to 45 (higher scores represent more procedural justice). Mean scale scores for the three treatment conditions (negative=21.3, neutral=31.7, and positive=39.7) varied in the expected direction and were statistically significant (F=220.0; df=2; p<.000).

We also conducted balance tests to ensure that the randomization procedure was effective in generating balanced groups that did not exhibit significant differences in meaningful covariates thought to be associated with the outcomes. Previous studies have found that demographic characteristics influence people's view of police and the law (Engel 2005; Hurst and Frank 2000; Jesilow et al. 1995; Taylor et al. 2001; Tuch and Weitzer 1997; Webb and Marshall 1995). We conducted balance tests for five demographic variables: age (in years), sex (female/intersex=0, male=1), race (nonwhite=0, white=1), ethnicity (non-Hispanic=0, Hispanic=1), and birthplace (foreign born=0, U.S. born =1). The results of these tests revealed no statistically significant differences between groups for four of the five demographic covariates, including age (F=0.38, p>.05), race (F=0.54, p>.05), ethnicity (F=2.36, p>.05), and birthplace (F=1.67, p>.05). However, the groups did differ significantly with regard to sex (F=6.75, p<.001). In the negative condition, 36.5% of respondents were male, compared with 22.2% in the neutral condition and 48.9% in the positive condition. These differences in percent male across the three experimental conditions constitute a form of randomization failure that must be controlled for statistically in the multivariate models that follow.[10]

The principal analytical task was to estimate the effect of dummy variables representing each experimental condition on the six latent outcome measures. Since including all three dummy variables in the model simultaneously would introduce perfect multicollinearity, we estimated the model for each set of outcomes twice, treating the negative condition and then the neutral condition as the reference category. This approach enabled us to examine all possible contrasts between the three experimental conditions.

Given the results of the balance tests, the final models to be estimated included sex (% male) as a covariate in addition to the two dummy variables representing the experimental conditions (a third dummy variable represents the reference category and is therefore excluded). The inclusion of sex as a covariate was meant to control for the unequal distribution of men across the experimental conditions. To be cautious, we also included the other four demographic variables as covariates in each model to account for the possibility that minor demographic differences between groups may influence the treatment estimates.[11] Structural equation models were estimated to test the effects of the treatment dummy variables and the covariates on the latent variables representing the three encounter-specific outcomes and the three global outcomes. Structural equation modeling provides a number of benefits for the analysis of data from randomized experiments, including improved measurement of outcomes and "more powerful tests of intervention effects" (Russell et al. 1998: 28; also see Bagozzi & Yi 1989 and Hancock 2004).

Figures 1A and 1B illustrate the general form of the structural equation models being tested (although the covariates were included as separate measures in the models, they are represented in Figures 1A and 1B by a single rectangle to enhance visual clarity). Consistent with conventional practice in diagramming structural equation models, rectangles represent

observed variables and ellipses represent latent variables. Curved two-headed arrows represent correlations, and straight arrows represent causal effects. The straight arrows that link the observed exogenous variables to the latent outcomes represent the structural (regression) part of the model. The straight arrows linking the latent outcomes to their observed indicators represent part of the measurement (confirmatory factor analysis) model. The short arrows pointing from right to left into the latent outcomes and their indicators represent error terms.

-- INSERT FIGURES 1A AND 1B ABOUT HERE--

Because the indicators used to measure each latent outcome variable are ordinal variables ranging in value from 1 to 5, we estimated the models using a robust weighted least squares estimator (WLSMV). Monte Carlo simulation research has shown that the WLSMV estimator performs well for measurement models with categorical indicators (Flora and Curran 2004; DiStefano and Morgan 2015; Muthén et al. 1997). The sample used here is somewhat small relative to the complexity of the models being tested (the full sample size is 266, but due to missing data, the sample size in the multivariate models drops to 254). Asparouhov and Muthén (2010: 31) note that "WLSMV methods are based on and designed for large sample size and have no guarantee to work well in small sample size." Thus we also estimated the models with a Bayesian estimator in Mplus using the Markov Chain Monte Carlo algorithm. Simulation research has found that this estimator has good small-sample performance relative to other estimation procedures, particularly for CFA models with ordinal outcomes (Asparouhov and Muthén, 2010; Liang and Yang, 2014). An additional benefit is that the Bayesian approach is a full-information estimator; simulation research shows that it performs better with missing data than WLSMV, which relies on pairwise estimation (Asparouhov and Muthén, 2010). The use of both estimation methods allows for a more complete understanding of the models.

FINDINGS

We began our analysis by estimating the effects of the experimental conditions on the three encounter-specific and three global outcomes. Table 3 contains partially standardized regression coefficients for all of the models estimated using WLSMV based on three contrasts: positive vs. neutral, negative vs. neutral, and positive vs. negative. The regression estimates are conditioned on the five covariates described earlier (age, sex, race, ethnicity, and birthplace) to account for demographic differences across groups.[12] The inclusion of covariates is intended to reduce error variance and enhance statistical power (Russell, et al., 1998). Because the regression coefficients are standardized on the outcome variables, they represent the difference in means (in standard deviation units) between the treatment conditions, adjusting for the effects of the covariates. Therefore they are interpretable as standardized mean difference effect sizes and are analogous to Cohen's d. The models used to generate the estimates fit the data well according to several fit statistics.[13]

The results for the encounter-specific outcomes shown in Table 3 reveal that participants exposed to the procedural justice condition are significantly more willing to cooperate with the officer, feel more obliged to obey the officer's directives, and have more trust and confidence in the officer than participants exposed to the neutral or negative condition. In contrast, those exposed to the negative condition are significantly less willing to cooperate with the officer, feel less obliged to obey the officer's directives, and report lower trust and confidence in the officer than participants exposed to the neutral or positive condition. At the encounter-specific level, these findings provide solid support for procedural justice theory.

The results for the global outcomes shown in Table 3 are less consistent than those for the encounter-specific outcomes. Scores on the three global outcome measures do not differ significantly between participants exposed to the procedural justice condition and those exposed

to the neutral condition. However, participants exposed to the procedural justice condition are significantly more willing to cooperate with the police, feel more obligated to obey the police and the law, and have more trust and confidence in the police than participants exposed to the negative condition. Finally, participants exposed to the negative condition are significantly less willing to cooperate with the police than those who were exposed to the neutral condition. However, participants exposed to the negative condition do not differ significantly from those exposed to the neutral condition with regard to either obligation to obey the police and the law or trust and confidence in the police. These findings provide partial support for procedural justice theory, but the results are less consistent than those for the encounter-specific outcomes.

[Insert Table 3 about here]

Table 4 contains partially standardized regression coefficients based on Bayesian estimates for the same three contrasts shown in Table 3.[14] As with Table 3, these coefficients are interpretable as standardized mean difference effect sizes conditioned on the effects of the covariates. At the encounter-specific level, the Bayesian estimates are very close to the WLSMV estimates. The substantive inferences to be drawn from both sets of estimates are identical. At the general level, the findings are largely the same with just one exception. The WLSMV estimates suggest that participants exposed to the negative condition do not differ significantly from those exposed to the neutral condition with regard to obligation to obey the police and the law. In contrast, the Bayesian estimates suggest that participants exposed to the negative condition feel significantly less obligated to obey the police and the law than those exposed to the neutral condition. Overall, the findings from the Bayesian models are largely consistent with those from the WLSMV models. At the encounter-specific level, the findings provide strong

support for procedural justice theory. At the global level, the findings provide partial but less consistent support for the theory.

[Insert Table 4 about here]

To explore the relative effects of positive and negative police encounters, we then compared the effect sizes across the experimental conditions. We found partial support for the idea that positive and negative encounters have asymmetric effects. In the WLSMV results (reported in Table 3), the mean effect size for the negative encounters (relative to neutral encounters) was about 1.2 times as large as the mean effect size for the positive encounters (here we refer to the absolute value of the effect sizes, ignoring their signs and focusing only on their magnitudes). However, it is worth noting that for two of the outcomes (encounter-specific willingness to cooperate and obligation to obey), the absolute value of the effect sizes for the positive treatment condition were *larger* than those for the negative condition. In the Bayesian results (reported in Table 4), the mean effect size for the negative encounters was also about 1.2 times as large as the mean effect size for the positive encounters (once again, two of the six positive effect sizes were larger in absolute value than the negative effect sizes). These ratios suggest that the effects of negative encounters are slightly stronger than the effects of positive encounters. However, as we will demonstrate shortly, they do not tell the whole story.

To further clarify the relative effects of negative and positive encounters, we examined their effects separately for encounter-specific outcomes and global outcomes. For encounter-specific outcomes, the mean ratio of the effect sizes for negative and positive encounters was approximately 1.0 in the WLSMV model and 0.9 in the Bayesian model; negative encounters produced similar or slightly weaker effects than positive encounters. Procedural justice and injustice appear to have opposite effects of roughly similar magnitude on the encounter-specific

outcomes examined in this study. For global outcomes, however, the ratios of the effect sizes for the negative and positive treatment conditions are larger: 2.0 in the WLSMV model and 2.6 in the Bayesian model. The coefficients for the negative condition (relative to the neutral condition) are uniformly larger in magnitude than those for the positive condition. These results suggest that the asymmetry between negative and positive effects may be more pronounced with regard to global outcomes than encounter-specific outcomes.

DISCUSSION

Using an experimental design, this study examined the absolute and relative effects of observing procedurally just and procedurally unjust police behavior during a simulated traffic stop on respondents' willingness to assist police, trust police, and obey the police and the law. We found that the positive and negative treatments examined in this study exerted a more consistent influence on encounter-specific outcomes than on global outcomes. This is not surprising given that the encounter-specific outcomes are proximate to the encounter whereas the global outcomes are more distal.[15] When compared with the neutral condition, exposure to the procedural justice condition enhanced people's encounter-specific willingness to cooperate, obligation to obey, and trust and confidence in police. These effects were magnified when comparing the positive (procedurally just) condition to the negative (procedurally unjust) condition. When compared with the neutral condition, exposure to the negative condition reduced people's encounter-specific willingness to cooperate, obligation to obey, and trust and confidence. In short, these findings suggest that the way a police officer talks to people during an encounter influences the extent to which people are willing to cooperate with the officer, feel a duty to obey the officer, or trust the officer. Police officers may be able to enhance cooperation,

compliance, trust, and confidence during encounters with the public by treating people in a procedurally just manner.

Some of these same patterns were also evident with regard to the global outcomes, though the effects were less consistent. Whereas all nine of the coefficients for the encounter-specific outcomes (three outcomes * three contrasts) were statistically significant, only four of the nine coefficients for the global outcomes were significant (a fifth coefficient was significant in the Bayesian estimates). When compared with the neutral condition, the effects of exposure to the positive condition on global outcomes were uniformly small and not statistically significant. Thus, when a police officer behaves in a procedurally just manner rather than a neutral manner, it may add value with regard to encounter-specific outcomes, but it appears insufficient to alter more general views of police and the law. However, when compared with the negative condition, the effects of exposure to the positive condition were larger and were significantly different from zero. Relative to the negative condition, the display of procedural justice in the positive condition enhanced people's willingness to cooperate with police, their obligation to obey police and the law, and their trust and confidence in the police at a global level.

With regard to the global outcomes, the effects of exposure to the negative condition relative to the neutral condition were more ambiguous. According to the WLSMV estimates, only one of the three global outcomes (willingness to cooperate) was significantly different from zero. According to the Bayesian estimates, two of the global outcomes (willingness to cooperate and obligation to obey) were significant. The coefficients associated with these two outcomes were both negative, suggesting that relative to a neutral encounter, people who are exposed to a negative encounter are less willing to cooperate with police and feel less of an obligation to obey the police and the law.

One clear pattern that emerges from this study is that the treatments produced much larger effects for encounter specific outcomes than for global outcomes. In both the WLSMV and Bayesian results, for instance, the mean absolute value of the effect sizes (partially standardized regression coefficients) for the encounter-specific outcomes was approximately 3.1 times as large as the mean absolute value of the effect sizes for the general outcomes (see Tables 3 and 4). Observing a single police-citizen encounter can exert a strong influence over people's views of the specific officer involved in the encounter, including their willingness to cooperate with, obey, and trust the officer. However, the influence of a single encounter on people's more general perspectives on law and legal authorities is less pronounced. The effects of procedural justice on both proximate and distal outcomes are not yet well understood, though an emerging body of experimental research is now beginning to clarify these effects (Lowrey, Maguire, and Bennett, 2016; MacQueen and Bradford 2015; Mazerolle et al. 2013; Sahin 2014).

The Relative Effects of Positive and Negative Police Encounters

The findings from this study are uniquely suited for drawing inferences about the relative effects of exposure to negative and positive encounters with police. As noted earlier, existing research on this issue has reported contradictory findings (Bradford et al. 2009; Myhill and Bradford 2011; Skogan 2006). Skogan (2006) found that positive and negative encounters have asymmetric effects on confidence in the police. In Skogan's study, the effects of negative encounters on general police attitudes were four to fourteen times as large as the effects of positive encounters. Here, we find some support for the idea that positive and negative encounters have asymmetric effects, but the magnitudes of these effects appear to be much smaller than those reported by Skogan (2006). Our results suggest that the effects of observing

negative encounters are slightly stronger than the effects of observing positive encounters (the ratio of negative effects to positive effects is approximately 1.2 in both the WLSMV and Bayesian models). However, we also find that the asymmetry between negative and positive effects is more pronounced for the global outcomes than for the encounter-specific outcomes. Based on a randomized experimental design, we find that the effects of observing negative encounters on global outcomes are a little more than twice as large as the effects of observing positive encounters (2.0 in the WLSMV model and 2.6 in the Bayesian model). In contrast, for the encounter-specific outcomes, the effects of the positive and negative encounters have approximately equivalent magnitudes (1.0 in the WLSMV model and 0.9 in the Bayesian model).

One way to interpret these findings is that when evaluating a specific police-citizen encounter, people's views toward the officer may be equally responsive to procedurally just and unjust treatment. Negative encounters reduce people's willingness to cooperate with the officer, their obligation to obey the officer, and their trust and confidence in the officer; positive encounters have opposite effects of similar magnitude on these outcomes. The asymmetry in the effects of positive and negative treatment may not occur at the encounter-specific level, at least as a result of vicarious experiences with police. However, when evaluating a police-citizen encounter, people's *general* views of the police and the law appear to be much more responsive to negative (procedurally unjust) treatment than to positive (procedurally just) treatment. Skogan (2006) found that negative encounters had significantly larger effects than positive encounters. It is worth emphasizing that his findings were based on global outcomes and not on encounter-specific outcomes. Thus, at the level of global outcomes, our findings are consistent with Skogan's asymmetry thesis.[16]

These asymmetric effects are also consistent with a more general body of research and theory on the effects of negative, neutral, and positive events. As Taylor (1991: 67) notes "…other things being equal, negative events appear to elicit more physiological, affective, cognitive, and behavioral activity and prompt more cognitive analysis than neutral or positive events." For instance, in the study of impression formation, negative information contributes much more heavily than positive information to people's evaluations. In the psychology literature, this phenomenon is referred to as either a *negativity bias* or a *positive-negative asymmetry effect* (Baumeister, et al. 2001; Peeters and Czapiniski 1990). Understanding the various ways that people respond to negative and positive encounters with authority figures, both psychologically and physiologically, represents an important new frontier in procedural justice scholarship.

Implications for Theory and Policy

The results of this study are consistent with theoretical perspectives on the role of police and other legal authorities in promoting citizen trust, improving compliance with the law, and increasing public cooperation by incorporating key elements of procedural justice into their encounters with the public. When individual police officers treat citizens with fairness and respect and make decisions that are free from bias, people are more trusting of those officers, more willing to obey their directives, and more willing to assist those officers in their duties. Moreover, in contrast to negative police-citizen interactions characterized by rude language and an abrupt or hostile officer demeanor, the use of a procedurally just approach can engender more positive views of police at a more global level as well.

Our findings are also consistent with the policy and training recommendations of recent high-profile national initiatives in the United States, such as the President's Task Force on 21st Century Policing and the Department of Justice's National Initiative for Building Community Trust and Justice. As noted in the final report of the President's Task Force (2015: 9): "law enforcement agencies should adopt procedural justice as *the guiding principle* for internal and external policies and practices to guide their interactions with rank and file officers and with the citizens they serve" (emphasis added). Indeed, as noted in a recent review by Tyler and his colleagues (2015), the scholarly literature on procedural justice has had a profound impact on how law enforcement leaders and policymakers think about policing. To the extent that police agencies adopt procedural justice training and incorporate elements of fair treatment and fair decision-making into their interactions with the public, they should see an increase in citizen assistance, cooperation, and compliance.

Directions for Future Research

Given the promise of procedural justice for enhancing public trust and confidence in police and inculcating law abiding behavior, more research on the influence of procedural justice (and injustice) on citizens' views of the police and the law is warranted. For example, little is known from experimental research about the extent to which race, ethnicity, and other characteristics moderate the effects of procedural justice or injustice. Thus, experimental research that varies the demographic characteristics of the officer or the driver would provide important additional insight. Moreover, in light of research that shows profound racial differences in perceptions of and outcomes related to investigatory stops (e.g. Epp et al. 2014; Gelman, Fagan and Kiss 2007), experimental studies that expand beyond the context of vehicle

stops for traffic violations to examine a wider variety of police-initiated and citizen-initiated interactions would add to the scholarly literature and be more informative for police policy and practice.

Furthermore, we know very little about how the different components of procedural justice influence perceptions of police, citizen behavior, or other outcomes. For instance, which is more important for promoting positive views of the police or increasing police legitimacy – quality of decision making or quality of treatment? Do people place greater weight on one component or the other when deciding whether to help an officer with an investigation or to comply with an officer's requests? Experimental research that seeks to manipulate these two aspects of procedural justice independently could help improve the knowledge base on how people conceptualize procedural justice. Moreover, this type of detailed knowledge would be useful for developing more finely calibrated procedural justice interventions. Continuing to expand the methodological and theoretical boundaries of procedural justice scholarship will help to improve the knowledge-base and maximize the applicability of this knowledge for policy and practice.

Limitations

A few cautions about the limitations of this study are in order. First, the participants in this study were college students whose attitudes and experiences may differ from the general population. Future research should replicate this experimental design using larger and more representative probability samples that include respondents with a more diverse set of experiences and greater demographic variation (especially in terms of age, race, ethnicity, education, and social disadvantage). Second, as discussed previously, this study relied on a

simulated traffic stop involving a driver unknown to the respondent. It is possible that the artificial and vicarious nature of the interaction may have produced less pronounced effects than if the respondent had interacted personally with an officer. While public attitudes toward the police are shaped heavily by vicarious exposure to the police, the effects of direct and vicarious contacts may differ (Rosenbaum et al., 2005). Third, while we found some evidence of asymmetry in the effects of the positive and negative conditions consistent with past research, we cannot be sure that the valence of the positive (procedural justice) script used in this study was equivalent to the valence of the negative (procedurally unjust) script. Fourth, our study design does not allow us to make inferences about the duration of the experimental effects on trust, cooperation, and obligation. Further research is necessary to determine whether such effects are long-lasting or if they decay quickly. Fifth, our study design focused only on verbal communication. It is possible that nonverbal cues such as facial expressions, body language, or demeanor influence citizen perceptions as well. Finally, this study was administered at a time when police behavior was under significant scrutiny following a series of highly-publicized incidents across the United States, which may have had a priming effect. However, such a concern is tempered by the fact that our goal was not to evaluate the overall tenor of citizen attitudes toward the police, but to examine how perceptions of the police varied across these experimental conditions. That we found significant differences between the procedural justice and injustice conditions even during this period of heightened police scrutiny and public awareness is notable.

Despite these potential drawbacks, this experiment adds to the literature by providing greater clarity on the causal relationship between the use of procedural justice and procedural injustice during police-citizen interactions and outcomes such as increased trust in police, citizen

compliance with police directives and the law, and police-citizen partnerships to address crime. Moreover, the results of the study should provide additional encouragement to police leaders who are considering or who have adopted procedural justice initiatives in their training and operations.

Compliance with Ethical Standards

Conflict of Interest: The authors declare that they have no conflict of interest.

Ethical approval: All procedures performed in studies involving human participants were in accordance with the ethical standards of the institutional and/or national research committee and with the 1964 Helsinki declaration and its later amendments or comparable ethical standards. This article does not contain any studies with animals performed by any of the authors.

Informed consent: Informed consent was obtained from all individual participants included in the study.

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Table 1. *Demographic Characteristics as a Percentage of the Sample*

Characteristic	AU	Mason	Total
Age			
18-19	60.7	39.4	46.4
20-21	32.1	35.3	34.2
22-23	4.8	12.9	10.3
24-25	1.2	5.3	4.0
26+	1.2	7.1	5.1
Sex			
Male	22.6	42.9	36.2
Female	76.2	56.5	63.0
Intersex	1.2	0.6	0.8
Race			
White only	76.2	58.6	64.4
Black only	3.6	10.7	8.3
Asian or Pacific Islander only	6.0	13.6	11.1
Native American only	0.0	0.6	0.4
Other	8.3	9.5	9.1
Multiracial	6.0	7.1	6.7
Ethnicity			
Hispanic	16.7	19.4	18.5
Non-Hispanic	83.3	80.6	81.5
Birthplace			
Born in the U.S.	86.9	83.5	84.6
Born elsewhere	13.1	16.5	15.4

Note: Age presented in categorical form in Table 1 to clarify its distribution for readers. When included as a covariate in the regression models, age is a continuous variable measured in years.

Table 2. *Indicator Means and CFA Factor Loadings for Six Outcomes*

Outcome Level	Outcome Type	Indicator	Mean	λ
Encounter-Specific Outcomes	Willingness to Cooperate	v1. I would provide information to help this officer solve a crime.	3.88	.923
		v2. I would provide information to help this officer find a suspect.	3.80	.955
		v3. I would report suspicious activity to this officer.	3.62	.818
	Obligation to obey	v4. I would feel a moral obligation to obey this officer's commands.	3.85	.972
		v5. I would feel a moral obligation to do what this officer told me to do.	3.79	.940
		v6. I would feel a moral obligation to follow this officer's instructions.	3.87	.931
	Trust and confidence	v7. I would have confidence in this officer.	3.51	.930
		v8. I would count on this officer to do his job well.	3.73	.962
		v9. I would trust this officer.	3.53	.943
Global Outcomes	Willingness to Cooperate	v10. I would help the police if asked.	4.22	.886
		v11. I would call the police to report a crime.	4.36	.809
		v12. I would provide information to the police to help solve a crime.	4.18	.934
		v13. I would report suspicious activities to the police.	4.04	.787
	Obligation to obey	v14. I feel a moral obligation to follow the law, even if I don't agree with it.	3.95	.718
		v15. I feel a moral duty to obey the law.	4.26	.815
		v16. I feel a moral obligation to do what the police tell me to do, even if I disagree.	3.73	.794
		v17. I feel a moral duty to follow police orders.	4.04	.927
	Trust and confidence	v18. I have confidence in police	3.72	.924
		v19. Police are trustworthy	3.67	.928
		v20. Most police officers do their job well	3.94	.810

Note: Response options range from 1 to 5 (1=strongly disagree, 2=disagree, 3=neither agree nor disagree, 4=agree, 5=strongly agree); n=254.

Table 3. *Regression Results for all Models (WLSMV Estimates)*

Outcome Level	Outcome Type	Positive vs. Neutral	Negative vs. Neutral	Positive vs. Negative
Encounter-Specific Outcomes	Willingness to Cooperate	0.637***	-0.608***	1.258***
	Obligation to obey	0.520**	-0.474**	0.994***
	Trust and confidence	0.724***	-0.872***	1.595***
Global Outcomes	Willingness to Cooperate	0.174	-0.357*	0.533**
	Obligation to obey	0.056	-0.280	0.341*
	Trust and confidence	0.186	-0.203	0.386*

*Note: * p<.05 ** p<.01 *** p<.001; n=254*

Table 4. *Regression Results for all Models (Bayesian Estimates)*

Outcome Level	Outcome Type	Positive vs. Neutral	Negative vs. Neutral	Positive vs. Negative
Encounter-Specific Outcomes	Willingness to Cooperate	0.674***	-0.527***	1.205***
	Obligation to obey	0.518**	-0.434**	0.946***
	Trust and confidence	0.730***	-0.793***	1.524***
Global Outcomes	Willingness to Cooperate	0.175	-0.356*	0.513**
	Obligation to obey	0.022	-0.291*	0.299*
	Trust and confidence	0.134	-0.229	0.353*

*Note: * p<.05 ** p<.01 *** p<.001; n=254*

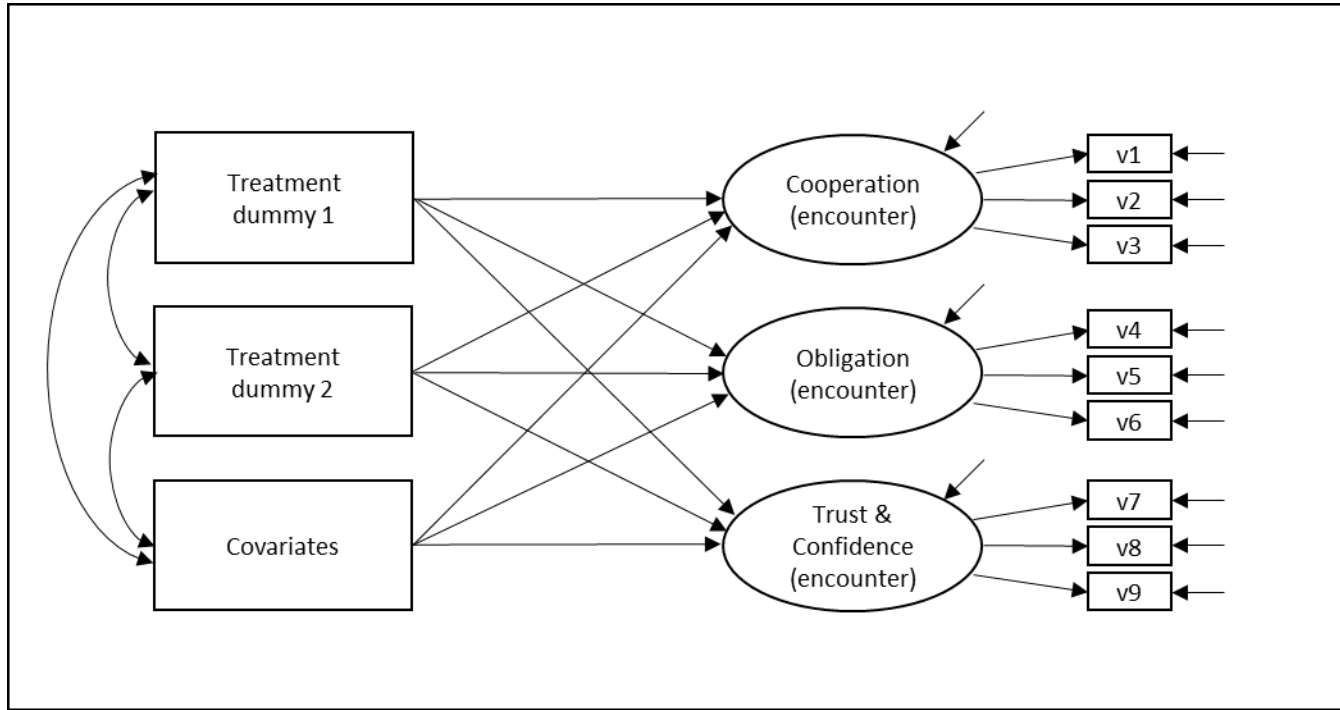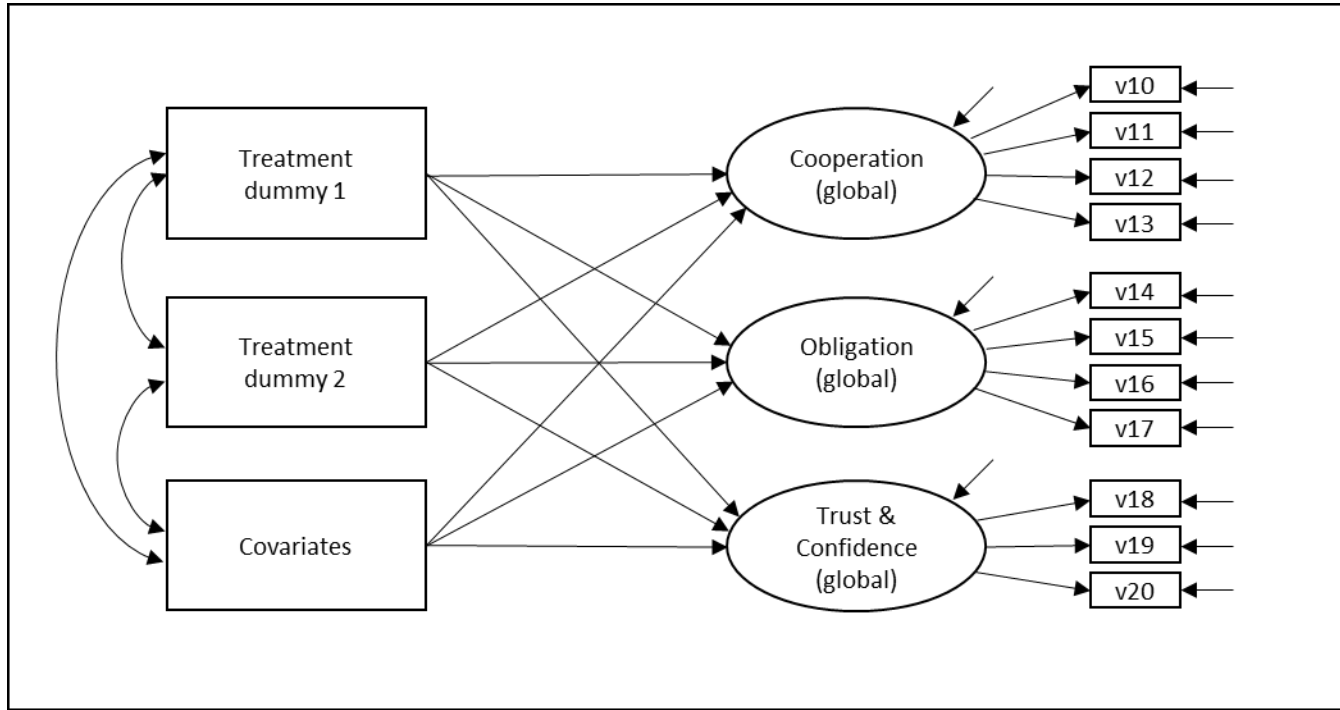**Figure 1a: Structural Equation Model for Encounter-Specific Outcomes**

Figure 1b: Structural Equation Model for Global Outcomes

ENDNOTES

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[1] For example, Manning (1996: 52) notes that the core technology of policing "remains people talking to people, officers trying to persuade people by various interactional strategies to comply with requests, threats, and commands…."

[2] Note that some studies have relied on the analysis of two-wave panel data to estimate the effects of procedural justice on a variety of outcomes (e.g., Beijersbergen, et al., 2015; Murphy, 2005; Tyler, 2006). The use of panel data typically offers stronger internal validity than correlational studies based on cross-sectional data, but weaker internal validity than studies relying on well-executed experimental or quasi-experimental designs (Worden & McLean, in press).

[3] There is also a growing body of experimental research on the effects of procedural justice interventions outside of criminology. For instance, Wenzel (2006) randomly allocated taxpayers to receive one of three reminder letters: a standard letter (which served as the control condition), and two others containing different elements of procedural justice. The reminder letters that incorporated procedural justice principles generated greater levels of tax compliance. Several field experiments have also tested the effects of procedural justice interventions on the attitudes, intentions and behaviors of employees in organizations (e.g., Hunton & Beeler, 1997; Schaubroeck, May, & Brown, 1994).

[4] Communication accommodation theory (CAT) posits that people subconsciously modify their speech patterns to match those of others with whom they are speaking. This communication accommodation, if well calibrated, can generate a number of benefits, including increased trust. However, overaccommodation can be perceived as disingenuous or artificial and can decrease trust (Lowrey, Maguire, and Bennett, 2016).

[5] Confidence in police was measured using questions on "police responsiveness to community concerns and whether police were dealing with problems that really concerned residents. There were also questions about 'how good a job' police were doing in preventing crime, keeping order and helping victims" (Skogan, 1996: 107).

[6] In Tyler's process-based model of regulation, obligation to obey is treated as the principal measure of institutional legitimacy. Thus, legitimacy is said to mediate the relationship between procedural justice and outcomes like cooperation and compliance. However, recent theoretical challenges call into question the meaning and measurement of legitimacy as specified by Tyler. For instance, Tankebe (2013) has articulated a model in which legitimacy is comprised of procedural justice, distributive justice, effectiveness, and lawfulness, and obligation to obey is treated as an outcome that is influenced by legitimacy. Given that the meaning and measurement of institutional legitimacy is currently under debate, we do not incorporate legitimacy as a construct in our model. Instead we focus on the major construct used to measure it in the work of Tyler and his colleagues: obligation to obey. Obligation to obey serves as a mediator between procedural justice and outcomes like cooperation and compliance in both Tyler's and Tankebe's models and therefore its effects are not part of the current debate.

[7] In addition to the three procedural justice conditions that are the focus of this study, the larger research project included additional experimental conditions that varied the demographic characteristics of the driver. These results are not reported here. The present study relies only on survey data from the 266 respondents who viewed a video featuring a teenage white male driver. Based on preliminary power analyses, we estimated that a minimum sample size of 159 would be necessary to detect a medium-sized effect (f=.25) with a power of .80 and an α level of

.05 (Cohen 1992). Our achieved sample size of 266 is likely sufficient for detecting medium and large effects, but insufficient to detect small effects.

[8] Our appraisals of model fit are informed by the following considerations. For the Root Mean Square Error of Approximation (RMSEA), Browne and Cudeck (1993) conclude that values of .06 to .08 constitute acceptable fit, while values of .01 to .06 constitute "close fit." Hu and Bentler (1999) also treat a RMSEA value of .06 as the upper threshold for close fit. For the Confirmatory Fit Index (CFI) and the Tucker-Lewis Index (TLI), Hu and Bentler (1999) suggest that values of .95 or greater indicate close fit. For the Weighted Root Square Mean Residual (WRMR), simulation evidence suggests that values below 1 are indicative of good fit (Yu, 2002).

[9] We estimated composite reliabilities using coefficient omega (Ω), which is based on the ratio of the true score variance to the total variance (McDonald, 1999; Raykov, 1997). Omega values for the encounter-specific outcomes were as follows: cooperation (Ω=.898), obligation (Ω=.940), and trust and confidence (Ω=.941). Omega values for the general outcomes were as follows: cooperation (Ω=.865), obligation (Ω=.838), and trust and confidence (Ω=.884).

[10] Participants were assigned to groups based on a randomization algorithm in Qualtrics that was not susceptible to intentional or unintentional manipulation. We do not have a ready explanation for the differences in group composition. Randomization is premised on the law of large numbers and sometimes fails in small samples. The most likely possibility in this case is that the differences between groups in the number of males compared to females/intersex resulted from having a relatively small sample.

[11] Respondents were allowed to mark more than one racial group when asked about their racial identity, and 6.7 percent of the sample did so. Multiracial respondents who selected white and

one or more other races were alternatively coded as either white or non-white, and regression models were run using both configurations. The coefficients and significance levels were virtually the same regardless of how these respondents were classified. The results presented here are based on the former classification (multiracial respondents who marked white as one of their racial identities were coded as white).

[12] Since these variables were only included as covariates to account for differences between groups rather than for substantive reasons, the coefficients are not reported. Of the 18 coefficients for percent male (six outcomes * three contrasts), only two were statistically significant. In both cases, male respondents were found to have greater levels of general trust in police than female and intersex respondents.

[13] Four models were used to generate the estimates reported in Table 3: one with encounter-specific outcomes and negative as the reference category; one with encounter-specific outcomes and neutral as the reference category; one with global outcomes and negative as the reference category; and one with global outcomes and neutral as the reference category. All four models fit the data well, with RMSEA values ranging from .051 to .052, CFI ranging from .990 to .996, TLI ranging from .986 to .994, and WRMR ranging from .525 to .685.

[14] Our Bayesian regression analysis relies on iterative Markov chain Monte Carlo (MCMC) algorithms to "obtain an approximation to the posterior distributions of the parameters from which the estimates are obtained" (Muthén 2010: 8). The estimates in Table 4 are partially standardized regression coefficients derived from the medians of the posterior distributions. The asterisks associated with the Bayesian estimates summarize the one-tailed p-values based on the posterior distributions.

[15] Here we use the term "proximate outcomes" to refer to phenomena that are near in time and scope to the encounter being evaluated. We use the term "distal outcomes" to refer to phenomena that are more distant in time and scope from the encounter being evaluated. The encounter-specific outcomes measured here are examples of proximate outcomes, whereas the global outcomes measured here are examples of distal outcomes. The procedural justice literature is replete with numerous other examples of proximate and distal outcomes.

[16] An anonymous reviewer noted that because this was a vicarious encounter, participants may have been uncertain about whether to respond to the encounter-specific items from their own personal perspective or from what they viewed as the driver's likely perspective. Due to this uncertainty, the reviewer suggested that some participants may have answered the encounter-specific items from the perspective of the driver but the general items from their own perspective. We intended for respondents to answer the encounter-specific items from their own perspective (not from the perspective of the driver), and took steps to encourage this approach when designing the experiment and survey instrument. For instance, we carefully considered the placement of these items in the survey, as well as the survey instructions to the respondent. For example, the instructions for the encounter-specific items read: "Thinking specifically about the police officer in the video, please indicate the extent to which you agree or disagree with the following statements." The goal here was to focus respondents on their own personal evaluation of the police officer, and not on the driver's likely perspective. In addition, we placed the encounter-specific items immediately after the items for the manipulation check, which focused on the respondents' assessment of the police officer's behavior (for example: "The officer was respectful.").