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# Did the Murder of George Floyd Damage Public Perceptions of Police and Law in the United States?

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**Abstract**

*Objectives*: The police killing of George Floyd energized the Black Lives Matter (BLM) social movement across the United States in the summer of 2020. We test the impact on public perceptions of the fairness and

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Replication materials for this paper can be found at: https://github.com/oliveirathiago/JRCD_Floyd.

legitimacy of the police and law. *Methods*: A four-state, three-wave, short-term longitudinal study ($N = 1048$; Arizona, Michigan, New York, and Texas) used a novel design focused on differences in change over time to test whether public perceptions changed after the killing of Floyd. *Results*: Fielding multiple outcome markers, as well as multiple pseudo-placebo comparison variables, we found that perceptions of police procedural justice, distributive justice, and bounded authority, as well as perceptions of the legitimacy of the police and law, declined following Floyd's murder. Levels of trust in science, identification with healthcare workers, and collective efficacy perceptions did not change. As discussed in the paper, the effects varied by participants' political views. *Conclusions*: The police killing of George Floyd and subsequent protests seemed to have damaged attitudes towards police and the law.

**Keywords**
procedural justice, police perceptions, police legitimacy, police brutality

On May 25, 2020, Derek Chauvin, a Minneapolis police officer, murdered George Floyd, a Black community member. Over 1,000 members of the public die at the hands of the police in the United States every year (Edwards and Esposito 2019; GBD 2019; Schwartz and Jahn 2020; Hirschfield 2023), but what made Floyd's murder particularly disturbing was that Floyd was unarmed and Chauvin knelt on Floyd's neck for almost 10 minutes in plain view of three other officers, numerous witnesses with smartphones, and local security cameras. With approximately 10,000 demonstrations and protests across all 50 states and Washington, D.C. taking place over the following three months (ACLED 2020), Floyd's murder reenergized one of the largest social movements in U.S. history, the largest series of police reform efforts (Buckholz 2021; Smith 2022), and, in all likelihood, an ongoing crisis in police retention and recruitment (Mourtgos et al. 2022).

The killing led to a rapid resurgence in the Black Lives Matter (BLM) social movement—an activist group that campaigns against violence and discrimination towards Black people, particularly in the form of police brutality and under- and overpolicing (Francis & Wright-Rigueur, 2021; Buchanan et al. 2020). Of the close to 10 million distinct Twitter users who had tweeted the #BlackLivesMatter hashtag between July 2013 and

March 2023, just under seven million unique users posted it between May and September 2020 (Bestvater et al. 2023; see also Wu et al. 2023). The event and its aftermath also sparked significant counter-protests, with sharp spikes in the use of #BlueLivesMatter and #AllLivesMatter hashtags (Giorgi et al. 2022; Wu et al. 2023; Francisco and McMillan 2024).

Following Floyd's murder, numerous polls entered the field (AP 2020; Rakich 2020; Reny and Newman 2021; Fine and Del Toro 2022; Bestvater et al. 2023). A few studies focused on the impact of the murder of Floyd. Yet, they were either cross-sectional or utilized a single-item measure of views of police. While some found subsequent increases in violent firearm incidents (Boehme et al. 2022) or decreases in types of 911 calls (Ang et al. 2021), the longitudinal studies conducted during that time typically focused on anger and sadness (Eichstaedt et al. 2021), on the obligation to obey (Cross et al. 2023), or distress about police brutality (Howard et al. 2022).

In this paper, we examine the potential effects of the police killing of George Floyd on public attitudes toward the fairness and legitimacy of the police and law. We consider this high-profile police murder to be a focusing event. The notion of a focusing event (Kingdon 1995) has its roots in "triggering devices" within political science (see Cobb and Elder 1983). While more robust explanations exist elsewhere (see Wood 2006), focusing events punctuate public consciousness and draw attention to a particular issue (Farley et al. 2007).

Scholars within criminology and criminal justice have been studying various focusing events for at least the last few decades. As one of the highest profile examples of studying such focusing events within criminology and criminal justice, David Kirk studied recidivism and residential change within the context of post-Hurricane Katrina Louisiana (Kirk 2009, 2015). Within the context of policing, "high-profile cases of excessive police force constitute a severe breach in the social contract that exists between citizens and the criminal justice system" (Desmond et al. 2016: 871) and are considered to be focusing events. For instance, Desmond and colleagues (2016) found that the beating of Frank Jude Jr. reduced 911 calls for police service for over a year in Milwaukee.

Our study specifically focuses on George Floyd's murder as a focusing event. Certainly, we are not the first to do so. Piza and Connealy (2022) found that crime rates increased in an autonomous zone of Seattle that emerged in the wake of protests in 2020. Relatedly, Nix and colleagues (2023) found that when police reduced high-discretion police actions in the wake of Floyd's murder, violent and property crimes likely increased

in Denver, Colorado. More specifically, we build on the work by Reny and Newman (2021). Their study found that police favorability decreased after the police killing of George Floyd, but not among conservatives and high-prejudice U.S. citizens (Reny and Newman 2021).

Our study makes two critical contributions beyond the foundation set by these previous studies. First, methodologically, the Reny and Newman (2021) study was limited by its single-item measurement approach, lack of comparison variables, and cross-sectional design. In comparison, we draw on data from a three-wave, repeated measures study and use a novel design focused on estimating differences in changes over time. We are able to estimate within-person changes in this short-term longitudinal design.

Moreover, we help push the field forward in terms of both theory and research methodology. Drawing on and contributing to procedural justice theory (Sunshine and Tyler 2003), we present evidence that the police killing and the ensuing protests against racialized policing damaged not only public perceptions of the police (procedural justice, distributive justice, respecting the limits of their rightful authority, and legitimacy) but also the legitimacy of the law. Notably, we also leveraged pseudo-control variables so that we could examine the extent to which purported effects for constructs might be smaller when their distance to the central construct is larger. That is, we consider perceptions of police to be at the core of what we expect to change. Perceptions of the law are a bit further away, and thus should change somewhat less. Finally, perceptions of our pseudo-control variables (identification with healthcare workers, trust in science, and collective efficacy) should be the furthest away and should therefore change the least. Indeed, we found that while the public perceptions of police and law declined during this period, perceptions of our pseudo-control constructs did not change. This finding—that effect sizes declined as the constructs radiated away from the core of perceptions of police—indicates that studies of focusing events should include metrics that radiate away from the central constructs so that we can determine the extent to which purported findings are not simply artifacts of a flawed study design.

In the following few sections, we outline the procedural justice framework, motivate the theoretical goals of the study, and set out the rationale of the repeated measures and short-term longitudinal design of the study.

## Procedural Justice Theory

The foundational elements of the procedural justice framework can be traced back to Thibaut and Walker (1975). Today, procedural justice studies

pervade both the criminology and psychology-law literatures. Broadly speaking, these studies share a common notion—that people care about the extent to which authorities treat people fairly and justly, that it is important to give a sense of voice and enable participation when officials and citizens interact, and that these interpersonal aspects of process matter a great deal to people's connections to, and behavior towards, group authorities. As Tyler (2023: 14.3) recently summarized, "people make distinct evaluations of the appropriateness of the manner in which authorities or institutions exercise their authority and use those evaluations to shape their attitudes about their legitimacy and in shaping [their] behaviors."

Consistent with these propositions, both personal and vicarious experiences with legal authority have been linked with people's perceptions (Oliveira et al., 2021), with procedural justice predicting cooperation and compliance primarily through perceptions of legitimacy (see Sunshine and Tyler 2003; Mazerolle et al. 2013; Reisig et al. 2023). Altogether, there is significant empirical support for the central features of this framework (Jackson 2018; Jackson et al., 2023a; Walter and Bolger 2019), though more longitudinal and experimental work in policing and legal contexts is needed (Nagin and Telep 2020). We also have limited data on the impact of a societal-level event like the killing of Floyd and extraordinary rise of a social movement like BLM.

About 25 years after Thibaut and Walker's (1975) foundational piece, scholars in the field began developing the group engagement model (GEM; Tyler and Blader 2000; Blader and Tyler 2009). While the GEM helps explain why people engage in cooperative behavior, it also explains why people might refuse to engage in behaviors that support the group's goals, and which factors shape commitment to the group. Broadly speaking, the GEM starts from the premise that people have social identities, that they shape their social identities based on the behaviors of the groups with which they identify, and that they behave according to a concordance—or discordance—between what they perceive to be right, how they expect the group's representatives to behave, and how they see the group's representatives actually behaving. When there is concordance, people behave in ways that support the group's goals and expectations. When there is discordance, people may dissociate from the group, refuse to engage in supportive behavior, and/or actively engage in unsupportive or noncooperative behavior (Giles et al., 2021).

The procedural justice framework and the GEM are compelling for a variety of different reasons. Community members *should* be treated justly by armed (and unarmed) authorities (Reiss 1971; Lind and Tyler

1988; Nagin and Telep 2020), but if a government is concerned with governing through mutual consent and legitimacy, one thing that is central (Tyler and Nobo 2022) is procedural justice (i.e., how the police, as representatives of the law, treat people and make decisions during interactions with officers). To the extent that the public feels that police treat people and make decisions fairly, justly, and with procedural justice, they will be more likely to legitimize law enforcement and the broader rule of law that the police represent. In turn, the more people legitimize police and the law, the more likely they should be to engage in cooperative, compliant, and supportive behaviors (Peyton et al. 2019) and less likely to engage in crime or take the law into their own hands (Haas et al. 2014). In contrast, when people believe that the police are acting in procedurally unjust ways, by for instance using unnecessary force, then the police's actions—or responses—may undermine their own legitimacy. Critically, because normatively appropriate police behavior encourages people to self-regulate, this then reduces the need for more coercive and less effective forms of social control.

Recently, procedural justice scholars have started to explore the idea that legitimacy—and the consent that legitimacy generates—is not only enhanced by procedurally just policing that sends identity-relevant messages about status, neutrality, and group-standing, but also by practices that signal respect for people's autonomy and right not to be arbitrarily controlled and harassed (Tyler et al. 2015; Huq et al. 2017; Trinkner et al. 2018). Conversely, policing that seems neglectful and heavy-handed towards racialized communities may lead some people to question the legitimacy of an institution that directs its power toward maintaining established racial hierarchies, especially if they identify with BLM (Bradford and Jackson 2017; Jackson et al. 2023b).

## *Did the Events of Summer 2020 Damage Public Perceptions of the Police and Law?*

In this paper, we consider the impact of the police killing of George Floyd and the subsequent resurgence in BLM on public opinion. Drawing on Reny and Newman (2021), we frame the killing as what political scientists call a *focusing event*. Focusing events act as a "push…like a crisis or disaster that comes along to call attention to the problem, a powerful symbol that catches on" (Kingdon 2003: 94–95), generating extensive media coverage,

revealing significant public harms, and mobilizing social movements and protests (Birkland 1998).

We treat the killing and subsequent protests and counter-protests as something akin to a "bundled treatment," that is, something that cannot easily be disentangled. In this spirit, we test whether the police killing of George Floyd and the rapid resurgence in BLM (as well as counter-protests like Blue Lives Matter) shaped people's perceptions of the fairness and legitimacy of the police and law. Public perceptions of procedural justice may have been damaged by the idea that officers use violence against unarmed Black civilians in biased and unaccountable ways. Public perceptions of distributive justice may have been eroded by the idea that officers impose the burdens of policing (especially aggressive social control) on Black communities. Public perceptions of officers' respect for authority boundaries may have been damaged by the idea that officers abuse their power through intrusive tactics.

Additionally, we test the effect on police and legal legitimacy. Building on the idea that normatively inappropriate police behavior harms the belief that they are (a) a moral, just, and appropriate institution and (b) entitled to obedience (Jackson 2018), we assess whether people questioned the legitimacy of the police. We also test whether the legitimacy of the law was damaged. Work on legal socialization (Tapp 1991; Tyler and Trinkner 2018) suggests that tangible (direct and mediated) interactions with the police (and other legal authorities) can shape the more abstract representation of the law that permeates society. In the words of Trinkner et al. (2018: 283):

> The latter represents people's ideas about the purpose of law and how it assists in the creation and maintenance of a just and mutually beneficial scheme of social cooperation (Rawls 1964). The former is a more concrete representation of the values that underlie that scheme (Tapp and Levine 1974). Whereas laws represent societal norms about what is right and proper behavior, law enforcement represents notions about how right and proper behavior should be transmitted and enforced within the populace.

On this account, if the law was being seen to be enforced in racist ways, then people may start to question the *quid pro quo*—whether the law really does reflect, generally speaking, a morally just regime. People give up freedoms in exchange for a just system of social order and cooperation, and the Summer 2020 events may have damaged people's perceptions of the normative, binding qualities of the law—although any putative effect on perceptions of the legitimacy of the law may be weaker compared to police

legitimacy, given that the police are the face of the law, but not coterminous with the law (see Fine et al. 2019).

## Politics as a Lens

Crucially, we also assess whether effects were uniform across our sample, or whether they varied according to people's prior political views. Tests of the procedural justice model have examined whether the framework works better for some social groups and neighborhood contexts than it does for others. Thus far, the evidence seems to be somewhat mixed; a recent meta-analysis suggests that the evidence is "more supportive of the invariance thesis than not" (Chan et al. 2023: 17).[1]

But there are reasons to believe that the effects of the Summer 2020 events on public attitudes towards the police and law may have been different for different people, depending on their prior political views. Work in political science shows how minority-led protests can push an issue that was previously specific to a subpopulation group to the forefront of the public agenda, shifting attitudes *especially among segments of society* who were ideologically predisposed to be open to the issues at their heart (Gillion 2012; Branton et al. 2015; Wasow 2017; Mazumder 2018). For example, Wasow (2017) found that proximity to Black-led nonviolent protests in the 1960s and 1970s increased White Democratic vote-share, while proximity to violent protests decreased Democratic vote-share.

Reny and Newman (2021) found that police favorability decreased after the police killing of George Floyd, but only among liberals and low-prejudice individuals (i.e., not among conservative and high-prejudice individuals). They concluded with the idea that social protest can help "create a favorable opinion climate for pursuing reforms aimed at reducing racial bias in policing" (Reny and Newman 2021: 9) but that it may be more difficult to persuade groups who have prior dispositions against the issues (see also Francisco and McMillan 2024). As such, we test whether the events had a negative effect on liberals and participants who did not intend to vote for Trump in the 2020 election. We reason that people with these political views will be more open, on average, to the relational signals sent by the killing of Floyd and surge in protests against racialized policing. Their perceptions of the police and law may have been affected by specific patterns of policing in Black communities, in part because they thought about the police (and the superordinate group they represent) in terms of a larger, overarching group identity (the country as a whole, including all its different communities) rather than more immediate, localized, or subgroup racial or majority–minority

identities. Liberals and intended non-Trump voters may thus have, in response to the events, come to question the moral authority of an institution that they believe directs its power towards maintaining established racial hierarchies.

Building on Reny and Newman (2021), we add a set of supplemental and exploratory analyses in which we examine what factors may explain the differences between participants. Specifically, we examine whether intended Trump voters (versus those who did not intend to vote for Trump) varied on key characteristics, including identification with the police, identification with BLM, and racialized beliefs about crime. This approach differs from Reny and Newman (2021), who used an index of Black–White group favorability difference and agreement or disagreement with the statement "Generations of slavery and discrimination have created conditions that make it difficult for Blacks to work their way out of the lower class." We instead measure racialized beliefs about crime. Our expectation is that conservatives and intended Trump-voters did not change their views of police and law, in part because they did not believe that police were biased against people of color.

## Current Study

The police killing of George Floyd captured the public's attention in the United States and energized the BLM social movement. As a critical focusing event, it may have impacted the public's perceptions and relationships with law and law enforcers. In perhaps the best work in this area, Reny and Newman (2021) leveraged a large dataset ($N = 378{,}507$; opt-in process conducted by Lucid) of weekly cross-sectional data weighted to reflect the U.S. public to examine the effects of Floyd's murder on public perceptions of the favorability of the police. Our *within-person* study makes three specific contributions.

First, the data we use in this study were collected in three waves between April 30 and June 17, 2020. George Floyd was murdered on 25 May towards the end of Wave-2 data collection (Wave 2 respondents interviewed after the 25th were excluded from analysis). In contrast, Reny and Newman (2021) included weekly, cross-sectional snapshots of large samples weighted to be nationally representative, meaning they did not model within-person change over time. While both approaches are clearly important, triangulating findings with different methodologies is critical. Altogether, to assess the potential impact of this likely critical period in American history, we

model within-person changes in perceptions of the fairness and legitimacy of the police and law using a *difference-in-changes* design.

Second, Reny and Newman (2021) used a single attitudinal measure, based on whether research participants in weekly online surveys during the 2019–2020 primary and general elections felt favorably towards the police. Their single indicator likely condensed a whole series of more specific attitudes concerning the fairness and effectiveness of police activity, including whether they treat people with respect and dignity, make unbiased and objective decisions, allocate the burdens and benefits of policing fairly across aggregate social groups in society, and do not unnecessarily intrude into people's lives and freedoms. Our study happened to be in the field at the time and it was specifically designed to inform measurement strategies for future work in procedural justice and legitimacy scholarship. As such, we are able to leverage a substantially more robust and differentiated measurement strategy, including measuring police procedural justice, distributive justice, violation of authority boundaries[2] and the legitimacy of the police and law. Unpacking the putative effects on these more specific dimensions of police activity can help us shed light on the nature and extent of any effects of the events, especially in terms of relational issues that signal procedural injustice, distributive injustice, and stepping over the boundaries of rightful authority in the context of policing Black communities.

Third, for scholars studying police attitudes, there is a large yet mostly unaddressed elephant in the room: most studies are self-report and use limited scales. Such attitudinal measures are clearly and inherently subject to a host of various biases, especially due to the risk of shared method variance. For instance, a participant may report negative perceptions of police, including in response to experimental stimuli, yet that does not inherently mean that the single finding tells the full story. To date, little work has been devoted to carefully parsing results to ensure that they are specific and unique to the purported effect. They could, for instance, just be having a bad day and report negative—or more negative—perceptions of that construct accordingly.

As a point of differentiation, Reny and Newman (2021) did not include comparison variables. One method for increasing confidence in our results is through testing the impact of a focusing event on a host of outcomes that radiate away from the central outcome of interest. As the proximity to the constructs of central concern (i.e., perceptions of the police and law) decreases, the effects should decrease. It is therefore necessary to compare effects for a set of constructs that are related to and also distinct from legal-oriented perceptions. In doing so, we can create more confidence in our

findings. Accordingly, we tested whether three pseudo-controls change over the time period of interest: trust in science, identification with healthcare workers, and collective efficacy perceptions. Considering the measurement issues within procedural justice and legitimacy scholarship (Fine et al. 2022), this approach may serve as a model for self-report studies in the future.

Therefore, our study included a set of pseudo-control variables which help us counter any concerns about general negativity bias in the sample. To test whether any such effects were specific to perceptions of police and the law, we were able to make pseudo-placebo comparisons, essentially assessing whether there were similar changes in respondents' levels of trust in science, identification with healthcare workers, and perceptions of collective efficacy. We expected that, because the measured constructs (e.g., trust in science) were conceptually far from the central constructs relating to police and law, effect sizes would be zero.

As a final point, we consider one critique of procedural justice theory: namely, that we lack a large body of compelling evidence of *change* in people's views of police as a result of experiencing problematic policing (Nagin and Telep 2017, 2020). While experimental studies that do indicate such effects are increasingly emerging (e.g., Mazerolle et al. 2013; Demir et al. 2020; Posch et al. 2021; Weisburd et al. 2022; Tom et al. 2023), many previous studies have been cross-sectional and correlational in nature. Notably, the extent to which people react to and reflect on police behavior, and as a result update their views on police legitimacy, remains something of an open question (though see Oliveira et al. 2021; Oliveira 2022). We do so in the context of the putative effect of a major societal focusing event.

## Method

### *Participants*

On April 30, 2020, we launched a four-state, three-wave, short-term longitudinal study ($N = 1048$; Arizona, Michigan, New York, and Texas) of the public's views of police through Prolific Academic. Prolific Academic is an online, crowd-source service that has higher quality control levels than other platforms that are becoming widely used in academic work, including criminology and psychology-law research (see Peer et al. 2017; Wilford et al. 2021; Pickett et al. 2022). Prolific also uses a range of tactics to limit duplicate respondents/accounts and bots, including requiring a non-Voice over Internet Protocol (non-VOIP) phone number to verify the account, limiting the

number of accounts that can use the same IP addresses, and requiring participant accounts to have a unique account (e.g., PayPal) to receive renumeration (for more information, see Bradley 2018).

We applied filters so that only adults over the age of 18 who resided in New York, Michigan, Arizona, or Texas were eligible for the study. We chose these states because we wanted geographic and political diversity (Weigel 2020) yet had a limited budget. We aimed to select states in various regions of the country that also varied in their political makeup. For instance, the Pew Research Center indicates that the self-identified political leanings in each state are: Arizona: 40% Republican and 39% Democratic; New York: 28% Republican and 53% Democratic; Michigan: 35% Republican and 47% Democratic; Texas: 39% Republican and 40% Democratic (Pew Research Center 2023). Relatedly, the Cook Political Report (2022) lists Arizona as Republican + 2, Michigan as Republican +2, New York as Democratic + 10, and Texas Republican + 5 (Cook 2023).

Our goal was to obtain approximately 1,000 participants, with about 250 participants from each state, and then conduct a three-wave, repeated measures study with these same participants. When we posted the study on Prolific for waves two and three, we utilized their filters so that the study was only viewable and accessible to people who had participated in the first wave. Everyone who was eligible (based on their Prolific ID) was also sent an email from Prolific asking them to participate. Participants were paid $2.15 for participating in Wave 1, $2.25 for Wave 2, and $2.50 in the final wave. In addition, those who participated in all three waves were given a $1 bonus.

Wave one occurred between April 30 and May 5, 2020, yielding 1048 participants: New York ($n = 301$), Michigan ($n = 262$), Arizona ($n = 205$), and Texas ($n = 321$). Each subsequent wave was three weeks apart, and the study was limited to people who had participated in wave one. Wave two ($n = 861$, 82% retention) occurred between May 22–28, 2020. On May 25, when approximately 86% of wave two's data had been collected, Floyd was murdered; we therefore omitted from analysis the 82 Wave 2 participants interviewed after the 25th. Wave three ($n = 742$, 70% retention) occurred three weeks later between June 12 and June 17, 2020. Participants did not need to complete wave two to be eligible to participate in wave three or be included in the analysis. Respondents who completed only wave one were eventually dropped in the regression models presented below, but were used in the measurement models. Note that a respondent who completed wave three but not wave two can have their wave two scores estimated via full information maximum likelihood (FIML).

**Table 1.** Sample Descriptive Statistics by State (all Measured at Wave 1).

| | Age (Mean) | Race/Ethnicity | | | | Education | | | | Political Orientation | | | Intended Trump Voter (Moderately Likely or Extremely Likely to Vote for him, Versus the Rest) | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| State | | White | Black | Hispanic | Asian | Less than High School | High School or Equivalent | Bachelors' Degree | Masters or Above | Republican | Democrat | Independent | Intended Trump Voter | Non-Trump Voter |
| Arizona | 38 | 81% | 5% | 12% | 9% | 1% | 46% | 33% | 20% | 14% | 35% | 28% | 17% | 83% |
| Michigan | 36 | 79% | 11% | 6% | 9% | 3% | 42% | 35% | 20% | 13% | 46% | 21% | 15% | 85% |
| New York | 34 | 70% | 8% | 10% | 18% | 1% | 37% | 37% | 25% | 12% | 56% | 12% | 13% | 87% |
| Texas | 32 | 60% | 11% | 20% | 17% | 1% | 49% | 37% | 13% | 18% | 44% | 19% | 18% | 82% |
| Full Sample | 35 | 71% | 9% | 12% | 14% | 1% | 43% | 36% | 19% | 14% | 46% | 19% | 16% | 84% |

The final analytic sample consisted of 962 participants, of whom 52% identified as women, 46% men, and 2% unlisted gender. Demographic characteristics of the sample are provided in Table 1. Because of FIML estimation, the number of participants could vary in the regression models (762–769).

## Measures

We used a variety of outcome markers to assess perceptions of police and law. First, we considered perceived procedural justice, which involves treating people fairly and respectfully, specifically through voice, neutrality, dignity, and respect (Tyler 2004). The procedural justice scale was adapted from Round 5 of the European Social Survey and consisted of three items: "*How often (if ever) do you think the police in your neighborhood make fair and impartial decisions in the cases they deal with*?"; "*How often (if ever) do you think the police in your neighborhood explain their decisions to the people they deal with*?"*;* and "*How often (if ever) do you think the police in your neighborhood treat people with respect*?" Response alternatives ranged from 1 (*never*) to 5 (*always*). To measure perceived procedural justice as a latent construct, we pooled observations from all three waves,[3] conducted a confirmatory factor analysis (CFA) using FIML to handle missing data, and then extracted factor scores for each observation so that higher scores indicated more perceived procedural justice. (See the Appendix for more information on measurement models.)

Second, we assessed perceived distributive justice, which relates primarily to the fair allocation of scarce resources across aggregate social groups in society. The distributive justice scale was also adapted from Round 5 of the European Social Survey, as well as Deutsch's (1975) principles of distributive justice. They consisted of five items: "*The police provide the same level of security to all community members*"*; "The police provide the same quality of service to all community members"; "The police enforce the law consistently when dealing with all community members"; "The police deploy their resources in this city in an equitable manner";* and "*The police ensure that everyone has equal access to the services they provide.*" Response alternatives ranged from 1 (*strongly disagree*) to 5 (*strongly agree*). As before, we conducted a pooled CFA (using FIML for missing data) and extracted factor scores for each observation so that higher scores indicated more perceived distributive justice.

Third, we assessed perceptions of bounded authority, which refers to the degree to which officers violate the boundaries of their rightful authority

(Trinkner et al. 2018). The seven items, adapted from previous work (Jackson et al. 2023c), were: "*How often do you think the police exceed their authority*?"*; "How often do you think the police get involved in situations that they have no right to be in*?"*; "How often do you think the police bother people for no good reason*?"*; "How often do you think the police overstep the boundaries of their authority*?"*; "How often do you think the police abuse their power*?"; "*How often do you think the police violate your personal sense of freedom*?," and "*How often do you think the police restrict your right to determine you own path in life.*" Response alternatives ranged from 1 (*never*) to 5 (*always*). However, note that we reverse-scored the items such that higher scores on the final variable, "respect for authority boundaries," indicated that participants perceived that the police respected the boundaries of their authority more. Consistent with our measurement strategy of latent constructs, we conducted a pooled CFA with FIML and extracted factor scores for each observation so that higher scores indicate police respect their authority boundaries more, and lower scores indicated police disrespect their authority boundaries less.

Fourth, we assessed perceptions of police legitimacy. We differentiated between assent/approval (do people believe that officers generally wield their power in normatively appropriate ways?) and consent/duty (do people feel a moral duty to obey the police?). The items for assent, also adapted from Round 5 of the European Social Survey (Jackson et al. 2011) and from previous work (Jackson et al. 2012), were: "*I support the way the police usually act*"; "*The police usually act in ways that are consistent with my own ideas about what is right and wrong*"*;* and "*The police stand up for values that are important for people like me.*" Response alternatives ranged from 1 (*strongly disagree*) to 5 (*strongly agree*). The items for consent were "*To what extent is it your moral duty to obey the police?"; "To what extent is it your moral duty to support the decisions of police officers, even if you disagree with them?";* and "*To what extent is it your moral duty to do what the police tell you even if you don't understand or agree with the reasons?"* Response alternatives ranged from 1 (*not at all my duty*) to 5 (*completely my duty*). After conducting a pooled CFA, we extracted factor scores for each observation so that higher scores indicated more normative alignment with police (i.e., more police legitimacy).

Fifth, we measured perceptions of law's legitimacy also as assent/approval (the belief that the law is, generally speaking, normatively appropriate) and consent/duty (internalization of the moral value that one should obey the law simply because it's the law). The three items for normative alignment with the law were: "*My own feelings about what is right and*

*wrong usually agree with the laws that are enforced by the police and the courts"; "The laws in my community are consistent with my own intuitions about what is right and just"; and "The laws of our criminal justice system are generally consistent with the views of the people in my community about what is right and wrong."* The three items for moral duty to obey the law were consent: "*People should do what the law says"; "A person who disobeys laws is a danger to others in the community";* and *"Obeying the law ultimately benefits everyone in the community."* Response alternatives for all six measures of legal legitimacy ranged from 1 (*strongly disagree*) to 5 (*strongly agree*). Measurement modeling of this latent construct followed similar procedures, and factor scores for each observation, extracted after conducting a pooled CFA using FIML, indicate that higher scores indicated more normative alignment with law (i.e., more law legitimacy).

We measured identification with healthcare workers. The approach and items were adopted from previous work (Radburn et al. 2018). The prompt read: "*Now, we want you to think about essential health care workers. To what extent do you agree or disagree with the following statements."* The three items were: "*In general, I identify with the health care workers"; "In general, I feel similar to health care workers";* and "*In general, I feel a sense of solidarity with health care workers."* Response alternatives ranged from 1 (*strongly disagree*) to 5 (*strongly agree*). Factor scores were extracted after a pooled CFA, with higher scores indicating higher levels of identification.

We assessed perceived collective efficacy using four items adapted from previous work (Brunton-Smith et al. 2014): "*People in this neighborhood can be trusted"; "This local area is a place where people from different backgrounds get on well together"; "If I sensed trouble whilst in this area, I could get help from people who live here";* and "*The people who live here can be relied upon to call the police if someone is acting suspiciously."* Response alternatives ranged from 1 (*strongly disagree*) to 5 (*strongly agree*). Higher scores of factor scores extracted from a pooled CFA indicated higher levels of perceived collective efficacy. Trust in science was measured using a single indicator: "*How much do you trust scientists to create knowledge that is unbiased and accurate?"* Response alternatives ranged from 1 (*do not trust at all*) to 5 (*completely trust*).

Political ideology was measured by asking: "*Please rate your political ideology on the following scale."* Response options were: extremely liberal (1); liberal (2); sort of liberal (3); centrist (4); sort of conservative (5); conservative (6); extremely conservative (7). We collapsed categories to create liberal or extremely liberal (combining Categories 1–2), versus

everyone else (combining Categories 3–7). Intention to vote for Trump was measured by asking research participants: "*In the upcoming 2020 election, what is the likelihood that you would vote for … Donald Trump.*" Response alternatives ranged from 1 (*extremely unlikely*) to 7 (*extremely likely*). We collapsed categories to create intended Trump voters (combining Categories 6–7) and non-Trump voters (combining Categories 1–5).

We measured racialized beliefs about crime at Wave 3 by asking research participants the extent to which they agreed or disagreed with the following two statements: "*People of color disproportionately commit more crime than White people*"; and "*Racial/ethnic disparities in policing can be explained by disproportionate offending by people of color.*" Response alternatives ranged from 1 (*strongly disagree*) to 5 (*strongly agree*). The mean of the two items was taken to create a scale that we used in the supplemental and exploratory analysis.

Finally, we measured identification with the police and identification with BLM. The approach and items were adopted from previous work (Radburn et al. 2018; Jackson et al. 2023b). The prompt for identification with police read: "*Thinking about the police in your neighborhood, to what extent do you agree or disagree with these statements about the police?*" The three items were: "*In general, I identify with the police"; "In general, I feel similar to the police*"; and "*In general, I feel a sense of solidarity with the police.*" Response alternatives ranged from 1 (*strongly disagree*) to 5 (*strongly agree*). For identification with BLM, the same three items and answer scale were used, simply replacing "police" with "the Black Lives Matter movement/cause." Each was measured following the same analytic procedures as other latent constructs, with factor scores extracted after a pooled CFA and higher scores indicating higher levels of identification. These two scales were used in the supplemental and exploratory analysis.

Descriptive statistics by state and for the full sample are presented in Table 1.

## Analytic Strategy

Our goal is to explore changes in perceptions of police before and after the murder of George Floyd. While it would be of substantive interest to estimate the effects of this episode on changes in public perceptions of police, making strong causal claims would be beyond the scope of this study as the necessary assumptions to identify unbiased effects would be unrealistically strong. Yet, our research question is a causal question at its

core. We draw on the potential outcomes framework (Morgan and Winship 2015) as a general methodological framework to inform our analytic strategy. Even though we cannot identify credible causal estimates, we do leverage our unique dataset in such a way that allows us to approximate—as much as possible—an estimate of the impact of George Floyd's murder on changes in public perceptions of police in the United States.

With repeated measures, one standard approach to make causal inference is the difference-in-differences design. This would imply, for example, in a hypothetical two-period scenario (e.g., before and after the George Floyd's murder), comparing change scores of police perceptions between those who were exposed to the George Floyd murder (i.e., a "treatment" group) and those who were not (i.e., a "control" group). Under a set of assumptions, this comparison could represent the average treatment effect among treated units. However, we cannot apply this design to our data. Given that, realistically, most/all citizens were exposed to George Floyd's murder and the subsequent protests in the United States, we do not have a group of respondents who were not exposed to serve as the control group. As a consequence, we cannot rely on the traditional difference-in-differences design.

When all units are exposed to a treatment, and when data for a relatively long period is available, one can use interrupted time-series analysis. This involves analyzing trends before and after an intervention—in this case, it would consist of trends in public perceptions of police before and after George Floyd's murder. Again, however, we cannot apply this design to our data. Interrupted time series analyses require time series data, and in this study, we are drawing on repeated measures data from a three-wave survey. As such, we do not have the data necessary to conduct time series analyses.

To overcome these challenges and still leverage the unique timing of our sample, we adopt a novel analytic approach that draws on the logic of both difference-in-differences and interrupted time series. Our analytic strategy focuses on modeling within-unit change using respondents' *change scores*[4] prior to the intervention as the baseline to estimate the counterfactual change scores of treated units in the absence of treatment. We apply the interrupted time series logic of considering all units as members of the control group before the intervention, and then as members of the treatment group after the intervention—that is, every unit belongs to the control group before George Floyd's murder, and then to the treatment group afterwards—to a type of difference-in-differences estimator that compares differences in change scores between the two groups.

We explain our modeling strategy in greater detail in the Appendix. But in summary: we calculate change scores of outcome variables between Waves 1 and 2 and then between Waves 2 and 3; and because the murder of George Floyd and sudden surge in protests against police killings of unarmed Black men happened between Waves 2 and 3, we consider the latter to represent outcome scores of the treatment group and the former, because no similar scale meaningful event happened between Waves 1 and 2, to indicate outcome scores of the control group. Since every respondent belongs to the control group at first and then moves to the treatment group, we use each respondent's change scores between Waves 1 and 2 as the baseline to estimate the counterfactual change scores among those same respondents, between Waves 2 and 3, in the counterfactual scenario where they were not exposed to Floyd's murder.

We then organize the data set in such a way that our unit of analysis consists of respondent-change observations: each respondent has two rows in the data set, one indicating change scores from Waves 1 to 2, and one indicating change scores from Waves 2 to 3, as well as a new variable indicating treatment (i.e., changes from Waves 2 to 3) or control status (i.e., changes from Waves 1 to 2). We then regress change scores of each outcome variable on this treatment variable. Formally, we regress:

$$\Delta y_{i,t'} = \alpha_i + \gamma \cdot T_{i,t'} + \epsilon_{i,t'}$$

where $\Delta y_{i,t'}$ represents change scores of an outcome variable observed by respondent $i$ at period $t'$, $\alpha_i$ is a fixed intercept for unit $i$, $T_{i,t'}$ represents the treatment variable, and $\epsilon_{i,t'}$ represents a disturbance term. Crucially, there are only two periods $t'$: $t' = 0$ for changes between Waves 1 and 2, and $t' = 1$ for changes between Waves 2 and 3. This is also the definition of the treatment variable $T_{i,t'}$: $T_{i,t'} = 0$ if $t' = 0$ (i.e., change scores of all respondents between Waves 1 and 2 comprise the control group), and $T_{i,t'}$: $T_{i,t'} = 1$ if $t' = 1$ (i.e., change scores of all respondents between Waves 2 and 3 comprise the treatment group). We include individual fixed effects to account for respondent overrepresentation in the analysis (i.e., each respondent has two change scores computed) and focus on within-respondent change only.

This strategy, therefore, consists of comparing differences in change scores, which is aligned with the logic of the difference-in-differences estimator, particularly in the simple two-group, two-period scenario. Accordingly, in line with the traditional parallel trends assumption, our analytic strategy could yield unbiased causal estimates if we assume that

respondents' change scores between Waves 1 and 2 are a safe proxy for respondents' counterfactual change scores between Waves 2 and 3 in the absence of treatment. Of course, this is an untestable assumption. And even under this assumption, it is important to highlight that the treatment variable is exclusively an indicator of period. This implies that: (1) the treatment is everything that has happened to the respondents between Waves 2 and 3; and (2) the effects are sensitive to time-varying confounders within the three-week period between waves.

We estimated separate models for each outcome variable: change scores in perceived procedural justice, perceived distributive justice, perceived bounded authority, police legitimacy, and law's legitimacy. All models included individual fixed effects so that only within-respondent change was considered, and all potential time-constant confounders were controlled for. We assumed that there were few potential time-varying confounders because observations were only three weeks apart, nonetheless we did control for changes in police-initiated encounters, citizen-initiated encounters, and crime victimization. Models were estimated with ordinary least squares regression and robust standard errors clustered at the respondent level.

To bolster the robustness of our findings, we conducted pseudo-placebo tests as an additional analytical measure. The underlying premise of this study, rooted in procedural justice theory, posits that the exposure to George Floyd's homicide by police officers along with ensuing protests had a negative impact on public perceptions of police. Our modeling strategy relies on the assumption that, absent this episode of police brutality, public perceptions of the police would have exhibited relative stability within a brief three-week period. Consequently, we constructed models to assess the degree to which George Floyd's murder influenced other nonpolice-related variables, namely public perceptions of collective efficacy, trust in science, and identification with healthcare workers. Our driving motivation to conduct these pseudo-placebo tests is that these variables remain unaffected by the incidents of police brutality.

Note that we also tested interactions with state, and there were no significant differences, suggesting that the results are similar across states.

Next, we conducted the sensitivity analyses. We started by testing two sets of interaction effects. The first set involved self-placement of political views (dichotomized as "extremely liberal" or "liberal" versus the rest). The second set involved intentions to vote for Donald Trump in the upcoming Presidential Election (dichotomized as "extremely likely" or "likely" versus the rest).

Finally, in supplemental and exploratory analysis, we examined differences between participants who intended to vote for Trump (versus those who did not) on key variables, including racialized beliefs about crime, identification with the police, and identification with BLM.

## Results

### *Descriptive Analysis*

We begin by providing the trajectories of raw average scores of all variables of interest across the three waves. We compare the average trajectories of the different types of perceptions of police and law: perceived procedural justice, perceived distributive justice, respect of boundaries, normative alignment with the police, moral duty to obey the police, normative alignment with the law, and moral duty to obey the law. As expected, all constructs appeared stable between waves one and two, before lowering between waves two and three, albeit duty to obey the law exhibited the least amount of change (Figure 1a).

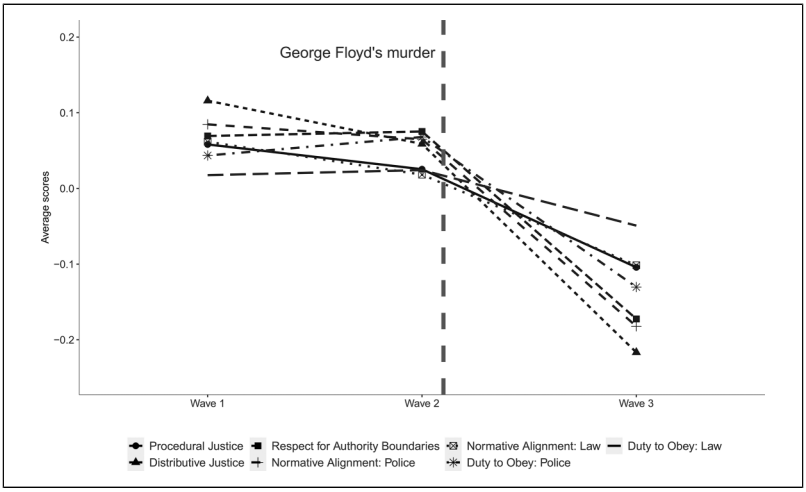


**Figure 1a.** Average trajectories of participants' perceptions of police and law over time. Note: The *y*-axis depicts factor scores extracted from confirmatory factor analysis models (one separate pooled CFA for each latent construct). They are normally distributed *z*-standardized scores representing the latent constructs.

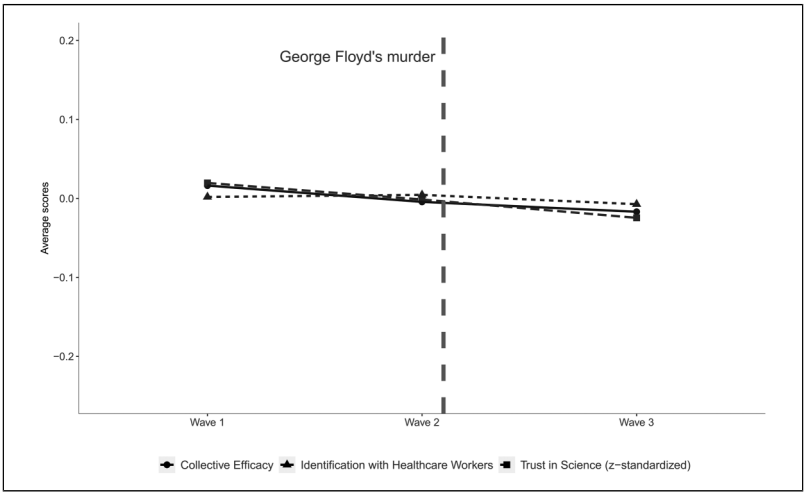


**Figure 1b.** Average trajectories of participants' perceptions over time. Note: The *y*-axis depicts factor scores extracted from confirmatory factor analysis models (one separate pooled CFA for each latent construct). They are normally distributed *z*-standardized scores representing the latent constructs.

Next, we plot the trajectories of raw average scores of the nonpolice-related variables (i.e., the pseudo-control variables), namely: perceptions of collective efficacy, trust in science, and identification with healthcare workers. As expected, unlike public perceptions of police, those variables remained stable over all three waves, suggesting little changes between waves one through three (Figure 1b).

To further explore the dynamics of all variables across the three waves, we provide trajectories of raw average scores of all variables by state. Figure 1c displays trajectories of all police-related-variables, whereas Figure 1d displays trajectories of all nonpolice related variables.

States of New York and Texas behave in a relatively similar way, with all six variables depicting different aspects of perceptions of police remaining constant between Waves 1 and 2, and then dropping between Waves 2 and 3. In Michigan, four out of the six variables display a similar pattern —the exceptions being perceptions of procedural justice and duty to obey the law, which remain somewhat constant between Waves 2 and 3. Finally, in Arizona, while three variables (normative alignment with the police, respect for authority boundaries, and perceptions of distributive justice) follow a similar pattern of dropping between Waves 2 and 3,

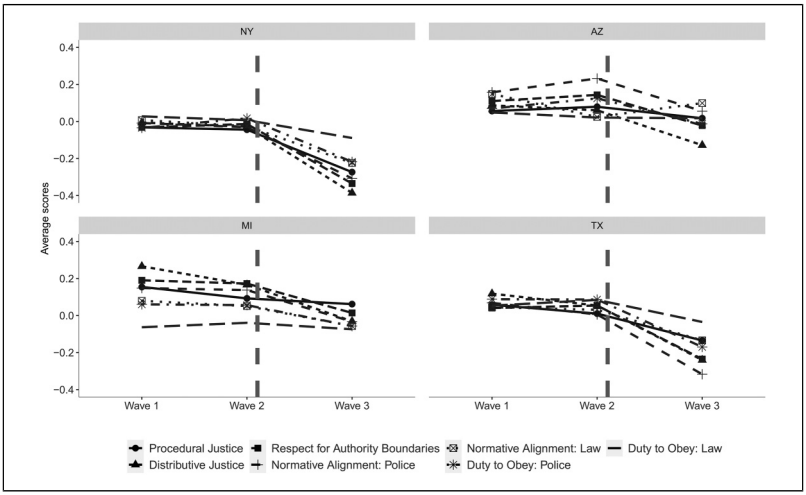


**Figure 1c.** Average trajectories of participants' perceptions of police and law over time by state. Note: The *y*-axis depicts factor scores extracted from confirmatory factor analysis models (one separate pooled CFA for each latent construct). They are normally distributed *z*-standardized scores representing the latent constructs.

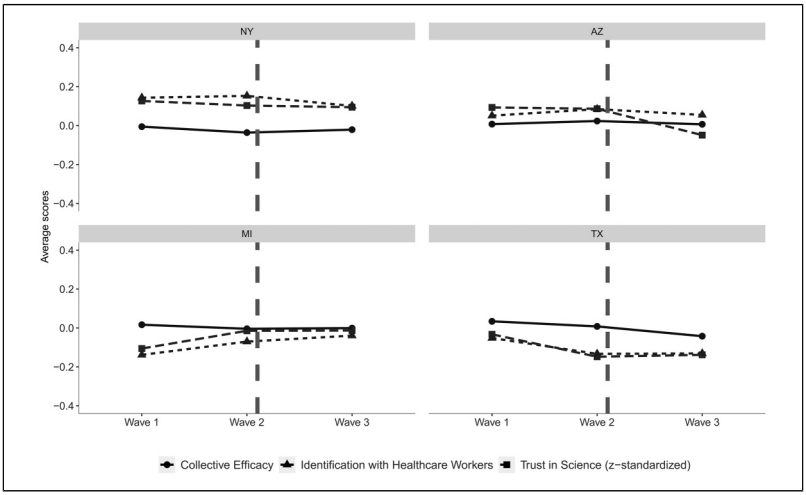


**Figure 1d.** Average trajectories of participants' perceptions over time by state. Note: The y-axis depicts factor scores extracted from confirmatory factor analysis models (one separate pooled CFA for each latent construct). They are normally distributed z-standardized scores representing the latent constructs.

normative alignment with the law and perceptions of procedural justice remain relatively constant. All three non-police-related variables remain relatively constant across the three waves in all four states used in this analysis. It is important to note, however, that these are within-state depictions of the raw unadjusted means that do not account for any demographic characteristics.

## *Inferential Analysis*

We estimated the effects of Floyd's murder on changes in respondents' perceptions of police and the law. As described above and in the Appendix, the estimation strategy used change scores between Waves 1 and 2 as the control and change between 2 and 3 as the treatment group.

Table 2 and Figure 2a present the findings from models estimating the potential impact of events that transpired between Waves 2 and 3—most notably, Floyd's murder—on changes in perceptions of police. Respondents reported more negative perceptions of police and more negative perceptions of the legitimacy of the law following Floyd's murder. In Wave 3, respondents reported worse perceptions of police procedural justice ($\hat{\gamma} = -0.13$ standard deviations in comparison with changes observed between Waves 1 and 2) and distributive justice ($\hat{\gamma} = -0.27$), as well as respect for police authority boundaries ($\hat{\gamma} = -0.27$). Respondents also reported lower scores on perceptions of police legitimacy. In comparison to prior changing patterns, after Floyd's murder, respondents had an average drop of $\hat{\gamma} = -0.23$ standard deviations in normative alignment with the police and an average drop of $\hat{\gamma} = -0.22$ standard deviations in duty to obey the police. Respondents' judgments about the legitimacy of the law may also have been affected by the events between Waves 2 and 3: we estimated an average drop of $\hat{\gamma} = -0.09$ standard deviations in their scores of normative alignment with the law, although the upper bound of the 95% confidence interval (estimated based on robust standard errors clustered at the respondent level) just crossed zero, and an average drop of $\hat{\gamma} = -0.11$ standard deviations in their scores of duty to obey the law.

To further investigate the extent to which our results might be attributable to the aftermath of George Floyd's murder, we explored potential pseudo-placebo effects. We conducted the same analysis comparing changes between Waves 2 and 3 with changes between Waves 1 and 2, but now primarily focused on nonpolice-related variables. As expected, we found little evidence of differences in change scores of respondents' levels of trust in science, collective efficacy and identification with healthcare workers across the three waves (Figure 2b).

**Table 2.** Findings from the Difference-in-Changes Modeling.

| | Procedural Justice | Distributive Justice | Respect for Authority Boundaries | Normative Alignment: Police | Duty to Obey: Police | Normative Alignment: law | Duty to Obey: Law |
|---|---|---|---|---|---|---|---|
| Intercept | −0.13* | −0.06* | −0.26* | 0.12* | 0.11* | 0.05 | 0.06* |
| | [−0.17, −0.10] | [−0.11, −0.01] | [−0.30, −0.22] | [0.07, 0.16] | [0.06, 0.17] | [−0.00, 0.09] | [ 0.01, 0.10] |
| Treatment | −0.13* | −0.27* | −0.27* | −0.23* | −0.22* | −0.09 | −0.11* |
| | [−0.20, −0.06] | [−0.38, −0.17] | [−0.35, −0.18] | [−0.32, −0.14] | [−0.33, −0.11] | [−0.19, 0.00] | [−0.19, −0.03] |
| Change: police stop | −0.03 | −0.07 | −0.01 | 0.08 | 0.03 | −0.05 | 0.02 |
| | [−0.14, 0.09] | [−0.24, 0.10] | [−0.14, 0.12] | [−0.09, 0.25] | [−0.14, 0.20] | [−0.21, 0.12] | [−0.14, 0.17] |
| Change: citizen-initiated contact | 0.03 | 0.05 | −0.07 | −0.01 | 0.15 | 0.14 | −0.01 |
| | [−0.09, 0.15] | [−0.14, 0.23] | [−0.22, 0.08] | [−0.18, 0.16] | [−0.04, 0.34] | [−0.03, 0.30] | [−0.17, 0.15] |
| Change: victimization | −0.04 | 0.07 | −0.00 | 0.07 | −0.16 | −0.03 | −0.01 |
| | [−0.24, 0.15] | [−0.21, 0.35] | [−0.33, 0.32] | [−0.22, 0.36] | [−0.44, 0.13] | [−0.27, 0.21] | [−0.31, 0.30] |
| Fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Clustered standard errors | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of observations | 1418 | 1418 | 1419 | 1416 | 1418 | 1419 | 1415 |
| Number of respondents | 769 | 769 | 769 | 769 | 769 | 769 | 767 |

*$p < .05$.

## Sensitivity Tests

We next tested whether the above putative effects depended on people's prior political views. Contrary to Reny and Newman (2021), we did not find strong and consistent interaction effects for liberals versus nonliberals (full results can be found in Appendix Table A1). For procedural justice, distributive justice, and respect for authority boundaries, the parameter estimates for the interaction effects with the treatment were −0.10, −0.11, and −0.05, respectively, none of which were statistically significant. For police and legal legitimacy, the results were a little more mixed, with the interaction terms for normative alignment with the police and duty to obey being −0.32 (statistically significant) and −0.08 (not statistically significant), and the interaction terms for normative alignment with the law and duty to obey being −0.04 (not statistically significant) and −0.15 (statistically significant).

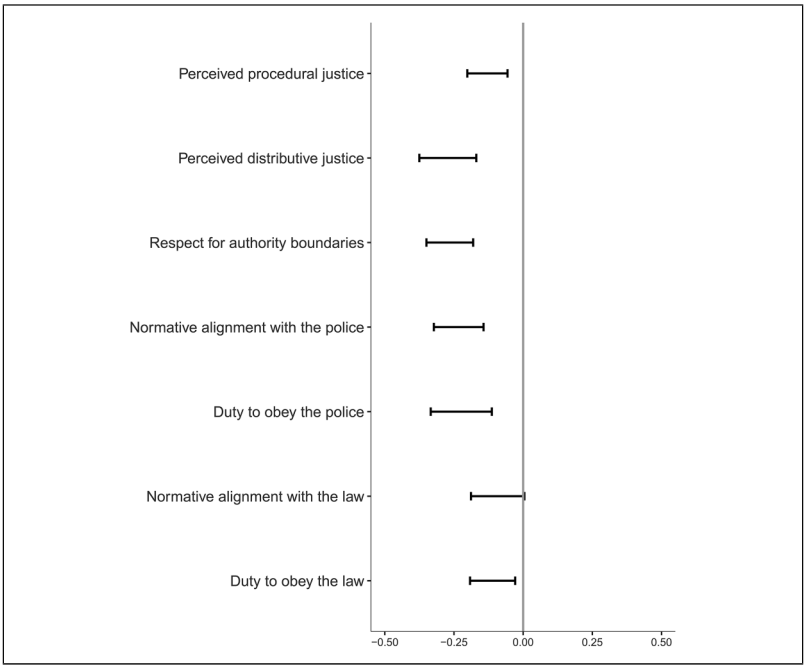


**Figure 2a.** Changes in participants' perceptions of police and law after George Floyd's murder. Note: The y-axis depicts factor scores extracted from confirmatory factor analysis models (one separate pooled CFA for each latent construct). They are normally distributed z-standardized scores representing the latent constructs.

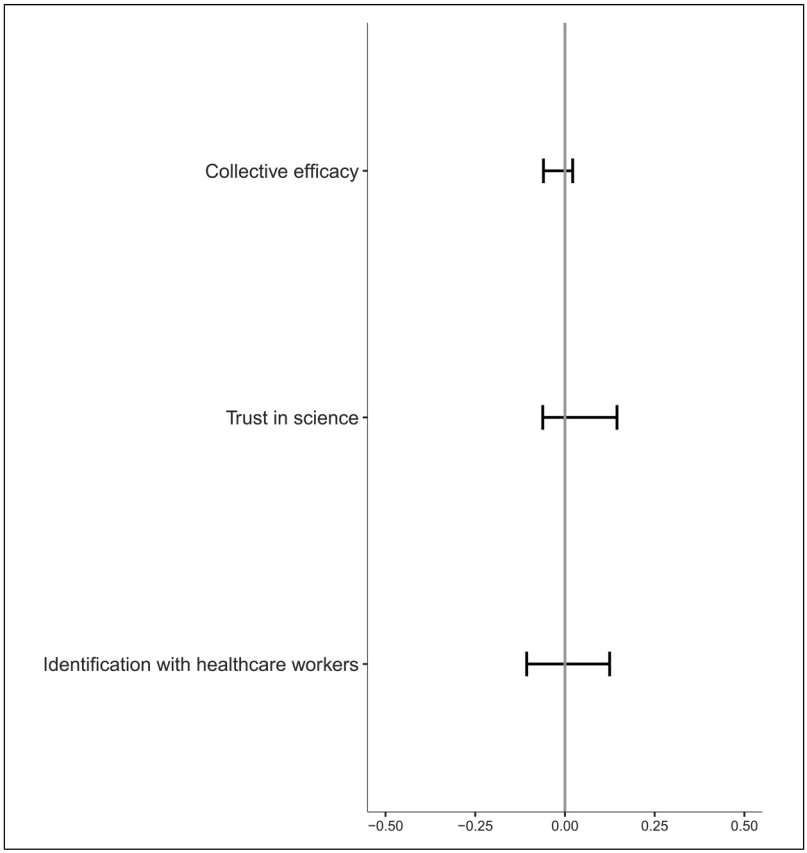


**Figure 2b.** Changes in participants' perceptions after George Floyd's murder. Note: The y-axis depicts factor scores extracted from confirmatory factor analysis models (one separate pooled CFA for each latent construct). They are normally distributed z-standardized scores representing the latent constructs.

The results were, however, stronger for the test of whether being an intended Trump voter moderated the effect (Figure 2c and Table 3 and 4). For procedural justice, distributive justice, respect for authority boundaries, and normative alignment with the police, the interaction terms were 0.31, 0.36, 0.23, and 0.26 respectively, all of which were statistically significant. For duty to obey the police, normative alignment with the law, and duty to obey the law, the interaction terms were 0.12, 0.12, and 0.09 respectively, none of which were statistically significant. Figure 2c shows that the 95%

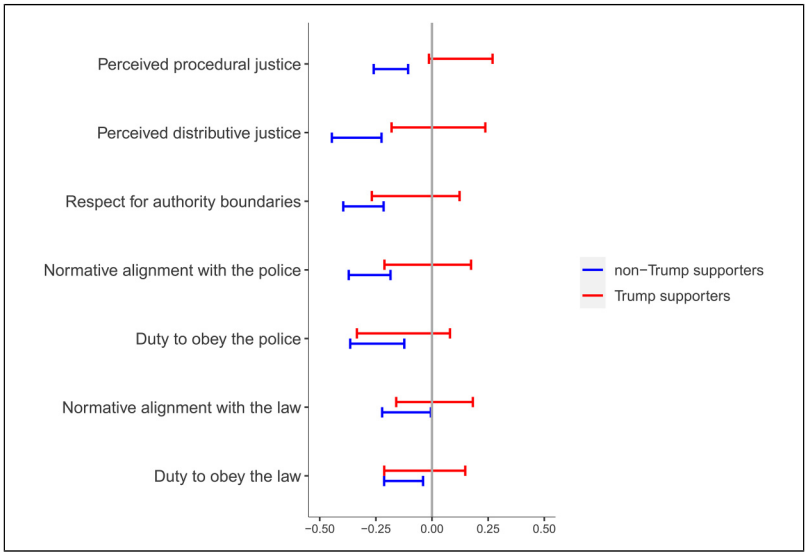


**Figure 2c.** Changes in participants' perceptions after George Floyd's murder by whether participants were intended trump voters. Note: The y-axis depicts factor scores extracted from confirmatory factor analysis models (one separate pooled CFA for each latent construct). They are normally distributed z-standardized scores representing the latent constructs. Trump supporters refers to being intended Trump voters in the election, and non-Trump supporters refers to participants who did not intend to vote for Trump.

confidence intervals for all of the coefficients for intended Trump voters overlapped with zero, unlike those for non-Trump voters, indicating that the drops in procedural justice, distributive justice, respect for authority boundaries, and normative alignment for the police were specific to participants who did not intend to vote for Trump.

## *Supplemental and Exploratory Analysis*

Finally, we sought to shed light on why there seemed to be an impact for non-Trump voters but not for intended Trump voters. Specifically, we examined differences between intended Trump voters and those who did not intend to vote for Trump in the 2020 election. We hypothesized that Trump voters may differ on three key factors: identification with BLM, identification with police, and racialized beliefs about crime. There were no

differences in these participants' locations across states or their education levels. However, as expected, we found that intended Trump voters identified significantly less with the BLM movement, identified significantly more with police, and reported significantly higher racialized beliefs about crime. As such, it is possible that these three factors—racialized beliefs about crime, identification with BLM, and identification with police—may help explain why the killing of Floyd and subsequent resurgence in BLM had no impact on intended Trump voters. In short, they tended to side with the police rather than BLM ideals.

## Discussion

In May of 2020, a Minneapolis police officer murdered George Floyd. The videos were quickly shared across the United States and initiated one of the largest social movements in the country's history (Buchanan et al. 2020), likely a crisis in policing (Mourtgos et al. 2020), and one of the most substantial police reform efforts in the nation's history (Buckholz 2021; Silver et al. 2022; Smith 2022). As such, we consider George Floyd's murder and the ensuring protests to be a focusing event in U.S. history (Reny and Newman 2021; Piza and Connealy 2022; Nix et al. 2023) that may have impacted the public's perceptions of police and the law. To examine the impact of George Floyd's murder on the public's perceptions of the fairness and legitimacy of the police and law, we leveraged three waves of repeated measures data from over 1,000 Americans in four states collected from April through June of 2020.

We grounded our study in the procedural justice framework and the GEM, which begin with the belief that people judge the ways police exercise their authority in large part through the relational, identity-relevant signals (of respect, dignity, and neutrality) that police behavior sends to community members. To the extent that police treat people fairly and justly, people become more likely to legitimize police and the broader rule of law that police represent, and then base their behaviors on that legitimation. Procedurally just policing legitimizes law enforcement and the law in the public's eyes, increasing their social identification with the police as well as their likelihood of engaging in behaviors that support the rule of law. Unjust policing, in contrast, undermines the public's perceptions of police and the law.

While scholars are beginning to uncover a wider range of concerns at the heart of legitimacy (Huq et al. 2017; Trinkner et al. 2018; Jackson et al. 2023; Bradford and Jackson 2024), no longitudinal, within-person study

**Table 3.** Findings from the Difference-in-Changes Modeling, with Interaction Effects for Intended Trump Voters (Extremely Likely or Moderately Likely to Vote for Trump in the Next Presidential Election) Versus Non-Trump Voters (the Rest).

| | Procedural Justice | Distributive Justice | Respect for Authority Boundaries | Normative Alignment: Police | Duty to Obey: Police | Normative Alignment: Law | Duty to Obey: Law |
|---|---|---|---|---|---|---|---|
| Intercept | −0.11* | −0.03 | −0.24* | 0.14* | 0.12* | 0.06* | 0.06* |
| | [−0.14, −0.07] | [−0.08, 0.03] | [−0.28, −0.19] | [ 0.09, 0.19] | [ 0.06, 0.18] | [ 0.00, 0.11] | [ 0.02, 0.11] |
| Treatment | −0.18* | −0.34* | −0.31* | −0.28* | −0.24* | −0.11* | −0.13* |
| | [−0.26, −0.11] | [−0.45, −0.22] | [−0.40, −0.22] | [−0.37, −0.18] | [−0.36, −0.12] | [−0.22, −0.01] | [−0.21, −0.04] |
| Intended Trump voter | −0.14 | 0.13 | 0.50* | 0.39* | −0.15 | 0.78* | 0.61* |
| | [−0.31, 0.03] | [−0.20, 0.47] | [0.20, 0.79] | [0.06, 0.72] | [−0.31, 0.02] | [0.52, 1.04] | [0.47, 0.75] |
| Change: police stop | −0.04 | −0.09 | −0.02 | 0.06 | 0.03 | −0.05 | 0.01 |
| | [−0.15, 0.07] | [−0.26, 0.08] | [−0.15, 0.11] | [−0.10, 0.23] | [−0.15, 0.20] | [−0.22, 0.11] | [−0.14, 0.17] |
| Change: citizen-initiated contact | 0.03 | 0.05 | −0.07 | −0.01 | 0.15 | 0.14 | −0.01 |
| | [−0.08, 0.14] | [−0.13, 0.23] | [−0.21, 0.08] | [−0.18, 0.16] | [−0.04, 0.34] | [−0.02, 0.30] | [−0.17, 0.15] |
| Change: victimization | −0.04 | 0.08 | −0.00 | 0.07 | −0.15 | −0.03 | −0.01 |
| | [−0.22, 0.14] | [−0.20, 0.36] | [−0.33, 0.33] | [−0.22, 0.36] | [−0.44, 0.14] | [−0.27, 0.21] | [−0.31, 0.30] |
| Treatment×Trump Voter | 0.31* | 0.36* | 0.23* | 0.26* | 0.12 | 0.12 | 0.09 |
| | [0.16, 0.46] | [0.14, 0.59] | [0.02, 0.44] | [0.06, 0.46] | [−0.11, 0.34] | [−0.07, 0.32] | [−0.10, 0.29] |
| Fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Clustered standard errors | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of observations | 1418 | 1418 | 1419 | 1416 | 1418 | 1419 | 1415 |
| Number of respondents | 769 | 769 | 769 | 769 | 769 | 769 | 767 |

*$p < .05$.

**Table 4.** Supplemental and Exploratory Analysis Comparing Intended Trump voters to Non-Trump Voters in the 2020 Election.

| Variables | Intended Trump Voters | Nontrump Voters | *t*-statistic | Confidence Interval of Mean the Difference | *p*-value |
|---|---|---|---|---|---|
| Racialized beliefs about crime[a] | 3.33 | 2.18 | −10.3 | [0.94, 1.38] | <.001 |
| Identification with police[b] | 2.99 | 2.23 | −8.07 | [0.58, 0.95] | <.001 |
| Identification with BLM[a] | 2.22 | 3.83 | 12.06 | [−1.87, −1.35] | <.001 |
| State | | | | | |
| Arizona | 0.22 | 0.18 | −0.98 | [−0.04, 0.11] | .33 |
| Michigan | 0.20 | 0.25 | 1.22 | [−0.12, 0.03] | .22 |
| New York | 0.23 | 0.29 | 1.46 | [−0.13, 0.02] | .15 |
| Texas | 0.35 | 0.29 | −1.52 | [−0.02, 0.15] | .13 |
| Education Level | | | | | |
| Less than high school | 0.01 | 0.02 | 1.14 | [−0.03, 0.01] | .25 |
| High school or equivalent | 0.41 | 0.44 | 0.63 | [−0.11, 0.06] | .53 |
| Bachelor's degree | 0.38 | 0.35 | −0.57 | [−0.06, 0.11] | .57 |
| Masters, professional, or doctoral degree | 0.20 | 0.19 | −0.34 | [−0.06, 0.08] | .73 |

[a]Assessed at Wave 3.
[b]Wave 1.

has looked at the role of a 'focusing event' (Birkland 1998) like the police killing of George Floyd on this host of outcomes. Studies within the procedural justice paradigm have tended to look at personal and vicarious experience with the police, with vicarious experience with the police typically involving either friends or family members or hearing about incidents in one's local neighborhood. Yet, there is a dearth of within-person evidence of the effects of sudden, high-profile focusing events like the police murder of George Floyd, which stimulated a resurgence in BLM and counter-protests rooted in intergroup competition (e.g., Blue Lives Matter).

Our study's main contribution lies in providing a nuanced analysis of the public's perceptions of police, the law, and pseudo-controls during the period immediately before and after George Floyd's murder by police. Our study leveraged a short-term longitudinal design. While perceptions of the police were largely stable in the period immediately before Floyd's murder, we found that they declined substantially in the aftermath (with the important caveat that attitudes towards police and law did not change for Trump voters, an issue we return to below). The findings were consistent across various ways of measuring the public's perceptions of police, including procedural justice, distributive justice, bounded authority, and legitimacy.

Critically, our study had another unique element that scholars should consider leveraging in future studies using self-report measures, including those on the effects of focusing events. We tested the impact of the focusing event on a host of outcomes that varied in their distance from our central outcome. Logically, as the proximity to the central construct decreases, the effects should decrease. Notably, the changes in people's perceptions of law's legitimacy were approximately half as large as the changes in their perceptions of police legitimacy. This was expected considering the police are the face of the law, yet people may differentiate police from other legal authorities (see Fine et al. 2019).

Moving away from perceptions of policing and the law yielded consistent trends. We did not expect Floyd's murder to have an impact on trust in science, identification with healthcare workers, and perceptions of collective efficacy, so assessing the stability of these pseudo-control variables was important because it would either strengthen or weaken the evidence that public perceptions of police specifically were affected by the events. Indeed, our analyses indicated that participants' trust in science, identification with healthcare workers, and perceptions of collective efficacy did not change during the same 3-week period. This finding suggests that the effects were unique to police and law. More broadly, this finding—that effect sizes

declined as the constructs radiated away from the core of perceptions of police—indicates that studies of focusing events, as well as studies using self-report measures in general, should include metrics that radiate away from the central constructs so that we can determine the extent to which purported findings are not simply artifacts of a flawed study design. Considering enthusiasm for conducting procedural justice scholarship may be outpacing methodological and theoretical clarity (Jackson 2018; Fine et al. 2022), we encourage studies of procedural justice and legitimacy to do so in the future.

Returning to our results, one critical finding is particularly noteworthy. Crucially, the sensitivity tests indicated that people's prior political views seemed to shape how they internalized and interpreted Floyd's murder, the eruption of protests, and the issues being debated (Reny and Newman 2021, cf. Gillion 2012; Wasow 2017; Mazumder 2018). The aforementioned findings on perceptions of the police and law only pertained to non-Trump voters. The final set of supplemental and exploratory analysis showed that intended Trump voters were less likely to identify with BLM, more likely to identify with police, and more likely to report racialized beliefs about crime. These exploratory findings indicate a clear need for more research in this area to illuminate potential causal pathways.

The study has several implications for procedural justice research and theory. First, the police killing of George Floyd, and the ensuing events, appears to have negatively impacted the public's perceptions of police and law, and may have sent important delegitimizing signals to the public, at least among people who did not intend to vote for Donald Trump. Second, it is true that more experimental work is necessary within the procedural justice literature (Nagin and Telep 2020; Tom et al. 2023), yet this study provides some indication that in the wake of an historic, widely publicized police murder, perceptions of police across multiple dimensions appear to have declined. While results could still be biased by unobserved time-varying confounders within a three-week period, the within-subject design and decreasing effect sizes based on proximity to police and law provides added confidence. Third, subsequent researchers and theorists should consider assessing related and unrelated constructs so that they can ensure they can actually differentiate the public's perceptions of police from other authorities, entities, and constructs, especially given the risk of shared method variance. We encourage researchers working on similar issues to those that concern us here to take a similar approach. It would increase confidence in their results if and when changes in procedural justice-related models either: a) do not replicate changes in other, unrelated

constructs; or b) where those other constructs yield weaker effect sizes based on proximity to the constructs of central concern (i.e., perceptions of the police). This is particularly true within procedural justice and GEM scholarship, as studies must differentiate true effects of participants' engagement in specific group-supporting behavior from their engagement in what may be merely moral or helpful behavior.

For law enforcement, the study's implications are clear: murdering community members can impact segments of the public's views of police across the nation. In such cases, people do not seem inclined to give police 'the benefit of the doubt', or to assign blame merely to 'bad apples.' Videos of the killing may have been critical in providing convincing evidence, which sparked public outcry and calls for justice (Chappell 2021). Social media may also have been crucial forgenerating the changes in opinion we observed, with the sharing of vivid and detailed accounts shaping people's reactions. It appears the effects of a few officers' actions or inactions, in one specific incident in one specific state, can and do spread out across the nation.

While experimental research is limited (see Tom et al. 2023), there is a widespread recognition that procedural justice and legitimacy in policing are critical. For instance, President Obama's task force named legitimacy its first pillar of policing and strongly advocated for enhancing legitimacy through engaging in more procedurally just practices. Moreover, many police departments and law enforcement agencies have redefined their missions such that they specifically emphasize procedural justice and legitimacy (Tyler and Nobo 2022). If law enforcement refuse or fail to consistently treat the public with procedural justice and fairness, they are highly likely to undermine their ability to promote the voluntary and consensual compliance and cooperation that they rely on to do their jobs effectively (Cross et al. 2023). The fact that the American public's views of police appear to have declined in recent years and that many community members support defunding or reforming the police (Jones 2021; Fine and Del Toro 2022; Pickett et al. 2022) should be a wakeup call for law enforcement.

This study's strengths lie in its timing, scope, and scale; to our knowledge this is the only dataset that would enable us to assess longitudinally (i.e., within-person) and with such nuance the impacts of Floyd's murder—a critical national event and historic shock—and its aftermath on the public's perceptions of police, the law, and pseudo-controls at these particular time points (i.e., just before and just after). However, there are clear caveats. First, we cannot establish causality. While we join others in viewing Floyd's murder as a focusing event, there may be other factors. For instance,

perceptions may have been shaped, for example, by police departments' responses to Floyd's murder or to the estimated 10,000 protests that ensued across the nation. As such, this may be more akin to a bundled treatment. Second, while we included data from over 1,000 American adults from four states, the study was not nationally representative and likely underpowered to test for differences by race or ethnicity, though it is entirely plausible that there were racial/ethnic differences (Graham et al. 2020; Fine and Del Toro 2022; Pickett et al. 2022).

Third, while we had three waves of data that were each three weeks apart, our analyses were limited because we did not have additional waves to test for stability and decay effects. Considering there were three weeks between waves, the results should be interpreted as immediate, short-term, and possibly short-lived changes in perceptions rather than long-term and stable changes. Fourth, it is plausible that some of the changes in participants' perceptions of police, namely bounded authority, might be due to external factors (e.g., policing related to COVID restrictions). While the methodology employed here helps alleviate this concern, it cannot be ruled out as a potential factor. Finally, it is important to recognize the overall historical context. We collected these data during the COVID pandemic. As such, it is possible that some variables (e.g., identification with healthcare workers) might have been impacted by the timing. Including them in our study and finding different effects, however, helps mitigate concerns of an overall negativity bias driving the central findings. In addition, George Floyd was certainly not the first community member to die at the hands of police. Just two months prior, Breonna Taylor was fatally shot inside her apartment. Her death at the hands of police may have impacted the participants' initial perceptions of police, though we do not have the data to examine if that set in motion a downward, within-person trend.

## *Future Lines of Inquiry*

Before we conclude, it is worth recalling that in the sensitivity and supplemental analyses, we explored some potential reasons why the effects may be different for people with different political views. Our sensitivity tests indicated that the overall effects were weaker for intended Trump voters as compared to participants who did not intend to vote for Trump. When we explored various dimensions on which intended Trump voters may differ from non-Trump voters, these sensitivity tests indicated that intended Trump voters identified significantly more with police, identified significantly less with the BLM movement, and reported significantly higher

racialized beliefs about crime. Jackson et al. (2023b) found that the attitudes of liberals and conservatives towards defunding the police could be usefully framed as intergroup dynamics and intergroup differentiation. To the extent that these constructs tap into elements of identity and also that perceptions of police misbehavior fuel deidentification processes, conducting future research on these particular elements among particular segments of the population may be particularly illuminative. Specifically, they may help illustrate the mechanisms through which prior experiences and existing elements of identity fuel de-identification processes in response to subsequent experiences. Unfortunately, we had a limited variable list in our dataset so we cannot explore alternative explanations or personal characteristics that may differ between the group, let alone parse the mechanisms through which these variables may operate.

## Conclusion

Finally, one critique of procedural justice theory is that we lack a large body of compelling evidence of change in people's views of police as a result of experiencing unfair, inappropriate policing (Nagin and Telep 2017, 2020). While experimental studies that do indicate such effects are increasingly emerging (e.g., Mazerolle et al. 2013; Demir et al. 2020; Weisburd et al. 2022; Tom et al. 2023), many previous studies have been cross-sectional and correlational in nature. More work here, including on the effects of focusing events, is clearly necessary. The extent to which people react to and reflect on police behavior, and as a result update their views of police legitimacy, remains something of an open question. Our study adds to the weight of evidence that suggests such changes can and do occur, even if only certain sections of the population are sensitive to the stimuli involved. It seems that public perceptions of the police did change after the murder of George Floyd, and the results we have described here underline that what police do is important for perceptions of procedural justice, legitimacy, and the other constructs we considered.

While it may be tempting—and presumably often correct—to see perceptions of police as stable over time and indeed 'sticky', resistant to change (Nagin and Telep 2020), the murder of George Floyd demonstrates that events can cut through—at least in the short term. This, in turn, supports one of the key underlying claims of procedural justice theory and the GEM, that people are at least some of the time engaged in a process of considering and re-considering their opinions of and relationships with the police. Evidence from our survey strongly suggests that when exposed to

blatant police wrongdoing, some people at least changed their minds, and, ultimately, police undermine their own legitimacy. Twin challenges for future research are thus: (a) identifying other such cultural, socio-political or inter-personal 'moments', when legitimacy is challenged and in flux, and (b) exploring how long such effects last. There remain a few fundamental challenges and questions for the field: when and to what extent do we see regression (back) to the long-term mean, and under what circumstances do changes becomes embedded, signaling a more fundamental shift in the relationship between police and public?

## Author Contributions

AF and RT conceptualized the study and collected the data. AF, TO, and JJ wrote the majority of the manuscript. TO, KP, and JJ conducted the analyses. RT and BB edited the manuscript.

## Data Availability

The data set is available from the corresponding author on reasonable request.

## Declaration of Conflicting Interests

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## Notes

1. A London-based study found relatively consistent relationships between police-citizen contact, procedural justice, legitimacy and willingness to cooperate across gender, age, and race, as well as levels of crime, deprivation and collective efficacy at the neighborhood level (Jackson et al., 2013). A US-based study set in the south east of the country also found relatively consistent associations across gender, race, age, education, contact, victim status, and perceived neighborhood

conditions (e.g. collective efficacy and fear of crime), this time between procedural justice, distributive justice, and police effectiveness each predicting trust and obligation to obey the police (Wolfe et al., 2016; see also Brown & Reisig, 2019; Sahin et al., 2023). However, other studies have found evidence against this "invariance thesis" (e.g. Pina-Sánchez & Brunton-Smith, 2021; Murphy & McPherson, 2022; Solomon & Ehlinger, 2023).

2. Bounded authority refers to people's perceptions of the intrusiveness of police action with respect to when, where, and why they regulate the community (see Trinkner & Tyler, 2016; Trinkner et al., 2018). Toward this end, it taps into issues distinct from how police behave (e.g., procedural justice) or distribute their resources (e.g., distributive justice). While there is overlap between people's views of bounded authority and beliefs of whether the police are following constitutional law, bounded authority is conceptually distinct (Trinkner et al., 2018).
3. Pooling observations to estimate measurement models is only acceptable if we assume measurement equivalence over time. We assessed measurement equivalence and found evidence that the scales worked in equivalent ways across each of the three waves. Having established measurement equivalence, we pooled observations from all three waves to ensure longitudinal equivalence and estimated each confirmatory factor analysis model (for each latent construct) using full information maximum likelihood estimation to handle missing data. Details about this assessment can be found in the Appendix.
4. Using change scores to model change over time is standard practice in the social sciences (Allison, 1990). Change scores are often utilized in the context of the difference-in-differences estimator (in the two-period scenario, Angrist & Pischke, 2009), the first-differenced estimator for one-way fixed effects models (Wooldridge, 2010), the latent change score model to estimate developmental trajectories (McArdle & Grimm, 2010), and in some time series analyses (Ewusie et al., 2020).

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# Appendix

## *Measurement Equivalence*

With the exceptions of trust in science, identification with healthcare workers, and identification with police (each measured using a single item), we tested separate measurement models for each of our five main outcome variables (perceived procedural justice, distributive justice,

bounded authority, police legitimacy, and law legitimacy), as well as one of the outcome variables used as pseudo-placebos (collective efficacy). The measurement modeling involved estimating a series of pooled confirmatory factor analysis models, checking for measurement invariance across the three waves, extracting of factor scores, then calculating change scores between Waves 1 and 2 and then between Waves 2 and 3.

We tested measurement equivalence for each of the scales. We used robust maximum likelihood estimation and made nested model comparisons using the rescaled difference in the model −2log-likelihood values and difference in model degrees of freedom. We first fitted a configural invariance model for each construct, where the construct at each of the three waves was estimated simultaneously, all factor means were fixed at 0 and at least one factor variance was fixed at 1 for the purpose of identification. We then estimated a metric invariance model (where the factor loadings were set to be equal across the three waves). Note that we do test a scalar invariance model, where the intercepts were also set to be equal across the three waves, since our goal is to assess whether perceptions of police and the law changed over time.

We found evidence that the scales worked in equivalent ways across each of the three waves. For all constructs, the metric invariance model had an comparable fit to the configural invariance model: procedural justice $-2\Delta LL(6) = 3.1$, $p = .79$; distributive justice $-2\Delta LL(10) = 15.7$, $p = .11$; bounded authority $-2\Delta LL(10) = 15.7$, $p = .11$; police legitimacy $-2\Delta LL(6) = 6.6$, $p = .36$; legal legitimacy $-2\Delta LL(6) = 2.7$, $p = .85$; and collective efficacy $-2\Delta LL(8) = 1.7$, $p = .99$.

Having established measurement equivalence, we then pooled observations from all three waves to ensure longitudinal equivalence and estimated a confirmatory factor analysis model for each latent construct, using FIML estimation to handle missing data. We obtained factor scores for all observations. These were normally distributed scores with a mean of 0 and each ranging from approximately −2 to approximately +2. Finally, using extracted factor scores for each variable or the single trust in science item, we computed the difference between Wave 1 and 2 scores, as well as the difference between Wave 2 and 3 scores.

## *Modeling Strategy*

The traditional strategy to leverage longitudinal data to make causal inferences, in a hypothetical two-period scenario (before and after the treatment), would consist of comparing change scores of an outcome variable before

and after an intervention among those who were exposed to the intervention (i.e., the treatment group) and those who were not exposed to the intervention (i.e., the control group). This difference in change scores, otherwise known as the difference-in-differences estimator, yields the average treatment effect among treated units (ATT), if we assume that change scores ATT in a counterfactual scenario where they were not treated would be the same change scores observed by untreated units (i.e., the parallel trends assumption). That is, if we assume that

$$E\left[Y_{it}^{0}|D_i=1\right]-E[Y_{i,t-1}^{0}|D_i=1]=E[Y_{it}^{0}|D_i=0]-E[Y_{i,t-1}^{0}|D_i=0\big],$$

where $Y_{it}$ represents an outcome variable observed by respondent $i$ at period $t$ (e.g., scores of police perceptions), $D_i$ indicates whether an individual belongs to the treatment group (e.g., was exposed to George Floyd's murder), and the superscript corresponds to the potential outcome of exposure to treatment between periods $t$ and $t-1$, then we can simply regress the change scores of the outcome variable on the treatment indicator to obtain the ATT. In this case, by regressing

$$\Delta Y_i=\alpha+\gamma\cdot D_i+\epsilon_i,$$

where $\alpha$ is an estimated intercept and $\epsilon_i$ is a disturbance term, the estimated coefficient $\gamma$ represents the ATT.

However, this was an impossible scenario in our application. In expectation, every respondent of our sample belonged to the treatment group (i.e., all respondents were vicariously exposed to the murder of George Floyd), which implies that we lack a proper control group—a group of respondents who were not exposed to the treatment and whose change scores could serve as the baseline to estimate the counterfactual change scores ATT. Our application is more like the classic interrupted time series design, in which all units belong to the control group prior to the intervention and to the treatment group after the intervention—although, again, we lack proper time series data to conduct an interrupted time series analysis.

To overcome this difficulty, we rely on the logic of the difference-in-differences estimator—that compares change scores between two groups—and the interrupted time series design—that relies on within-unit change before and after an intervention—and develop an alternative analytic strategy that leverages the unique timing of our sample and models within-unit change using respondents' *change scores* prior to the treatment as the baseline to estimate the counterfactual change scores of treated units in the absence of treatment.

To recap, we rely on three waves of data, each three weeks apart. George Floyd was murdered between Waves 2 and 3. Our modeling strategy involves computing two new variables for each outcome variable: change scores between Waves 1 and 2 and change scores between Waves 2 and 3. Because the murder of George Floyd happened between Waves 2 and 3, we consider the latter to represent outcome scores of the treatment group; whereas the former, because no similar scale meaningful event happened between Waves 1 and 2, indicates outcome scores of the control group.

That implies that our data are organized in such a way that our unit of analysis are neither respondents (as in a "wide" data set) nor respondent-wave observations (as in a "long" data set; in this case, each respondent would have three observations). Instead, our unit of analysis consist of "respondent-change" observations: a type of "long" data set in which each respondent has two observations (changes from Waves 1 to 2 and from Waves 2 to 3).

This type of "difference-in-changes" estimator is aligned with the logic of the difference-in-differences estimator. Similar to the parallel trends assumption, $\gamma$ can be identifiable if we assume that respondents' change scores between Waves 1 and 2 are a safe proxy for respondents' counterfactual change scores between Waves 2 and 3 in the absence of treatment. However, the treatment variable is exclusively an indicator of period. This implies that: (i) the treatment is everything that has happened to the respondents between Waves 2 and 3; and (ii) the effects are sensitive to time-varying confounders within the 3-week period between waves.

**Table A1.** Findings from the Difference-in-Changes Modelling, with Interaction Effects for Liberals ("Extremely Liberal" or "Moderately Liberal") Versus Nonliberals (the Rest).

| | Procedural Justice | Distributive Justice | Respect for Authority Boundaries | Normative Alignment: Police | Duty to Obey: Police | Normative Alignment: law | Duty to Obey: Law |
|---|---|---|---|---|---|---|---|
| Intercept | −0.15* | −0.08* | −0.27* | 0.04 | 0.09* | 0.04 | 0.02 |
| | [−0.20, −0.11] | [−0.15, −0.02] | [−0.33, −0.21] | [−0.01, 0.10] | [ 0.03, 0.16] | [−0.02, 0.09] | [−0.03, 0.07] |
| Treatment | −0.09 | −0.22* | −0.24* | −0.09 | −0.19* | −0.07 | −0.04 |
| | [−0.17, 0.00] | [−0.36, −0.09] | [−0.36, −0.13] | [−0.20, 0.02] | [−0.32, −0.06] | [−0.18, 0.04] | [−0.15, 0.06] |
| Liberal | 0.13* | −0.26* | 0.59* | 0.12 | 0.11 | −0.17* | −1.32* |
| | [0.01, 0.25] | [−0.44, −0.09] | [ 0.44, 0.73] | [−0.09, 0.33] | [−0.07, 0.30] | [−0.33, −0.01] | [−1.68, −0.97] |
| Change: | −0.03 | −0.07 | −0.01 | 0.07 | 0.03 | −0.05 | 0.01 |
| police stop | [−0.14, 0.08] | [−0.25, 0.10] | [−0.14, 0.12] | [−0.10, 0.23] | [−0.14, 0.20] | [−0.21, 0.11] | [−0.14, 0.16] |
| Change: citizen-initiated | 0.03 | 0.05 | −0.07 | −0.02 | 0.15 | 0.14 | −0.01 |
| contact | [−0.09, 0.15] | [−0.14, 0.23] | [−0.22, 0.07] | [−0.19, 0.14] | [−0.04, 0.34] | [−0.03, 0.30] | [−0.17, 0.14] |
| Change: | −0.03 | 0.08 | −0.00 | 0.10 | −0.15 | −0.03 | 0.00 |
| victimization | [−0.23, 0.16] | [−0.20, 0.36] | [−0.32, 0.32] | [−0.19, 0.38] | [−0.44, 0.14] | [−0.26, 0.21] | [−0.30, 0.31] |
| Treatment×Liberal | −0.10 | −0.11 | −0.05 | −0.32* | −0.08 | −0.04 | −0.15* |
| | [−0.21, 0.02] | [−0.28, 0.07] | [−0.18, 0.09] | [−0.47, −0.17] | [−0.27, 0.11] | [−0.22, 0.13] | [−0.29, −0.01] |
| Fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Clustered standard errors | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of observations | 1418 | 1418 | 1419 | 1416 | 1418 | 1419 | 1415 |
| Number of respondents | 769 | 769 | 769 | 769 | 769 | 769 | 767 |

*$p < .05$