



Do police videos impact youths' willingness to cooperate with the police? Results from a national experiment

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Accepted: 27 June 2022

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Abstract

Objective Test how virtual, vicarious exposure to a procedurally just versus unjust police traffic stop impacts youths' perceptions of police legitimacy and willingness to cooperate.

Methods Adolescents ($N=822$) were randomly assigned to watch a video featuring a procedurally just interaction, a procedurally unjust interaction, or no video. Analyses examined the effects of video exposure on youths' views of police.

Results Virtual exposure did not impact youths' views of police legitimacy. However, youth were more willing to cooperate with the just versus the unjust officer. Interestingly, exposure to the just officer reduced youths' willingness to cooperate with the police in their community as compared to the control group.

Conclusions A single virtual police exposure may not critically shape youths' overall perceptions of police legitimacy, but it may impact their willingness to cooperate. Youth may differentiate their evaluations of specific officers from their views of police more broadly.

Keywords Procedural justice · Police · Legitimacy · Cooperation · Juvenile

Introduction

Policy rely on the public's voluntary cooperation. Procedural Justice (PJ) Theory (Tyler, 1990) posits that when police engage in PJ by treating community members with respect, dignity, and neutrality, and give them the opportunity to voice their concerns (Tyler, 2003, 2006), people regard the police as more legitimate (Tyler, 1990) and are more willing to cooperate voluntarily (WTC; Murphy, 2015; Walters & Bolger, 2019). Experimental studies illustrate aspects of this process (Maguire et al., 2017; Mazerolle et al., 2013; Solomon, 2019). Exposure to a PJ

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police video improves adults' legitimacy perceptions and unjust treatment reduces perceptions (Maguire et al., 2017). Moreover, PJ can improve adults' WTC with a specific officer, though potentially not WTC with police more generally (Johnson et al., 2017). Although PJ perceptions may indirectly affect youths' WTC through legitimacy (Murphy, 2015), experimental evidence is lacking.

Using a youth sample, the current study sought to replicate and extend previous experimental studies conducted with adults. Findings may differ because unlike adults, modern youth have only known a world with smartphones (Gilbert, 2019) and social media. Considering nearly all teenagers regularly use social media (Anderson & Jiang, 2018) during a historic social movement protesting police brutality (Buchanan et al., 2020), most youth have likely been exposed to police videos (Cross & Fine, 2021). Yet, despite the large amount of time youth spend online, studies have not examined the impact of virtual exposure on youths' police perceptions and law-related behavior. Moreover, youths' perceptions of the police have dropped to a decades-long low (Fine et al., 2020a). Understanding how to improve youths' WTC is paramount for public safety considering offending and victimization peak during adolescence (Macmillan, 2001; Sweeten et al., 2013).

The current study tested four hypotheses (see Appendix for figures):

(H1) Youth exposed to the PJ video will view police as more legitimate than youth in the control group, who will report higher legitimacy than youth in the procedurally unjust condition.

(H2) Youth exposed to the PJ video will be more willing to cooperate with the depicted officer than youth exposed to the procedurally unjust officer video.

(H3) Watching a single video will not directly impact youths' global WTC with the police in their community.

(H4) The effects of the PJ video will indirectly affect youths' specific and global WTC through youths' views of police legitimacy.

Methods

Participants

A national sample of parents with adolescent children (ages 13–17) was recruited through CloudResearch, an online platform for survey research similar to past studies using Qualtrics or Survey Monkey (McDonald et al., 2018; Thompson & Pickett, 2020). CloudResearch recruited parents into non-probability sampling quotas based on demographic categories proportionate to the US Census (e.g., age, sex, race, Hispanic/Latinx). The study was explained to the parents and, following parental consent, their adolescent children (13–17 years old) were invited to participate in the study. Assenting youth ranged from 13 to 17 years old ($M = 15.41$, $SD = 1.40$) and approximately half identified as female (50.31%, see Table 1).

Table 1 Descriptive statistics

| | N/M | %/SD |
|-----------------------------------|-------|--------|
| Age | 15.41 | 1.40 |
| Gender | | |
| <i>Boy/male</i> | 411 | 49.69% |
| Race/ethnicity | | |
| <i>White</i> | 519 | 63.22% |
| <i>Black</i> | 139 | 16.93% |
| <i>Hispanic/Latinx</i> | 91 | 11.08% |
| <i>Multiracial/Other</i> | 72 | 8.77% |
| Previously arrested (1 = yes) | 33 | 4.03% |
| Knows a police officer (1 = yes) | 366 | 44.63% |
| Victimization (1 = yes) | 76 | 9.27% |
| Police legitimacy | 3.52 | 0.99 |
| Global cooperation | 4.12 | 0.97 |
| Specific cooperation ^a | 3.88 | 1.14 |
| Total | 822 | - |

N frequency, *M* mean, % percentage of the sample, *SD* standard deviation, ^aOnly youth who received the positive or negative video treatment conditions received the specific cooperation measures

Procedures

After being informed of the study's purpose, voluntary nature, and anonymity, 1014 youth began the 10-min survey. Youth completed demographics before being randomly assigned to one of three conditions: (1) positive (procedurally just) video; (2) negative (procedurally unjust) video; and (3) no video (control group). The 2–3 minute videos portrayed a traffic stop from a police officer's body-worn camera perspective (Johnson et al., 2017; Maguire et al., 2017). Briefly, in each, the same male officer (not visible) pulls over the same Black male teenager for driving 18 miles per hour over the speed limit. In the PJ video, the officer greets the teen, adopts a neutral approach, treats the driver with respect, issues a citation, and allows the adolescent to voice his concerns. In the negative video, the officer raises his voice, is disrespectful, issues the citation, and does not allow the youth to voice his concerns.

After the videos, youth completed the rest of the measures. Two attention checks (e.g., *Please select 'always' for this question*) were employed. Youth failed attention checks similarly across treatment groups ($n = 63$), suggesting treatment did not affect response quality; thus, we excluded those youth from analyses. Of the 951 remaining cases, CloudResearch flagged 129 for removal due to quality concerns, such as demographics outside of the target range, excessively short surveys wherein they skipped the videos, or attempted repeats. Thus, they were dropped from analyses, yielding a total n of 822.

Measures

Police legitimacy Six items assessed perceptions of police legitimacy. Four (e.g., *I have a great deal of respect for the police*) were drawn from a widely used scale (Tyler, 2006, p.48; Fine et al., 2020b). Two items also assessed normative alignment (e.g., *The police usually act in ways that are consistent with my own ideas about what is right and wrong*; Fine et al., 2021; Jackson et al., 2020). After factor analyses (see Appendix), items were mean-scored ($\alpha=0.92$) such that higher scores indicated more legitimacy.

Specific WTC Immediately after the video, youth indicated their willingness to cooperate with the officer by calling to report crime, reporting suspicious activity, and providing information (see Appendix; Bradford et al., 2014; Mazerolle et al., 2013). Items were mean-scored ($\alpha=0.92$), where higher scores indicated more WTC.

Global WTC Youth in all three conditions indicated how likely they would be to cooperate with police in their community using the same three items described above. The items were mean-scored ($\alpha=0.90$) such that higher scores indicated more WTC.

Covariates Prior to the treatment, youth reported their age, gender, race/ethnicity, prior arrest, crime victimization history, and relationships with police (see Appendix).

Analytic plan

Balancing was checked using chi-square tests and one-way analyses of variance (ANOVA). Next, *t*-test analyses determined the direct effects of treatment on youths' perceptions of police legitimacy and WTC. Path analyses are more robust and stringent than *t*-tests because they include covariates, direct effects, and indirect effects; thus, path models (Models 1, 2a, 2b) assessed the indirect effects of treatment on legitimacy perceptions and WTC, using maximum likelihood estimation in Mplus8 (Muthén & Muthén, 2017) and 10,000 bootstrap draws for the indirect effects' confidence intervals (Allison et al., 2017). Model fit was assessed through the chi-square fit statistic (χ^2), root mean square error of approximation (RMSEA), standardized root mean square residual (SRMRS), comparative fit index (CFI), and Tucker-Lewis fit index (TLI).

Results

Balance checks

The randomly assigned conditions were balanced on every metric (Table 2).

Table 2 Descriptive statistics by treatment condition

| | Control <i>n/M (%/SD)</i> | Just condition <i>n/M (%/SD)</i> | Unjust condition <i>n/M (%/SD)</i> | X^2 /ANOVA | <i>p</i> |
|--|------------------------------|-------------------------------------|---------------------------------------|--------------|----------|
| Age ^a | 15.49 (1.40) | 15.39 (1.33) | 15.34 (1.47) | 15.93 | 0.42 |
| Gender | - | - | - | 0.12 | 0.94 |
| <i>Boy/male</i> | 157 (49.37%) | 114 (49.14%) | 135 (50.56%) | - | - |
| Race/ethnicity ^a | - | - | - | 0.13 | 0.94 |
| <i>White</i> | 194 (60.82%) | 152 (65.24%) | 173 (64.31%) | - | - |
| <i>Black</i> | 58 (18.18%) | 38 (16.31%) | 43 (15.99%) | - | - |
| <i>Hispanic/Latinx</i> | 40 (12.54%) | 23 (9.07%) | 28 (10.41%) | - | - |
| <i>Multiracial/other</i> | 27 (8.46%) | 20 (8.58%) | 25 (9.29%) | - | - |
| Arrested (1 = <i>yes</i>) | 13 (4.09%) | 10 (4.29%) | 10 (3.73%) | 0.11 | 0.95 |
| Knows a Police Officer (1 = <i>yes</i>) | 151 (47.48%) | 92 (39.48%) | 123 (45.72%) | 3.67 | 0.16 |
| Victimization (1 = <i>yes</i>) | 34 (10.66%) | 22 (9.44%) | 20 (7.46%) | 1.78 | 0.41 |
| Total | 320 (100%) | 233 (100%) | 269 (100%) | - | - |

Chi-square analyses were conducted to assess differences across groups unless otherwise noted *n* frequency, *M* mean, % percentage of each treatment group, *SD* standard deviation, X^2 chi-square, ^aOne-way ANOVA tests were conducted to assess differences across groups on multigroup categorical or continuous variables. ^bSpecific cooperation items were not given to the control group

Legitimacy

T-test analyses (Table 3) indicated that youth in the PJ condition reported significantly more legitimate views of police than youth in the control group. However, *t*-test analyses revealed no significant group mean differences between youth in the unjust and control conditions, or between youth in the PJ and the unjust conditions.

The path models fit the data well (see Appendix). When accounting for covariates, exposure to a treatment video was not associated with youths' perceptions of police legitimacy. Contrary to the results from the *t*-test analyses, this more robust test

Table 3 Mean differences in legitimacy perceptions and willingness to cooperate

| | Just | Unjust | Control | <i>t</i> | <i>df</i> | <i>p</i> | <i>Cohen's d</i> | 95% C.I. lower | 95% C.I. upper |
|----------------------|------|--------|---------|----------|-----------|----------|------------------|-------------------|-------------------|
| Legitimacy | 3.62 | - | 3.44 | -2.15 | 551 | 0.03 | -0.19 | -0.35 | -0.17 |
| | - | 3.55 | 3.44 | -1.38 | 587 | 0.17 | -0.11 | -0.28 | 0.04 |
| | 3.62 | 3.55 | - | -0.84 | 500 | 0.40 | -0.07 | -0.24 | 0.10 |
| Specific cooperation | 4.24 | 3.58 | - | -6.73 | 500 | <0.001 | -0.66 | -0.85 | -0.47 |
| Global cooperation | 4.08 | - | 4.14 | 0.77 | 551 | 0.44 | 0.06 | -0.09 | 2.23 |
| | - | 4.14 | 4.14 | 0.01 | 587 | 0.99 | 0.00 | -0.16 | 0.16 |
| | 4.08 | 4.14 | - | 0.70 | 500 | 0.48 | 0.06 | -0.11 | 0.24 |

t *t*-test statistic, *df* degrees of freedom, *p* *p*-value, *C.I.* confidence interval

of the first hypothesis indicated watching a treatment video did not meaningfully impact youths' legitimacy perceptions. However, White youth, boys, and those who knew police officers viewed the police as more legitimate than non-White youth, girls, and those who did not know police officers, respectively (Tables 4–5).

Specific willingness to cooperate

In both the *t*-tests (Table 3) and path models (Table 4), youth in the PJ condition reported significantly more WTC with the officer than youth in the unjust condition. The path analyses did not detect an indirect treatment effect through legitimacy (Table 6). Altogether, youth were more willing to cooperate with the just officer than the unjust officer, though the effect did not operate through youths' perceptions of police legitimacy in general.

Global willingness to cooperate

Results of *t*-tests indicated no significant differences between treatment groups in global WTC (Table 3). Two path analyses were conducted for a more robust test of the hypothesized model (Table 5). The results of model 2a indicated that youth who watched the PJ video did not significantly differ in their global WTC than youth who watched the unjust video. The results of model 2b indicated that youth in the unjust condition reported similar global WTC as youth in the control group. Interestingly, youth in the PJ group reported significantly *less* global WTC compared to youth in the control group.

Table 4 Effects on youths' willingness to cooperate with the just video officer (model 1)

| | Legitimacy | | Specific cooperation | |
|-------------------------------|------------|-----------|----------------------|-----------|
| | <i>b</i> | <i>SE</i> | <i>b</i> | <i>SE</i> |
| <i>Key variables</i> | | | | |
| Procedurally just video | 0.09 | 0.09 | 0.62 *** | 0.09 |
| Police legitimacy | - | - | 0.54*** | 0.06 |
| <i>Control variables</i> | | | | |
| Age | -0.01 | 0.02 | -0.02 | 0.03 |
| Gender (<i>boy/male</i> = 1) | 0.25*** | 0.07 | -0.07 | 0.09 |
| Race/ethnicity | | | | |
| <i>Black/African American</i> | -0.81*** | 0.10 | -0.04 | 0.13 |
| <i>Hispanic/Latinx</i> | -0.40*** | 0.11 | -0.27 | 0.20 |
| <i>Multiracial/other</i> | -0.27* | 0.10 | 0.09 | 0.16 |
| Previously arrested | -0.23 | 0.23 | -0.11 | 0.23 |
| Knows a police officer | 0.15* | 0.07 | -0.01 | 0.09 |
| Victimization | -0.23 | 0.13 | 0.12 | 0.17 |
| Constant | 3.69 *** | (0.38) | 2.04*** | (0.51) |

b coefficient represents estimated change for each unit increase, *SE* standard error, **p* < 0.05, ***p* < 0.01, ****p* < 0.001

Table 5 Treatment effects on global cooperation

| Outcome: | Model 2 ^a | | | Model 2 ^b | | | | |
|-------------------------------|------------------------|------|--------------------------------|----------------------|------------------------|------|--------------------------------|------|
| | Legitimacy <i>b</i> | SE | Global Cooperation <i>b</i> | SE | Legitimacy <i>b</i> | SE | Global Cooperation <i>b</i> | SE |
| <i>Key variables</i> | | | | | | | | |
| Treatment video | | | | | | | | |
| <i>Procedurally just</i> | 0.10 | 0.09 | -0.09 | 0.07 | 0.15 | 0.08 | -0.19 | 0.06 |
| <i>Procedurally unjust</i> | - | - | - | - | 0.06 | 0.08 | -0.10 | 0.06 |
| Police legitimacy | - | - | 0.61*** | 0.04 | - | - | 0.61*** | 0.04 |
| <i>Control variables</i> | | | | | | | | |
| Age | -0.01 | 0.02 | -0.03 | 0.02 | -0.01 | 0.02 | -0.03 | 0.02 |
| Gender (boy/male = 1) | 0.25*** | 0.07 | -0.09 | 0.05 | 0.25*** | 0.07 | -0.09 | 0.05 |
| Race/ethnicity | | | | | | | | |
| <i>Black/African American</i> | -0.81*** | 0.10 | -0.04 | 0.09 | -0.80*** | 0.10 | -0.04 | 0.09 |
| <i>Hispanic/Latinx</i> | -0.40*** | 0.11 | -0.09 | 0.10 | -0.40*** | 0.11 | -0.10 | 0.10 |
| <i>Multiracial/other</i> | -0.27* | 0.10 | -0.05 | 0.10 | -0.27* | 0.10 | -0.05 | 0.10 |
| Previously arrested | -0.23 | 0.23 | -0.15 | 0.18 | -0.23 | 0.23 | -0.15 | 0.18 |
| Knows a police officer | 0.15* | 0.07 | 0.07 | 0.05 | 0.16* | 0.07 | 0.06 | 0.05 |
| Victimization | -0.23 | 0.13 | -0.11 | 0.13 | -0.23 | 0.13 | -0.12 | 0.12 |
| Constant | 3.69*** | 0.38 | 2.49*** | 0.33 | 3.66*** | 0.38 | 2.55*** | 0.33 |

b estimated change for each unit increase, *SE* standard error, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6 Indirect, direct, total indirect, and total effects on cooperation

| | Model 1 | | Model 2a | | Model 2b | |
|------------------------|----------|-----------|----------|-----------|----------|-----------|
| | <i>b</i> | <i>SE</i> | <i>b</i> | <i>SE</i> | <i>b</i> | <i>SE</i> |
| <i>Just video</i> | | | | | | |
| Indirect effects | 0.05 | 0.05 | 0.06 | 0.05 | 0.09 | 0.05 |
| Direct effects | .62*** | 0.09 | -0.09 | 0.07 | -0.19** | 0.06 |
| Total indirect effects | 0.05 | 0.05 | 0.06 | 0.05 | 0.09 | 0.05 |
| Total effects | 0.67 | 0.10 | -0.03 | 0.08 | -0.10 | 0.08 |
| <i>Unjust video</i> | | | | | | |
| Indirect effects | - | - | - | - | 0.04 | 0.05 |
| Direct effects | - | - | - | - | -0.10 | 0.06 |
| Total indirect effects | - | - | - | - | 0.04 | 0.05 |
| Total effects | - | - | - | - | -0.06 | 0.08 |

SE standard error, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Discussion

This study assessed the impact of vicarious virtual exposure to a procedurally just versus unjust traffic stop on youths' perceptions of and willingness to cooperate with the police. Through assessing both the direct and indirect effects, the present study provides a methodologically rigorous assessment of the procedural justice framework in explaining the effects of virtual exposure to procedurally just and unjust policing practices. Moreover, this study also uniquely accounts for the possibility that youths' willingness to cooperate with specific officers may differ from their willingness to cooperate with police in their community.

Procedurally just treatment should elicit higher levels of police legitimacy (Tyler, 1990). Yet as Sahin and colleagues (2017) posit, "...it might be overly optimistic to believe that durable citizen perceptions of confidence and trust in the police can be strongly impacted by a solitary interaction" (p. 712). Indeed, that is exactly what this study found; unlike adults (Maguire et al., 2017), a single virtual exposure to police conduct in this study did not appear to shape youths' views of police legitimacy in general. There are several possible explanations.

First, youth spend significant amounts of time online (Anderson & Jiang, 2018). In fact, within this sample, 97% of youth who used social media reported previous exposure to police-related content on social media (Cross & Fine, 2021). Adolescents who watch hours of videos per day on Twitter, Instagram, or TikTok may not be as affected by virtual stimuli as adults with less exposure. Second, a single video may impact how youth viewed the legitimacy of the officer in the video without changing perceptions of police in their community. Although more research is needed, a single virtual exposure to a traffic stop may not meaningfully shape youths' perceptions of police legitimacy in general. Unfortunately, there were clear methodological limitations that temper such conclusions, as youth were asked to reflect on their views of the police as a general institution rather than how legitimate

they found the specific officer. Finally, it is important to note the content of the virtual exposure. Youth saw a relatively neutral traffic stop between a male officer and a Black male teenager where the teenager clearly violated minor traffic laws (Johnson et al., 2017; Maguire et al., 2017). There were no physical altercations or overtly racialized language that may elicit strong emotional responses. A single exposure to a more evocative virtual portrayal of procedural injustice could impact youths' views.

As expected, youth who watched the procedurally just video were more willing to cooperate with that officer as compared to youth exposed to an unjust officer video. The effects appeared direct, above and beyond youths' general perceptions of police legitimacy. However, these effects did not generalize to youths' WTC with police more generally in their community. As such, youth appear to differentiate between their evaluations of a specific officer and police more broadly. Youth may view a specific officer who acts in a procedurally just manner to be an exception to the rule, rather than representative of officers more generally. For this reason, police departments should ensure that officers who behave fairly and respectfully are the norm rather than the exception.

Interestingly, youth who watched the procedurally just video reported less WTC with the police in their community, relative to youth in the control group. This result was surprising because it was the opposite of what PJ theory contends. However, communication accommodation theory suggests using more effort than socially appropriate for the situation could negatively affect approval and WTC (Giles et al., 2007). Youth may have interpreted the procedurally just officer as artificial and reacted negatively, such as in previous experiments with disingenuous officers engaging in PJ practices (Macqueen & Bradford, 2015, 2017). Alternatively, youth may have viewed the officer as acting in ways inconsistent with their expectations of police (Lowrey et al., 2016). This could mean that youth expect to be treated so poorly by police that being treated respectfully was construed as disingenuous, which underscores a much deeper need for the police to implement higher standards of police behavior.

Intriguingly, almost half (44.63%) of youth personally knew a police officer. With over one million sworn active-duty officers nationwide and officers in a majority of public schools (Banks et al., 2016; Diliberti et al., 2019), it is reasonable to expect that youth may know police officers. Because such interpersonal relationships are so prevalent and may impact how youth perceive police, future studies must consider assessing these relationships.

Limitations

As the first experimental study on a national scale testing the effects of virtual vicarious police exposure, this study offers a valuable foundation for future inquiry. Yet, there are clear limitations. First, this study used a single video. Future studies should assess repeated exposure, as well as differential effects depending on the race, gender identity, or sexual orientation of the police officer

or community member. Future work should assess participants' perceptions of the videos to confirm that the treatment conditions were perceived as intended. Moreover, the sample may differ on characteristics not included in the quota sampling technique, and generalizability may be limited to families with Internet access who participate in studies online. Finally, we grouped multiracial/ethnic youth as belonging to their minority racial/ethnic statuses under the assumption that their minority features and identity likely contribute to the way they are treated by and perceive the police (Fine et al., 2020b; Johnson et al., 2017; King & Johnson, 2016). However, this approach inherently reduces their unique lived experiences and diminishes the complexity of multiple racial/ethnic heritages. Future studies should consider diverse dimensions of race (Roth, 2016).

Conclusions

The findings have several implications. First, many youth know a police officer, and when they do, they tend to report more positive perceptions of police legitimacy. This marks an underdeveloped area of research and a critical metric for future studies to include. Furthermore, a single exposure to a video of procedurally just traffic stop can shape youths' willingness to cooperate with the specific officer. If adolescents see even one video of a police officer treating another youth justly, they may be more willing to contact that officer with crime-related information. However, simultaneously, seeing even one video of an officer engaging in unjust police behavior may damage adolescents' willingness to reach out to that officer with critical, crime-related information. Police should recognize the far-reaching impact of each procedurally unjust interaction beyond those with whom they interact, to any who may virtually witness their interaction. Virtual vicarious exposure to procedural injustice, then, may jeopardize the voluntary public cooperation police require.

Supplementary Information The online version contains supplementary material available at <https://doi.org/10.1007/s11292-022-09525-x>.

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