Aggressive policing and undermined legitimacy: assessing the impact of police stops at gunpoint on perceptions of police in São Paulo, Brazil

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Abstract

Objectives Test the effects of a recent police stop and a recent police stop at gunpoint on changes in attitudes towards the police among residents of Brazil's biggest city.

Methods A three-wave longitudinal survey of São Paulo residents (2015–2019) measured people's beliefs about police legitimacy, expectations of police procedural fairness, effectiveness, and overpolicing, whether they were recently stopped by the police, and whether officers had pointed a gun at them during that stop. A novel causal estimand focused on the effect of change in treatment status is estimated using matching methods for panel data combined with difference-in-differences.

Results While estimates are too imprecise to suggest an effect of a recent police stop on attitudinal change, recent police stops at gunpoint decrease public expectations of procedural fairness, increase expectations of overpolicing, and harm public beliefs of police legitimacy.

Conclusions Under a credible conditional parallel trends assumption, this study provides causal evidence on the relationship between aggressive policing practices and legal attitudes, with implications to public recognition of legal authority in a major Global South city.

Keywords Aggressive policing · Brazil · Causal inference with panel data · Perceptions of police · Police legitimacy · Police stops · Procedural justice

Confrontational proactive policing methods are often used to tackle crime in the USA (Fagan et al. 2016; Manski and Nagin 2017). Previous work suggests that this type of policing approach can indeed contribute to reduce crime: for instance,

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Braga et al. (2018) showed that focused deterrence strategies are associated with crime reduction, and Sharkey (2018) argued that the expansion of criminal justice institutions through aggressive policing strategies and mass incarceration was at least partly responsible for the great crime decline in the country in the twentieth century. However, despite potential benefits in crime reduction, aggressive policing practices based on increased coercive presence might also have a range of negative unintended consequences, including costs in intrusion on the rights and privacy of innocent persons Manski and Nagin (2017). Sharkey (2018) also demonstrated how the reliance on confrontational policing practices has taken a heavy toll. Some of the consequences of the persistent reliance on aggressive policing practices in the USA reported by recent work include worse educational performance among African American boys (Legewie and Fagan 2019), more trauma and anxiety symptoms (Geller et al. 2014), and damages in legal socialization (Geller and Fagan 2019).

It is important to explore adverse effects of aggressive policing practices, especially in a period of increasing globalization and outsourcing of some of those tactics across the Global South (Steinberg 2020; Christensen and Albrecht 2020). The Brazilian case is an instructive example. Street-level policing in Brazilian cities is conducted by highly militarized police forces (Lima et al. 2016), and officers frequently draw their guns even in mundane police-citizen interactions. Most people expect law enforcement agents to regularly engage in aggressive behavior even in non-threatening situations (Jackson et al. 2022), making use-of-force not only a legal prerogative, but rather a constant, salient threat. In Rio de Janeiro, a recent survey estimates that almost 30% of all recent self-reported pedestrian stops involved an officer pointing a gun at the citizen;[1] in São Paulo, various survey estimates reveal that almost 50% of all police stops usually occur at gunpoint (Oliveira 2022; Cardia et al. 2012), the same proportion identified by Pinc 's 2007 systematic social observation of police stops in the city. Mirroring the increasing process of police militarization in the USA (Mummolo 2018), policing tactics in Brazil are firmly centered around ideas of confrontation and coercion (Zanetic et al. 2016).

In this paper, I use the case of self-reported police stops in the city of São Paulo to investigate one potential adverse effect of aggressive policing practices: the undermine of public beliefs in the legitimacy of legal institutions (Tyler and Jackson 2014; Tyler et al. 2014). According to procedural justice theory (PJT), police-citizen interactions can either boost or damage public beliefs of police legitimacy and trustworthiness depending on the extent to which they are perceived as positive or negative encounters (Sunshine and Tyler 2003; Tyler 2006). Previous observational work has found some empirical support for a symmetric association between police contact and perceptions of police (Bradford et al. 2009; Oliveira et al. 2021, and some experimental studies suggest that procedural justice training seems to have a positive impact on legal attitudes (Mazerolle et al. 2013; Sahin et al. 2017). What is yet to be demonstrated, however, is the extent to which *negative* police encounters causally affect perceptions of police (Thompson and Pickett 2021). This is particularly

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[1] https://oglobo.globo.com/rio/pesquisa-mostra-que-negros-sao-maioria-dos-abordados-pela-policia-25394648 (retrieved on February 15, 2020).

pressing in São Paulo, where policing tactics are extensively based on aggressive and confrontational practices (Jackson et al. 2022; Oliveira 2022).

According to Nagin and Telep (2017, 2020), whether police contact has a *causal* impact on legal attitudes has not been established, particularly in terms of negative contacts (Thompson and Pickett 2021). Although important, previous studies that relied upon observational data were not able to establish causal relationships. While experimental designs could theoretically allow for causal claims, they are impractical in this case as it would be unethical to assign police mistreatment (Maguire et al. 2017). Thus, any assessment of the impact of aggressive police-public encounters on perceptions of police needs to rely upon observational data — which is not trivial because causal inference involves comparing potential outcomes that cannot be simultaneously observed (see Morgan and Winship 2015). Leveraging longitudinal data can be a potentially powerful strategy to overcome these difficulties. For instance, previous studies have modeled attitudinal change before and after a police-citizen encounter, focusing on within-individual variation to remove time-constant confounding bias (Slocum and Wiley 2018; Tyler and Fagan 2008; Oliveira et al. 2021). However, the assumptions and modeling strategies necessary to remove time-varying confounders and identify causal effects with panel data (e.g., parallel trends and the correct specification of treatment dynamics) are generally under-appreciated (An and Winship 2017; Vaisey and Miles 2017); in the study of police contact and legal attitudes, they are seldom directly addressed (Nagin and Telep 2017).

In this study, I make use of three waves of longitudinal survey data representative of the adult population residing in selected neighborhoods in São Paulo, Brazil (2015–2019), to assess the impact of aggressive policing practices on perceptions of police. Using a multi-period difference-in-differences design and recently developed matching methods for panel data (Imai et al. 2021), I estimate the causal effects of being recently (self-reportedly) stopped by the police at gunpoint on four aspects of perceptions of police: perceived procedural fairness, perceived police effectiveness, perceived overpolicing, and beliefs about police legitimacy. Building on recent discussions about the challenges involved in inferring causality with panel data and the limitations of some commonly used estimators (Goodman-Bacon 2018; Callaway and Sant' Anna 2020; Imai and Kim 2020), I focus on a novel causal estimand that emphasizes *change in treatment status* to estimate the effects of a recent police stop at gunpoint among respondents with no police stop experiences at a previous point in time: the average treatment effect among units that recently moved into the treatment group, which I label ATT_{change} Imai et al. (2021).

This paper makes two contributions to studies on public-police relations. First, I extend the discussion about adverse consequences of confrontational proactive policing methods (Manski and Nagin 2017) to a city in the Global South where police officers frequently threaten members of the public with their guns, and demonstrate how the experience of being stopped and questioned by law enforcement agents at gunpoint undermines public judgements about the legitimacy of the police among São Paulo residents. Second, from a methodological perspective, I partly address the gap in the literature identified by Nagin and Telep (2017, 2020) and argue that any posited relationship between the experience of aggressive policing and perceptions of police is best viewed as a causal account about *dynamic change*

over time; while longitudinal data can be powerful to model change in public opinion (see Pina-Sánchez and Brunton-Smith 2020), it is important to draw on the potential outcomes framework (Imbens and Rubin 2015) and/or directed acyclic graphs (DAGs) Pearl (2009) to explicitly define the targetted causal estimands and the necessary assumptions involved.

The rest of the paper proceeds as follows. In the next section, I review the literature discussing the relationship between police stops and perceptions of police. I rely on the PJT framework to argue why negative contacts affect public beliefs about police legitimacy and trustworthiness. I then discuss policing in the city of São Paulo, Brazil, a low-trust, high-violence context where officers frequently draw their guns. After that, I discuss causal inference with panel data, emphasizing the definitions of causal estimands, the logic of the difference-in-differences design, and applications in the case of aggressive policing and perceptions of police. The next section highlights the hypotheses, presents the data and measures, displays some descriptive statistics, and presents the estimation strategies. The following section shows the results, highlighting the effects of police stops at gunpoint on changes in perceptions of procedural fairness, police effectiveness, and police legitimacy. I conclude with a discussion on the social costs of aggressive and confrontational policing practices in a Global South city and its implications to public perceptions of police, some limitations of this study, and some potential avenues for future research.

Aggressive policing and the undermine of legal attitudes

According to Manski and Nagin (2017), effective policing in a democratic society must balance the sometime conflicting objectives of public safety and community trust. For instance, confrontational proactive policing tactics, such as the widespread use of stop-and-frisk powers and the strict enforcement of low-level crimes, promise important social benefits through crime reduction (see Kubrin et al. 2010; Tiratelli et al. 2018; Sharkey 2018), but often at the cost of increased intrusion on the privacy of members of the public (Bradford 2017; Rios et al. 2020) and the disproportionate targeting of certain ethnic minorities (Suss and Oliveira 2022; Gelman et al. 2007; Epp et al. 2014). In this context, Manski and Nagin (2017) developed a formal model that takes into account hypothetical costs and benefits and returns the optimal rate of law enforcement. Yet, as highlighted by the authors, there are no quantified estimates of the value of the social costs of aggressive policing methods to properly assess their balanced benefits in crime reduction — even though the costs seem high (Fagan et al. 2016). It is therefore crucial to better understand the adverse consequences of aggressive policing, particularly in terms of undermined legal attitudes (Tyler et al. 2015).

Previous work suggests that increased police contact, including involuntary police stops, is associated with damages in public confidence in policing. For instance, Geller and Fagan (2019) showed that the experience of being repeatedly stopped by officers conducting intrusive investigatory searches damaged the process of legal socialization among adolescents in New York City. Similarly, Hagan and colleagues analyzed data from thousands of Chicago public school students and concluded that

adolescents who had more police contact were more likely to agreeing with statements such as "people from my racial group are more likely to be unfairly stopped and questioned by the police" and "police treat rich people better than poor people" (Hagan et al. 2005, p. 388), suggesting a potential link between increased police contact and perceptions of injustice (see also Shedd 2015; MacDonald et al. 2007; Augustyn 2016). Nationally, Kirk et al. (2012) demonstrated that tough immigration enforcement was associated with increased levels of cynicism toward legal institutions. This connects to a large body of survey evidence suggesting that the experience of poorly handled involuntary encounters with police officers undermines trust and legitimacy — both in Western societies (Skogan 2006; Oliveira et al. 2021; Thompson and Pickett 2021) and in the Global South (Piccirillo et al. 2021; Komatsu et al. 2020; Jackson et al. 2014).

The suggestion from this work is that aggressive policing practices that communicate suspicion of ongoing or future criminal conduct and intrude upon the lives of members of the public can harm public-police relations (Tyler et al. 2015; Oliveira 2022) — although it is yet to demonstrate evidence of a causal relationship (Nagin and Telep 2017, 2020). Theoretically, the mechanisms that explain how and why inappropriate police behavior damages public beliefs about police legitimacy and trustworthiness are premised on procedural justice and legal cynicism frameworks (Sunshine and Tyler 2003; Kirk and Papachristos 2011), both of which highlight the importance of police-citizen encounters in shaping legal attitudes.

Procedural justice theory

PJT posits that every public-police interaction is a potential teachable moment during which values about how police power is usually exercised are passed on Tyler et al. (2014). People evaluate the normative appropriateness of the exercise of power and update their views about legal institutions' right to rule and authority to govern (Gur and Jackson 2020). This is premised on the idea that people have normative expectations about the exercise of legal power; when legal agents exert their power (e.g., during police stops), people evaluate whether that corresponds to their normative expectations about how power should be wielded (Huq et al. 2017; Gur and Jackson 2020). To the extent that they judge legal agents to be trustworthy to exert power as normatively expected, social bonds between legal authority and the public are strengthened (Oliveira and Jackson 2022), leading to a stronger public recognition of legal authority as the rightful authority (Lind and Tyler 1988; Tyler 2006; Tyler and Jackson 2014). Exposure to intrusive and intimidatory policing practices, on the other hand, could lead to expectations that officers usually exert power in normatively inappropriate ways, contributing to undermine public judgements about the legitimacy of the legal institutions (Oliveira 2022).

The public can be constantly exposed to legal agents exerting power. For instance, people can witness officers interacting with citizens (Oliveira et al. 2022), contact the police to report a crime or provide information and intelligence (Bradford et al. 2009), hear of friends and neighbors' experiences with law enforcement (Rios 2011), read about episodes of police violence on the news or on social media

(Desmond et al. 2016), among other scenarios. In this paper, I focus on one specific situation: the experience of being stopped by an officer for investigatory purposes; specifically, I consider police stops during which an officer points a gun at the citizen, an undeniably aggressive and yet not a rare practice among law enforcement agents in São Paulo. My goal is to assess whether a recent police stop at gunpoint causes changes in expectations about how officers tend to behave and in beliefs about police legitimacy.

Which aspects of police behavior people tend to evaluate during police stops is an empirical question (Oliveira and Jackson 2022; Huq et al. 2017). For instance, PJT hypothesizes that most people expect power to be exercised with procedural fairness: respect and dignity in treatment, and open and transparent decisions (Tyler 2006). As such, it is important to examine the impact of police stops at gunpoint on changes in perceptions of procedural fairness. But apart from procedural fairness, people could have other normative expectations about the exercise of police power.[2] In some Global South cities where the ability of the state to control crime is low, the extent to which police officers are effective at fighting crime has also been identified as a potential source of legitimacy beliefs (e.g., Tankebe 2009; Sun et al. 2017). Therefore, it is also important to examine the impact of police stops at gunpoint on changes in perceptions of police effectiveness. Additionally, recent work has emphasized the importance of perceived police intrusion, suggesting that people assess the degree to which they expect officers to over-patrol their communities and repeatedly intrude upon their lives (Oliveira 2022; Tyler et al. 201, 2015). It is thus equally important to explore the effects of police stops at gunpoint on changes in perceptions of overpolicing.

But crucially, it is of paramount importance to assess the effects of police contact, including negative contacts based on aggressive and confrontational policing practices, on public judgements about police legitimacy. Defined as the recognition, among citizens, that the law and its agents are the rightful authority and, as such, should expect people to consent to legal directives (Beetham 1991; Tyler 2006; Gur and Jackson 2020), public beliefs about the legitimacy of legal institutions have been linked to greater willingness to voluntarily comply with the law and to cooperate with legal authorities (see Walters and Bolger 2019; Bolger and Walters 2019 for two recent meta-analyses). Whether negatively perceived public-police encounters have a causal impact on police legitimacy beliefs is an important gap in the literature (Nagin and Telep 2017, 2020; Thompson and Pickett 2021).

In this study, I focus on the direct effects of police stops on three expectations of police behavior — perceptions of procedural fairness, perceptions of police

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[2] The discussion about whether a given aspect of police conduct is a legitimating norm is beyond the scope of this paper, but in a nutshell, Tyler's process-based model emphasizes procedural fairness because it sends identity-related messages of group status and value (Lind and Tyler 1988; Tyler and Huo 2002); by experiencing procedural fairness, people feel like they are valued members of the group legal authority represents. It is possible that other features inscribed in law enforcement agents' exercise of power also send identity-related messages. For more discussions about how other aspects of police behavior beyond procedural fairness contribute to enhance or undermine legitimacy beliefs, see Gur and Jackson (2020); Trinkner et al. (2018); Oliveira and Jackson (2022); Oliveira (2022).

effectiveness, and perceptions of overpolicing — and on public beliefs about police legitimacy.[3] Crucially, I focus on one specific type of aggressive policing practice: the experience of being stopped by a police officer at gunpoint in São Paulo. While there is some evidence on the impact of positive contacts on perceptions of police (Mazerolle et al. 2013; Sahin et al. 2017), the causal effects of *negative* experiences with law enforcement agents on attitudes towards legal authority are still being assessed by the literature (Thompson and Pickett 2021. By analyzing the causal effect of a recent police stop at gunpoint on perceptions of procedural fairness, police effectiveness, overpolicing, and police legitimacy beliefs in the city of São Paulo, I am partly addressing the gap in the literature identified by Nagin and Telep (2017, 2020).

Policing in São Paulo, Brazil

The city of São Paulo is a place in which income, health, safety, and even the rule of law are distributed with huge unevenness. With almost 20 million residents in the metropolitan area, the city is very densely populated, but with enormous differences from one neighborhood to the next (Marques et al. 2016). Residents of some structurally disadvantaged communities not only need to deal with the lack of welfare provision, but also face the threat of violence on a regular basis. Crime rates are high: while some wealthy, white neighborhoods have a null homicide rate, some poor and racially diverse districts have a rate of almost 20 homicides per 100,000 residents — considering how densely populated the city is, these are extremely high figures (Nery et al. 2012). Extralegal governance by organized crime is an additional source of threat in some neighborhoods. A highly bureaucratized criminal organization — the "Primeiro Comando da Capital" (PCC) — dominates several areas in the city, essentially governing and claiming power over a large number of residents (Lessing and Willis 2019; Willis 2015; Biderman et al. 2018).

On top of concerns about crime rates and organized crime, São Paulo residents face another source of constant threat: the state itself. With an authoritarian history characterized by colonialism, slavery and dictatorships (Schwarcz and Starling 2015),[4] the Brazilian state has continuously contributed to the widespread cultural

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[3] The impact of police stops on legitimacy beliefs is premised to be mediated by some task-specific expectations of police behavior that are legitimating norms (Pósch 2021) — e.g., perceptions of procedural fairness, but potentially also police effectiveness and overpolicing, among other assessments of police conduct (see Trinkner et al. 2018; Oliveira 2022). As a result, there should be both direct and indirect effects of police stops at gunpoint on police legitimacy. While estimating indirect effects of police contact on legitimacy beliefs through changes in task-specific assessments of police conduct could also be of interest to the literature, efforts in causal mediation analysis are beyond the scope of this article (see Pósch 2021, 2019; Pósch et al. 2021 for recent studies assessing causal indirect effects).

[4] Brazil has been under democratic rule since 1988, but authoritarianism has always been a distinctive characteristic of its history. The country went from Portugal's colonization rule to a monarchic system under an emperor's rule (1822–1889) and was the last country in the Americas to abolish slavery (in 1888). In the twentieth century alone, amid an oligarquic republic (until 1930), a populist dictatorship (1930–1945), and a military dictatorship (1964–1985), Brazil has lived under democratic rule for only one-third of the century (Schwarcz and Starling 2015).

understanding that the only way to exert power is through the concrete threat of violence, in what (Pinheiro 1991) has called a "socially rooted authoritarianism."

In that respect, policing tactics are mostly centered around ideas of confrontation and coercion, and episodes of police brutality, including use of lethal force, are not rare (Sinhoretto et al. 2016). Street-level patrolling in São Paulo is conducted by the Polícia Militar de São Paulo (PMSP), a militarized police force.[5] Mirroring the increasing process of police militarization in the USA (Mummolo 2018), the PMSP employs a combination of equipment, tactics and culture developed for theatres of war, with officers wearing highly visible uniforms, constantly carrying firearms, and patrolling neighborhoods expecting danger and confrontation (Pinc 2007). Unlike in the USA, however, militarized forces in Brazil also have official military links: the PMSP does not belong to the Armed Forces, as it consists of a state-level organization that responds to the São Paulo state governor, but it is legally considered an auxiliary and reserve force of the Army. This does not change general policing attributes, which are still largely similar to the ones employed by police departments in the US and other Western contexts (Batitucci 2010), but reflects how the representation of war can be inscribed even in routine street-level patrolling.

Within this context, public-police interactions are often moments in which officers exert power in aggressive and intrusive ways (Oliveira 2022). Most people fear the police, and the widespread idea that law enforcement agents are "just another violent gang" has cultural currency (Jackson et al. 2022). It is not uncommon for officers to draw their guns even in relatively mundane interactions, and sometimes even point them at citizens during pedestrian and vehicle investigatory stops (Pinc 2007; Cardia et al. 2012). Use-of-force is not only a legal prerogative, but rather a constant, salient threat (Jackson et al. 2022). The social costs of this aggressive approach to policing in terms of undermined beliefs about police trustworthiness and legitimacy, however, are not clear.

Causal effect of police stops and police stops at gunpoint

The goal of this study is to estimate the direct effects of the experience of being recently stopped by police officers, including at gunpoint, on attitudinal change considering three aspects of police trustworthiness — perceived police procedural fairness, i.e., the extent to which the police are seen as trustworthy to act with procedural fairness; perceived police effectiveness, i.e., the extent to which the police are seen as trustworthy to effectively fight crime; and perceived overpolicing, i.e., the extent to which officers are expected to repeatedly intrude upon the lives of people — and public beliefs about police legitimacy, i.e., the extent to which people believe the police have the right to rule and the authority to govern. By doing so, I partly address the gap in the literature identified by Nagin and Telep (2017, 2020), who emphasized the lack of causal evidence on the relationship between police-citizen

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[5] While street-level patrolling is conducted by the PMSP, investigations and intelligence are conducted by the Polícia Civil de São Paulo (PCSP), a civilian police force often referred to as the "judicial police."

encounters and attitudes towards legal institutions, particularly in terms negative contacts (Thompson and Pickett 2021).

I estimate the effects of police stops on each of the four variables separately.[6] Given the impossibility of random assignment, such effects need to be estimated with observational data. However, in order to identify causal effects, all observed and unobserved confounders need to be conditioned upon — i.e., any variable Z, not controlled for, influencing both the probability of being stopped by the police (at gunpoint) and people's perceptions of police introduces confounding bias. It is realistically impossible to adjust for every single potential confounder Z, which is why Nagin and Telep (2017, 2020) highlighted that studies reporting correlations between perceived police contact and legal attitudes cannot claim causality — because of potential third common causes (i.e., Z). According to the authors, establishing a causal connection between the experience of police contact and attitudinal change remains an important gap in the literature.

To quote Nagin and Telep (2017, p. 130), "in some circumstances, the addition of person and time fixed effects may resolve the third common cause problem, but no study that we have reviewed included such statistical controls." The authors are referring to the two-way fixed effects (TWFE) regression estimator in the context of a difference-in-differences design, the standard way to leverage panel data for causal identification. Although the TWFE estimator is actually biased in the presence of effect heterogeneity (Goodman-Bacon 2018; Imai and Kim 2020; Callaway and Sant' Anna 2020) and would probably be of no help in this context, exploring panel data to identify the causal effect of police stops at gunpoint on perceptions of police in São Paulo is the primary motivation of this study. I address this gap in the literature employing a recently developed matching estimator in the context of a multi-period difference-in-differences design (Imai et al. 2021), as this approach, under some assumptions, removes all confounding bias and makes specific causal effects identifiable. The next subsection summarizes how the literature often attempts to remove confounding bias using longitudinal data.

Causal inference with panel data

Assuming a longitudinal data set of $i = 1, ..., N$ units and $t = 1, ..., T$ time periods (a balanced panel is assumed for the sake of notation simplicity, but the same implications apply for unbalanced panels), estimating the causal effect δ of a treatment variable D_{it} on an outcome Y_{it} essentially means removing all confounding biases that emerge from common causes of D_{it} and Y_{it}. Put in another way, all back-door paths from the treatment D_{it} to the outcome Y_{it} shown in the directed acyclic graph

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[6] Presumably, perceptions of procedural fairness, police effectiveness, and overpolicing all mediate the effects of police stops on police legitimacy. This means that, when estimating the effects on each of the three aspects of police trustworthiness, police legitimacy acts as a collider variable (Elwert and Winship 2014), implying that each of the three effects does not need to condition upon the other two variables. Similarly, to estimate direct effects of police stops on police legitimacy, one should not need to condition upon any of the three mediators (decomposing natural direct and indirect effects could be of interest, but causal mediation efforts are beyond the scope of this study).
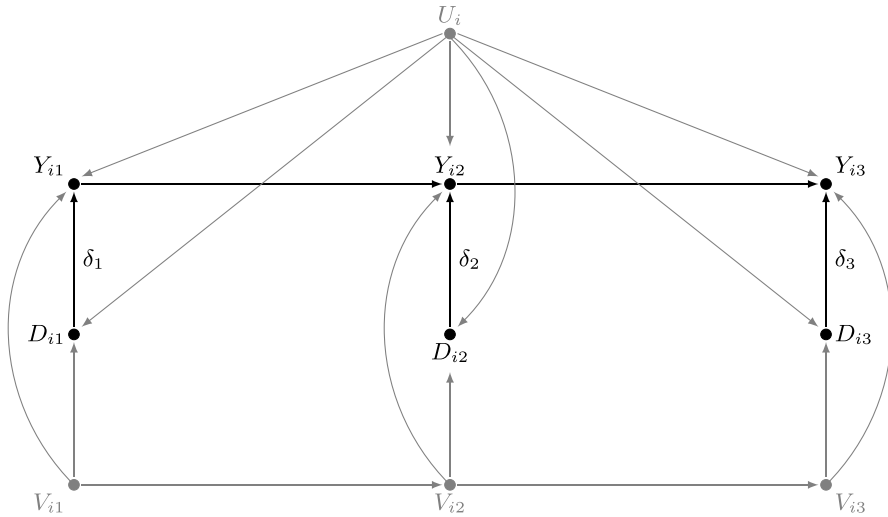
Fig. 1 Directed acyclic graph for panel data with three time periods. Note: Black nodes represent observed dependent Y_{it} and treatment D_{it} variables, gray nodes represent time-variant V_{it} and time-invariant U_i confounders. Past outcome $Y_{i,t-1}$ is assumed to affect current outcome Y_{it} as it is reasonable to assume that trust is stable over time, whereas past treatment $D_{i,t-1}$ is assumed not to affect current treatment D_{it} as it is reasonable to assume that the probability of multiple police stops over time is entirely captured by confounders U_i and V_{it}. No further dynamic relationship (e.g., past treatment $D_{i,t-1}$ affecting current outcome Y_{it}) is assumed, though this discussion will be revisited

(DAG) displayed in Fig. 1 need to be blocked — DAGs are used to graphically represent the premised structural causal model (assumed to contain all relevant variables), where arrows indicate the possible existence of causal relationships and the absence of arrows indicates the lack of causal relationships (Pearl 2009); this DAG depicts a structural model with three time points.[7] Some of those confounders might be observed variables, in which case they can simply be adjusted for using some matching or regression estimator, whereas others might be unobserved, in which case some alternative design is necessary to identify δ. Confounders can be further divided in two groups: time-constant confounders U_i, which are unit-specific and do not vary over time, and time-variant confounders V_{it}, which do.

Longitudinal data are usually very powerful at removing confounding bias that emerges from time-constant confounders U_i Imai and Kim (2019). The logic consists of using only within-unit variation (i.e., change over time). In the absence of dynamic relationships — e.g., past outcome affecting current treatment, and past treatment affecting current outcome — the family of regression models known as

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[7] This DAG assumes independence between treatments at various time points. While it is possible that being stopped and questioned by police officers at one time point increases the likelihood of being stopped again — e.g., for getting under the police "radar" or for differential law enforcement behavior (see Liberman et al. 2014) — I assume that observed and unobserved characteristics embodied by U_i and Z_{it} fully capture these scenarios and that there is no *direct* effect of the experience of a police stop itself on the probability or being stopped again.

fixed effects models[8] can safely remove any confounding bias that emerges from time-invariant confounders. That is, the back-door paths $D_{it} \leftarrow U_i \rightarrow Y_{it}$ are blocked by the inclusion of unit fixed effects.

Adjusting for time-variant confounders is a different story. Considering the potential outcomes framework (Imbens and Rubin 2015), blocking the back-door path $D_{it} \leftarrow V_{it} \rightarrow Y_{it}$ as well is equivalent to estimating the difference between the expected value of Y in the presence and in the (counterfactual) absence of treatment:[9] $\mathbb{E}\left[Y_i^1\right] - \mathbb{E}\left[Y_i^0\right]$, i.e., estimating the causal effect δ Morgan and Winship (2015). The problem, of course, is that each unit i only has one observed outcome (they either received treatment or not); the counterfactual outcome had they hypothetically (not) received treatment is missing (authorname year). In this application, counterfactual scores of perceptions of procedural fairness, police effectiveness, overpolicing, and police legitimacy among individuals who experienced a police stop (at gunpoint) had they hypothetically not experienced that police stop (i.e., $\mathbb{E}\left[Y_i^0 | D_i = 1\right]$) are not observed; and accordingly counterfactual scores among those who did not experience a police stop, had they hypothetically been stopped (i.e., $\mathbb{E}\left[Y_i^1 | D_i = 0\right]$), are not observed either.

Definitions of the causal effects of interest

My goal is to specifically estimate the average treatment effect among treated units,

$$ATT = \mathbb{E}\left[Y_{it}^1 | D_i = 1\right] - \mathbb{E}\left[Y_{it}^0 | D_i = 1\right], \tag{1}$$

which stands for the average difference between the observed levels of attitudes towards the police (perceptions of procedural fairness, police effectiveness, overpolicing, and police legitimacy) among individuals who were stopped by the police (at gunpoint) and the counterfactual attitudinal levels among those same individuals in the hypothetical scenario where they were not stopped by the police (at gunpoint). This implies that one counterfactual outcome ($\mathbb{E}\left[Y_{it}^0 | D_i = 1\right]$) is missing and needs to be estimated.

In the absence of random assignment of treatment groups and making use longitudinal data, a common approach to identify the potential outcome of treated units in the absence of treatment involves assuming that selection bias is constant over time. The difference-in-differences (DiD) design is one of the most widely used analytic strategies to estimate causal effects in social science disciplines (Cunningham 2021). Considering two time periods and that some units received treatment ($D_i = 1$) between time periods $t = 1$ and $t = 2$ while others did not ($D_i = 0$), the logic is to assume that the counterfactual change over time among treated units, had

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[8] Imai and Kim (2019) developed a non-parametric method based on matching that relaxes the linearity assumptions of unit fixed effects regression models.

[9] That is, calculating $\delta = \mathbb{E}\left[Y | do(D = 1)\right] - \mathbb{E}\left[Y | do(D = 0)\right]$ (see Pearl 2009).

they not been treated, would on average have been the same observed change over time among non-treated units — i.e.,

$$\mathbb{E}\left[Y_{it}^0|D_i = 1\right] - \mathbb{E}\left[Y_{i,t-1}^0|D_i = 1\right] = \mathbb{E}\left[Y_{it}^0|D_i = 0\right] - \mathbb{E}\left[Y_{i,t-1}^0|D_i = 0\right]. \quad (2)$$

This is known as the parallel trends assumption (Angrist and Pischke 2009). By focusing on within-unit change over time, time-invariant confounders U_i are removed; and by using the change over time among non-treated units as a *proxy* for the counterfactual change over time among treated units in the absence of treatment, time-variant confounders V_{it} are also (in expectation) removed. Under this parallel trends assumption, the *ATT* is identifiable as the difference between the observed change over time among treated and non-treated units.[10] By assuming that legal attitudes among individuals who were stopped by the police, had they not been stopped, would on average follow the same time trends as legal attitudes among individuals who were not stopped by the police in the same period, the impact of police stops (among those who did experience such interaction) can be estimated: it is the difference between their observed and their estimated counterfactual levels of attitudes towards the police.

However, the causal estimand defined in Eq. 1 only makes sense in the context of treatment implementation at a single time point (e.g., between time points $t = 1$ and $t = 2$). In multi-period settings, the causal estimand defined in Eq. 1 only makes sense when there is no effect heterogeneity over time (Goodman-Bacon 2018; Imai and Kim 2020); that is, considering the DAG in Fig. 1, the *ATT* would only make sense if the causal effect was constant over time, i.e., when $\delta = \delta_1 = \delta_2 = \delta_3$. Realistically, this is unfeasible in most applications. In the context of exposure to treatment occurring at multiple time periods, the causal estimand needs to be explicitly defined in terms of *change in treatment status over time* (de Chaisemartin and d'Haultfoeuille 2020; Imai et al. 2021; Callaway and Sant' Anna 2020). This is the case in the current study, as people can report or not the experience of an aggressive police stop at either time period.

I therefore define a slightly different causal estimand that takes change in treatment status into account. Adapting from Imai et al. (2021), I define the ATT_{change} as the average treatment effect of change in treatment status among observations that did change their treatment status and were exposed to treatment between any two periods $t - 1$ and t,

$$ATT_{change} = \mathbb{E}\left[Y_{it}^{(D_{it}=1,D_{i,t-1}=0)}|D_{it} = 1, D_{i,t-1} = 0\right] \\ - \mathbb{E}\left[Y_{it}^{(D_{it}=0,D_{i,t-1}=0)}|D_{it} = 1, D_{i,t-1} = 0\right], \quad (3)$$

where the superscript $(D_{it} = 1, D_{i,t-1} = 0)$ corresponds to the potential outcome of exposure to treatment between any two periods $t - 1$ and t, whereas the superscript

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[10] That is, $ATT = \left\{\mathbb{E}[Y_{it}^1|D_i = 1] - \mathbb{E}[Y_{i,t-1}^1|D_i = 1]\right\} - \left\{\mathbb{E}[Y_{it}^0|D_i = 0] - \mathbb{E}[Y_{i,t-1}^0|D_i = 0]\right\}.$

$(D_{it} = 0, D_{i,t-1} = 0)$ corresponds to the potential outcome of no change in treatment status.[11] Estimation procedures are discussed in detail below. It is important, however, to discuss what the ATT_{change} drawn from Imai et al. (2021) means in the context of the relationship between aggressive and confrontational policing practices (e.g., a police stop at gunpoint) and perceptions of police. This causal estimand is focused on units which were exposed to treatment between t and $t-1$, so the potential outcome that needs to be estimated is the counterfactual scores of Y_{it} among non-treated units at $t-1$ that were treated at t in the hypothetical scenario in which they remain non-treated at t. This can represent, for instance, the causal effect of a recent police stop at gunpoint on attitudinal change: for an individual who had not been stopped by the police at gunpoint at $t-1$ but then was stopped at t, the ATT_{change} refers to the difference between their observed scores of legal attitudes at t (after an actual police stop at gunpoint) and their counterfactual scores of legal attitudes at t in a hypothetical scenario in which they were not stopped by the police at gunpoint.

Causal effects of recent police stops at gunpoint on attitudinal change over time

What does the ATT_{change} mean in this context? Considering three waves of survey data, a gap of approximately 18 months between waves, and self-reported information on whether respondents experienced at least one police stop (including at gunpoint) between waves, the goal of this study is to assess the impact of the experiences of police stops and police stops at gunpoint on perceptions of police (perceived procedural fairness, police effectiveness, overpolicing, and police legitimacy). This implies direct comparisons between people's observed levels of trust and legitimacy after the experience of a police stop and their counterfactual levels of trust and legitimacy in a hypothetical scenario where they were not stopped by the police. However, people can have multiple experiences with law enforcement over time Jackson and Pósch (2019); Bradford et al. (2009). Considering three waves of data with respondents self-reporting or not a recent police stop at each time point, there are eight possible combinations of treatment statuses in the period covered by this study. Table 1 summarizes the eight groups.

This means that not only are people treated at different periods, but treatment status itself can also change over time. For instance, a respondent can report a police stop at $t = 1$, then no police stop at $t = 2$, and finally another police stop at $t = 3$ (i.e., group $F : 1 - 0 - 1$ in Table 1). This is where the ATT_{change} is crucial. It refers to the causal effect of a recent *change* in treatment status, from control to treatment, among members of the public who reported being stopped by the police (at gunpoint) between $t-1$ and t but who had not experienced a police stop between $t-2$ and $t-1$. The ATT_{change} therefore refers to the difference between observed levels of attitudes towards the police among individuals who did not experience a police stop (at gunpoint) at $t = 1$ but did experience

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[11] This causal estimand can be further refined to permit the inclusion the temporal lags and leads (see Imai et al. 2021). I return to this discussion below, but the definition of the causal effect of change in treatment status remains the same.

Table 1 Units considered in the definitions of ATT_{change}

Treatment dynamics	Role in the definition of ATT_{change}	
$(t = 1; t = 2; t = 3)$	$T1 \rightarrow T2$	$T2 \rightarrow T3$
$A : 0 - 0 - 0$ (Never treated)	Control	Control
$B : 0 - 0 - 1$	Control	Treated
$C : 0 - 1 - 0$	Treated	–
$D : 0 - 1 - 1$	Treated	–
$E : 1 - 0 - 0$	–	Control
$F : 1 - 0 - 1$	–	Treated
$G : 1 - 1 - 0$	Removed	
$H : 1 - 1 - 1$ (Always treated)	Removed	

a police stop (at gunpoint) at $t = 2$ (or, likewise, no experience at $t = 2$ but a reported experience at $t = 3$) and the counterfactual levels of attitudes towards the police among those same individuals in a hypothetical scenario where they do not experience a police stop at $t = 2$ (or, likewise, at $t = 3$).

Using the DiD logic, the counterfactual change in perceptions of police among individuals who changed their treatment status can be estimated based on the observed change scores among respondents who remain in the control group in the same period (Imai et al. 2021); e.g., for individuals who were not stopped by the police at $t = 1$ but then were stopped by the police at $t = 2$, individuals who were not stopped by the police at $t = 1$ *nor at* $t = 2$ constitute the control group. This means that, for any two periods $t - 1$ and t (e.g., from $t = 1$ to $t = 2$ or from $t = 2$ to $t = 3$), only people who report no police stop at $t - 1$ (and then either report a police stop or not at t) are used in this analysis. Respondents who reported being stopped by the police at the first two time points are removed as their perceptions of police are not part of the definition of the ATT_{change}.

The ATT_{change} therefore implies the effect of a recent change in treatment status from control to treatment. While it is possible to define long-term effects depending on the inclusion of temporal leads (see Imai et al. 2021), this definition is particularly useful to immediate, short-term effects — especially in the context of a three-wave longitudinal survey covering a relatively short amount of time. This study therefore focuses on the estimation of ATT_{change} to understand the impact of aggressive and confrontational policing practices on perceptions of police. Using a multi-period difference-in-differences approach and a recently developed estimator (Imai et al. 2021), I examine the causal effects of a recent police stop and a recent police stop at gunpoint on changes in people's levels of perceived procedural fairness, police effectiveness, overpolicing, and police legitimacy.

This study

Does the experience of being stopped by the police have an impact on perceptions of police trustworthiness, such as the expectation that police officers are trustworthy to act with procedural fairness, to effectively fight crime, and to refrain from intruding upon the lives of people? Crucially, does the experience of being stopped by the police have an impact on public beliefs about the legitimacy of legal institutions? According to Nagin and Telep (2017, 2020), the literature is yet to show evidence of a causal relationship between the experience police-citizen encounters and attitudinal change — especially in terms of negative contacts (Thompson and Pickett 2021). Making use of longitudinal survey data from the city of São Paulo, Brazil, and relying on a novel causal estimand, the ATT_{change}, in this study I focus on the impact of a recent police stop on perceptions of procedural fairness, police effectiveness, overpolicing, and police legitimacy. First, I test hypotheses that suggest a negative impact of the experience of simply being stopped and questioned by law enforcement agents.

- **Hypothesis 1.1:** A recent experience of being stopped by the police negatively affects perceptions of procedural fairness;
- **Hypothesis 1.2:** A recent experience of being stopped by the police negatively affects perceptions of police effectiveness;
- **Hypothesis 1.3:** A recent experience of being stopped by the police positively affects perceptions of overpolicing; and
- **Hypothesis 1.4:** A recent experience of being stopped by the police negatively affects public beliefs about police legitimacy.

Second, I test whether the experience of *aggressive* police stops leads to attitudinal change. According to PJT, it is the style of public-police interactions that should affect legal attitudes, not just the experience of a police stop. Specifically, police stops in which people perceive officers communicating procedural fairness are expected to boost police trustworthiness and legitimacy (Bradford 2017), while involuntary police-initiated interactions that communicate suspicion of ongoing or future criminal conduct are expected to undermine attitudes towards legal authority (Tyler et al. 2015). In this study, I do not assess the degree to which *evaluations* of police contact impact changes in police trustworthiness and legitimacy (see Jackson and Pósch 2019); instead, I focus on the effect of objectively aggressive police stops. To operationalize this hypothesis, I consider the experience of a recent police stop *at gunpoint*, an undeniably aggressive interaction. While I do not measure how respondents evaluated officers' conduct, officers pointing a gun at suspects during investigatory stops is in and of itself an intrusive practice in which law enforcement agents are explicitly threatening citizens with use-of-force, thus communicating at least some level of suspicion. So, the second set of hypotheses are based on the potential impacts of aggressive police stops at gunpoint.

– **Hypothesis 2.1:** A recent experience of being stopped by the police *at gunpoint* negatively affects perceptions of procedural fairness;
– **Hypothesis 2.2:** A recent experience of being stopped by the police *at gunpoint* negatively affects perceptions of police effectiveness;
– **Hypothesis 2.3:** A recent experience of being stopped by the police *at gunpoint* positively affects perceptions of overpolicing; and
– **Hypothesis 2.4:** A recent experience of being stopped by the police *at gunpoint* negatively affects public beliefs about police legitimacy.

To test these hypotheses, I rely on a recently developed analytic strategy that combines matching methods for panel data and the difference-in-differences estimator in the context of a multi-period difference-in-differences design (Imai et al. 2021). The goal of this study is to estimate the average treatment effect among units that were recently exposed to the treatment (the ATT_{change}) — i.e., among individuals with no prior recent experience of being stopped by the police (at gunpoint, the difference between attitudes towards the police among individuals who then experienced a police stop (at gunpoint) and the counterfactual attitudes towards the police that those same individuals would have in case they had not experienced a recent police stop (at gunpoint). By focusing on the effects of recent *change in treatment status*, this analytic strategy builds on the recent econometric discussions about the limitations of the two-way fixed effects regression estimator in the presence of effect heterogeneity (Goodman-Bacon 2018; Callaway and Sant' Anna 2020; Imai and Kim 2020) and permits the identification of the causal estimand of interest. I discuss the plausibility of the assumptions and the implications of this estimation strategy below, after introducing the data and measures used in this study.

Data

To conduct the analysis I rely on longitudinal survey data from Brazil. I draw upon a three-wave population-based survey representative of adults who reside in eight selected neighborhoods in the city of São Paulo, one of the largest cities in the Global South. The original study was designed by the Center for the Study of Violence of the University of São Paulo (NEV-USP).

Sampling procedures built on Nery et al. 's 2019 cluster analysis of nearly 20,000 census tracts in the city. Using information on urban, criminal, demographic, and structural conditions, the authors suggested eight urban patterns within the city of São Paulo, and argued that nonprobability survey designs sometimes misrepresent some of those clusters. Nery et al. also selected contiguous areas with approximately 30–60 census tracts each (all belonging to the same cluster) that were highly representative of each urban pattern, and referred to each of those as key areas. The population in each area ranges from 20,000 to 40,000 people. The longitudinal survey data used in this study was designed to represent adults living in each of the eight key areas.

In each area, a two-stage cluster sampling design was employed: first, census tracts within each key area were randomly selected; second, respondents in the

selected tracts were chosen following demographic quotas (gender, age, and education). The first wave of the survey was fielded in mid-2015 and had 1200 respondents (150 respondents in each area). A new wave was fielded in early 2017, when 928 of those respondents agreed to take part in the study again; and once more in mid-2018, with 801 respondents ($N = 2929$).[12] The attrition rate at each point in time was lower than 25%, the threshold originally expected by the researchers. All interviews were conducted face-to-face, in Portuguese, at the respondents' place of residence using Tablet-Assisted Personal Interviewing (TAPI). The final sample is broadly representative of the population of the eight selected areas: 53% of the respondents are female, 56% are white, and the average age is 40.2 years. Several recent studies of police-citizen relations in Brazil have made use of this data set (see, e.g., Jackson et al. 2022; Oliveira et al. 2019; Oliveira 2022).

Measures

The four dependent variables I use in this study correspond to perceptions of (1) procedural fairness, (2) police effectiveness, (3) overpolicing and (4) police legitimacy, each of which was measured based on several survey items adapted from previous research. To measure perceived procedural fairness, survey items based on Sunshine and Tyler (2003) and Jackson et al. (2012) were used: respondents were asked about the extent to which the police in their neighborhood usually "explain clearly why they stop and question someone," "make impartial and just decisions," "pay attention to the information people provide them with," and "treat people with respect." All four indicators were measured using a five-point Likert scale ranging from "never" (1) to "always" (5). Descriptive statistics of all survey items can be found in Table 2. All survey questions were originally measured in Portuguese and translated by me.

To measure perceptions of police effectiveness, respondents reacted to survey items tapping into ideas of police performance. They were asked how good a job they thought the police in their neighborhood were doing in relation to "reducing drug trafficking," "reducing armed robbery," "responding to emergency calls," "police station services," "criminal investigations," and "demonstrations and protests." All six indicators were measured using a five-point Likert scale ranging from "very bad" (1) to "very good" (5). To measure perceptions of overpolicing, respondents were asked about the extent to which they believed that police officers in their neighborhood usually "act as if they were above the law" and "follow and harass

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[12] I assume dropouts to be missing at random. A binomial logistic regression model predicting dropouts at the second wave indicates no association between police legitimacy at $T1$, perceptions of procedural fairness at $T1$, perceptions of overpolicing at $T1$, gender, race, social class, or age and the probability of dropping out at $T2$. The only significant predictor was perceived effectiveness at $T1$: an increase of one standard deviation in the scores of this variable was associated with a decrease of 4.6% in the probability of dropping out (marginal effects at the mean). If anything, respondents at $T2$ have slightly more negative views of the police. In terms of the probability of dropping out at $T3$, the only significant predictor was age, where every year was associated with a small decrease of 0.3% in the probability of dropping out.

Table 2 Descriptive statistics of survey items

Latent construct	Survey question	"Mean" at $t = 1$	"Mean" at $t = 2$	"Mean" at $t = 3$
Perceived procedural fairness	Police explain clearly why they stop and question or arrest people*	2.91	2.83	2.89
	Police make impartial and fair decisions*	3.15	3.12	3.17
	Police pay attention to the information people provide them with*	3.32	3.32	3.30
	Police treat people with respect*	3.67	3.69	3.70
Perceived police effectiveness	How good a good a police doing in relation to:			
	· Reducing drug trafficking†	3.47	3.43	3.74
	· Reducing armed robbery†	3.78	3.69	3.89
	· Responding to emergency calls†	4.10	4.00	4.01
	· Police station services†	3.97	3.76	3.91
	· Criminal investigations†	3.91	3.71	3.74
	· Demonstrations and protests†	4.09	3.79	3.91
Perceived overpolicing	Police act as if they were above the law*	3.44	3.42	3.33
	Police follow and harass people*	2.84	2.87	2.74
Police legitimacy	The police act in accordance with what you believe is right and wrong*	3.17	3.08	3.14
	People are afraid of the police*	3.87	3.44	3.33
	Do you think you should obey the police when you believe they are wrong? (1 = yes)	0.69	0.74	0.73
	Why do you think you (*should/should not*) obey the police even when you believe they are wrong?			
	- (Yes) Normatively grounded duty to obey	0.31	0.33	0.34
	- (Yes) Coercive obligation to obey	0.40	0.41	0.41
	- (No) Disobedient protest	0.08	0.09	0.07
	- (No) Rejection of authority	0.21	0.17	0.19
–	Were you stopped by the police (*over the last two years/since our last interview*) in the state of São Paulo? (1 =yes)	0.38	0.24	0.24
–	During that stop, did the police officers point a gun at you? (1 = yes)	0.17	0.12	0.12

* Never (1) → Always (5); † Very bad (1) → Very good (5)

people" — both survey items were measured using a five-point Likert scale ranging from "never" (1) to "always" (5).

I derive scores for these three dependent variables (perceived procedural fairness, perceived police effectiveness, and perceived overpolicing) using pooled graded response (IRT) models for polytomous data with logistic function links. Models were estimated using R's `ltm` package Rizopoulos (2006). Pooled IRT models ensure measurement equivalence and thus permit modeling change over time in latent variables. Trait scores of perceived procedural fairness, police effectiveness, and overpolicing are z-standardized and range from approximately −2 to approximately +2, with mean approximately 0.

To measure police legitimacy, I follow the modeling strategy developed by Jackson et al. (2022) and replicate Oliveira 's 2022 measurement model using the same data set as this study. I combine survey items tapping into normative alignment, obligation to obey the police, and fear of the police, and measure beliefs about the legitimacy of the police as a coercive-consensual continuum. Reflecting the nature of public-authority relations in São Paulo, this scale ranges from coercive and fear-based reasons to obey the police to consensual reasons and a normatively grounded sense of duty. To measure normative alignment with the police, respondents were asked whether they thought the police in their neighborhood acted "in accordance with what they believed was right or wrong"; to measure fear of the police, respondents were asked whether they thought "people were afraid of the police." Both survey items were measured based on a 5-point Likert scale ranging from "never" (1) to "always" (5).

Crucially, this measurement strategy involved the classification of open-ended survey responses about *why* people thought they should, or should not, obey the police even when they think the police are wrong. On previous work, Jackson et al. (2022) and Oliveira et al. (2020) analyzed data from a different survey and, based on a thematic analysis, proposed four categories of obligation to obey: some people think they should obey the police because of a *normatively grounded duty to obey* or based on a *coercive obligation*, whereas some people think they should not obey the police as a form of *rejection of authority* or as a type of *disobedient protest*. The same survey question was fielded in the longitudinal survey used in this study, and Oliveira (2022) classified those open-ended responses based on a supervised machine learning text classification model using a support vector machine algorithm Hastie et al. (2009). The measurement model was based on a longitudinal IRT model estimated on `MPlus` with ordinal and multinomial logistic links. I use trait scores derived from this IRT model reflecting police legitimacy judgements — lower values indicate lack of legitimacy beliefs and a coercive reasoning to comply with legal directives, whereas high values indicate that one believes that the police have a legitimate claim of power and, as such, can expect voluntary aquiescent behavior. Scores are also z-standardized and range from approximately −2 to +2, with mean approximately 0.

The treatment variables in this study correspond to the self-reported experience of police stops and police stops at gunpoint. Respondents were first asked whether they "were stopped by the police (*over the last two years/since our last interview*) in the state of São Paulo"; response alternatives were "yes" or "no." Those who

answered "yes" were then further asked whether during that stop "police officers pointed a gun" at them (again, "yes" or "no").[13] 38%, 24%, and 24% of respondents at each wave reported being recently stopped by the police, whereas 17%, 12%, and 12% reported being stopped by the police at gunpoint (i.e., 44%, 50%, and 50% of all self-reported police stops). I assess the impact of both police stops and police stops at gunpoint on perceptions of procedural fairness, police effectiveness, overpolicing, and police legitimacy.

Around half of respondents' most recent self-reported police stop involved a police officer pointing a gun at them, which is an extremely high proportion. The distribution of police stops at gunpoint is not homogeneous across the city and is mainly concentrated in disadvantaged and racially diverse neighborhoods, but even in wealthy white neighborhoods around a quarter of respondents' most recent police stop involved an officer pointing their firearm (see Oliveira 2022, foradiscussionontheunequaldistributionofpolicebehaviorinSãoPauloneighborhoods). This high proportion is consistent with previous estimates on the frequency of police stops at gunpoint in the city (see, e.g., Pinc 2007; Cardia et al. 2012). Crucially, it is remarkably similar to another important survey estimate. In 2015, the same year as the first wave of this longitudinal survey, researchers at the Center for the Study of Violence of the University of São Paulo carried out another survey — using the exact same survey instrument — representative of the entire population of adults residing in São Paulo.[14] 1806 respondents took part in that study (see Oliveira et al. 2020; Jackson et al. 2022 for other studies using that survey data). 568 respondents (31.5%) reported having been stopped by the police at least once in the previous two years; 282 of whom (49.7%) reported that a police officer had pointed a gun at them during their last stop.

Evaluations of police stops at gunpoint

Both treatment variables considered in this study refer to self-reported experiences, not to perceptions of such experiences, even though — according to PJT — it is the evaluation of power appropriateness during resident-police interactions that would lead to changes in beliefs about police trustworthiness and legitimacy, not simply the experience of being stopped by law enforcement agents. Yet, assessing the causal effect of evaluations of police-citizen encounters is not trivial (Jackson and Pósch 2019; Nagin and Telep 2017, 2020). The goal of this study is to assess

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[13] First wave respondents were first specifically asked whether they had recently experienced traffic or pedestrian stops. 25% reported being stopped by the police at least once in the previous two years when driving a car, 11% reported when riding a motorcycle, 18% when walking on the streets, and 3% in some other circumstance. To construct the indicator of whether the respondent had recently been stopped by the police, I consider all respondents who answered "yes" to any of these four questions. The follow-up question on whether the officer pointed a gun at them did not specify the type of police stop — those respondents who reported any recent police stop of any kind were simply asked "considering the last time you were stopped by the police in the state of São Paulo, did the officers point a gun at you?"

[14] The reason why they carried out another survey was to explore potential differences between a survey representative of eight selected neighborhoods and the overall population residing in the city.

Table 3 Average scores of perceptions of police stops among respondents who were recently stopped by the police

	Recent police stop at gunpoint?		
	Yes	No	t test
Recently stopped when driving a car (1 =yes)	0.60	0.67	−2.11
Recently stopped when riding a motorcycle (1 =yes)	0.36	0.17	6.53
Recently stopped when walking on the streets (1 =yes)	0.65	0.28	11.78
Recently stopped in some other circumstance (1 =yes)	0.07	0.06	0.59
Treated as if you were a criminal (1 =yes)[*]	0.60	0.14	11.00
Satisfied with treatment[†]	1.89	2.74	−13.67
Satisfied with the decision-making process[†]	2.09	2.85	−12.26

[*] Waves 2 and 3 only; [†] Very dissatisfied (1) → Very satisfied (4)

the impact of police stops at gunpoint on perceptions of police in order to better understand the social costs of aggressive policing practices in terms of undermined attitudes towards legal institutions. Rather than measuring perceived aggression during encounters, I rely on police stops at gunpoint as those are undeniably aggressive interactions — I do not claim this to be a comprehensive account of the effects of aggressive policing on trust and legitimacy, but rather an investigation on one particular dimension of aggressive policing.

But how did those interactions go? As a limitation intrinsic to survey research on resident-police encounters, very little information about the police stops is available. For instance, the perspective of the police officer is not taken into account (e.g., why did they decide to draw their guns in each specific case). Crucially, this study does not consider whether respondents who self-reported being stopped by the police at gunpoint actually perceived those encounters as an aggressive exercise of power — assessing the impact on legal attitudes of police stops at gunpoint is important on its own, as this study shows, but the extent to which such interactions are perceived as aggressive would deserve another study.

The longitudinal survey with São Paulo residents does not permit a full characterization of the police stops at gunpoint. For instance, while the survey asked respondents whether they had different experiences of police stops (e.g., pedestrian or traffic stops), it does not disentangle pedestrian police stops at gunpoint from traffic police stops at gunpoint. Similarly, the survey did not ask the exact same questions about respondents' most recent police stop over the three waves apart from whether officers had pointed a gun at them. Yet, some preliminary examination is still possible. For instance, respondents at waves 2 and 3 were asked whether they felt like police officers treated them as if they were criminals (measured as a binary indicator with "yes" or "no" options). Respondents across all three waves were also asked about the degree to which they believed officers communicated procedural fairness during the police stop: they were asked about the extent to which they were satisfied with (a) how the officers treated them and (b) the decision-making process during the interaction, both indicators measured using a 4-point Likert scale ranging from

Table 4 Descriptive statistics: treatment dynamics

Treatment dynamics ($T1 - T2 - T3$)	Police stops	Police stops at gunpoint
$A : 0 - 0 - 0$ (Never treated)	651 (54%)	938 (78%)
$B : 0 - 0 - 1$	35 (3%)	23 (2%)
$C : 0 - 1 - 0$	39 (3%)	28 (2%)
$D : 0 - 1 - 1$	17 (1%)	8 (0%)
$E : 1 - 0 - 0$	246 (21%)	108 (9%)
$F : 1 - 0 - 1$	45 (4%)	18 (2%)
$G : 1 - 1 - 0$	75 (6%)	32 (3%)
$H : 1 - 1 - 1$ (Always treated)	92 (8%)	45 (4%)

"very dissatisfied" (1) to "very satisfied" (4). Table 3 displays the mean of each of these indicators among respondents who self-reported being recently stopped by the police — broken down whether said stop involved an officer pointing a gun at them or not.

On average, among respondents who self-reported being stopped by the police at gunpoint over the last two years (for wave 1 responses) or between two waves (for waves 2 and 3 respondents), 60% reported being recently stopped by the police at least once when driving a car, whereas among those who were recently stopped by the police but not at gunpoint, 67% were stopped when driving a car; 36% when riding a motorcycle, as opposed to 17% not at gunpoint; and 65% when walking on the streets, as opposed to 28% not at gunpoint. This cautiously suggests that most police stops at gunpoint were either pedestrian stops or stops against motorcycle riders.

Considering respondents at waves 2 and 3 only, 55% of those who reported a recent stop at gunpoint felt like officers were treating them as if they were criminals, whereas only 15% of respondents who reported a recent police stop but not at gunpoint felt so, which could potentially indicate some level of perceived aggression when officers draw their weapons. Such respondents also had slightly different judgements about the procedural fairness communicated by officers during the interaction, as they had lower average scores on satisfaction with the treatment and the decision-making process.

Treatment dynamics

38% of the respondents reported being stopped by the police up to two years prior to the first wave, then 24% reported being stopped at some point between the first and the second waves, and 24% reported being stopped at some point between the second and the third waves. Accordingly, 17% of the respondents reported a police stop at gunpoint up to two years prior to the first wave, 12% reported so between the first and the second waves, and finally again 12% reported a police stop at gunpoint between the second and the third waves. Given that each of these variables includes two treatment groups (1 = experiencing a police stop; 0 = not experiencing a police stop) that can be implemented across three time periods, treatment dynamics consist of eight possible groups.

Table 4 shows the proportion of respondents in each group. Just over half (54%) never reported a police stop (group $A : 0 - 0 - 0$); over three quarters (78%) when it comes to experiencing a police stop at gunpoint. A small proportion of respondents (8% and 4%, respectively) were always treated (group $H : 1 - 1 - 1$), whereas all others reported some change in their treatment status during this period.

Modeling these treatment dynamics is not trivial as it depends on theoretical assumptions about treatment assignment and implementation over time. For instance, the ATT_{change} assumes a dynamic treatment adoption; i.e., units can switch their treatment status over time. This means that respondents who experienced a police stop before the first wave and between the first and the second waves (i.e., groups G and H) are not considered in the estimation of the ATT_{change}. Other analytic strategies differ in how to handle treatment dynamics. For instance, while the classic TWFE estimator does not impose assumptions about treatment dynamics (and end up including treated observations as control units; see Goodman-Bacon 2018; Imai and Kim 2020), Callaway and Sant' Anna 's 2020 group-time estimator assumes staggered treatment adoptions, implying that units can never become untreated after being exposed to treatment — which is obviously not accurate in terms of police stops (at gunpoint).

When it comes to police stops, citizens can indeed be stopped by the police or not at multiple periods — including at gunpoint. When examining causal effects of a recent police stop (at gunpoint), the focus is then on change in treatment status, from control to treatment: members of the public who did not experience a police stop at $t - 1$ and then reported being stopped by a law enforcement agent (at gunpoint) at t.

Impacts of police stops and police stops at gunpoint on perceptions of procedural fairness, police effectiveness, overpolicing, and police legitimacy

Estimation strategy

Under parallel trends, the difference between the average change scores of treated and control units (i.e., the difference-in-differences) constitutes the ATT. When treatment is implemented at multiple periods, one strategy consists precisely of comparing all pairwise comparisons between any two periods $t - 1$ and t. One possibility in this direction involves fitting a linear regression model with unit and time fixed effects simultaneously; this is known as the two-way fixed effects (TWFE) regression estimator of the multi-period DiD Cunningham (2021):

$$Y_{it} = \alpha_i + \gamma_t + \delta \cdot D_{it} + \varepsilon_{it}, \tag{4}$$

where α_i is a fixed intercept for unit i, γ_t is a fixed intercept for time period t, D_{it} is the treatment variable, δ is the estimated coefficient, and ε_{it} is a disturbance term for unit i at time period t. Under the same assumption of parallel trends, the TWFE estimator is believed to estimate the average treatment effect among treated units (i.e., $\delta_{TWFE} \sim ATT$). Outside of criminology, a survey conducted by de Chaisemartin and

d'Haultfoeuille ([2020](#)) found that almost a fifth of all empirical studies published by the *American Economic Review* (AER) between 2010 and 2012 relied on linear regression models with unit and period fixed effects to estimate causal effects.

However, recent studies have shown some issues with the TWFE regression estimator. For instance, Imai and Kim ([2020](#)) demonstrated that, unlike the ability of unit fixed effects to remove time-invariant confounders (assuming no dynamic causal relationships), the inclusion of unit and period fixed effects simultaneously does not configure a non-parametric, design-based strategy to estimate causal effects. TWFE models can only identify causal effects under the standard linear regression modeling assumptions — i.e., under the assumption that the data generating process corresponds to Eq. [4](#). Even more crucially, recent econometric studies have shown that one further assumption of TWFE models is no treatment effect heterogeneity; when treatment effect is not constant over time, bias is introduced. Essentially, the comparisons between treated and non-treated units are not clearly defined, and the final TWFE estimate is a weighted average of all possible two-period DiD designs inscribed within the multi-period setup where some estimates have negative weights (Goodman-Bacon [2018](#); Imai and Kim [2020](#)). Given that the combination of parallel trends, linearity, and effect homogeneity constitute very strong assumptions, the ability of TWFE models to estimate causal effects is often unrealistic in most applications.

Instead, in order to estimate the effects of a recent police stop, and a recent police stop at gunpoint, on changes in perceptions of procedural fairness, police effectiveness, overpolicing, and police legitimacy, I rely on Imai et al. 's ([2021](#)) matching methods for panel data that augments the difference-in-difference estimator and estimates the ATT_{change}. The authors developed a non-parametric approach that allows for comparisons between treated and non-treated observations with the same treatment history up to a pre-specified number of temporal lags. Under a conditional parallel trends assumption (similar to the one described in Eq. [2](#), but conditioning on treatment history, lagged outcome, and time-varying covariates), this matching framework identifies the ATT_{change} while relaxing the linearity and effect homogeneity assumptions of the TWFE regression estimator. Additionally, while they assume no spillover effects, their analytic strategy allows for some carryover effects up to the same number of pre-specified temporal leads.

This estimation strategy has three steps. First, treated observations are matched with non-treated observations that have the identical treatment history up to a pre-specified number of lags L. Second, this matched set is further refined by adjusting for other time-varying covariates (e.g., lagged dependent variable, age, prior offending behavior, and fear of the police); this step is conducted using the Mahalanobis distance measure. Third, the difference-in-differences is calculated in order to account for an underlying time trend and estimate the ATT_{change}.

Apart from specifying L, this method allows researchers to specify a number temporal leads F, which represents the outcome at F time periods after the treatment; e.g., $F = 0$ corresponds to contemporaneous effects, $F = 2$ corresponds to long-term treatment effects two time periods after the treatment, and so forth. The first step in any causally oriented study should be an explicit definition of the *causal quantity of interest*. In this case, this involves defining the temporal lags (L) — the causal

quantity of interest reflects the effect of change in treatment status after how many time periods (e.g., change from $t-1$ to t, or from $t-2$ to t, and so forth)? — and leads (F) — the causal quantity of interest reflects the treatment effect how many time periods after exposure to treatment (e.g., contemporaneous, short-term, or long-term effects)? According to Imai et al. (2021), the choice of F and L should be substantively oriented. In this application, I am interested in estimating the effects of a recent police stop on attitudinal change considering three time periods. I therefore adopt $L = 1$ so that only one previous treatment history is taken into account and both changes from $t = 1$ to $t = 2$ and from $t = 2$ to $t = 3$ are used in the analysis. Accordingly, I specify $F = 0$ as the focus of this study is on contemporaneous effects after a recent police stop (at gunpoint).

I estimate eight models, testing each of the eight outlined hypotheses. In each case, after matching observations with the same treatment history (at $t - 1$), the matched set is further refined by adjusting for lagged dependent variable (i.e., perceptions of procedural fairness, police effectiveness, overpolicing, and police legitimacy at $t - 1$), age (as older individuals are less likely to be stopped by the police and more likely to have more favorable views of legal authority; Bradford , 2017), prior offending behavior (presumably, people who engage in criminal conduct are more likely to being stopped by the police, particularly at gunpoint[15]), and fear of the police; matching is conducted using the Mahalanobis distance measure. Because the third step in the estimation procedures applies the difference-in-differences estimator, all time-constant confounders are automatically removed as only within-unit change over time is taken into account — so other important potential confounders such as race, social class and neighborhood of residence, among others, are automatically conditioned upon. Models were estimated using R's `PanelMatch` package (Imai et al. 2021).

Results

Four models assessing the impact of police stops (hypotheses 1.1–1.4) and another set of four models estimating the impact of police stops at gunpoint (hypotheses 2.1–2.4) on changes in perceptions of procedural fairness, police effectiveness, overpolicing, and police legitimacy were estimated. For each of the eight models, the first step involved matching observations based on their treatment history — i.e., each respondent-wave observation was matched to another respondent-wave observation that equally did not report a police stop (or a police stop at gunpoint) at a previous time period (only one previous time period as I specified $L = 1$). Given that few respondents reported being stopped by the police (around a third of the sample) and, obviously, even fewer reported being stopped at gunpoint (half of those, or around a sixth of the sample), treated observations were permitted five closest control units.

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[15] Offending behavior was measured as composite index based on five scenarios involving low-level inappropriate public behavior. Respondents were asked whether they had previously tried to bribe a traffic warden, buy counterfeit goods, use cable TV signal without paying for it, and buy products without a receipt to pay less. All five binary indicators were added to create a summative index ranging from 0 (no offensive conduct) to 5 (performed all five illegalities).

The second step consisted of further refining the matched sets by matching on time-varying covariates — this is crucial because this estimator is premised on a conditional parallel trends assumption, so achieving covariate balance in key potential confounders is important. The most important variable included in the refinement of the matched sets is the lagged dependent variable. That means that, on top of identical treatment history, observations are matched to observations with similar perceptions of police prior to the experience of being stopped and questioned (at gunpoint) by a law enforcement agent. Other time-varying variables included in this step are age, fear of the police, and prior (lagged) offending behavior.

Covariate balance was achieved. Standardized differences between treated and control observations at both t and $t − 1$ across all four variables (the relevant lagged dependent variable, age, prior offending behavior, and fear of the police) in all eight matched sets were always statistically indistinguishable from zero and never surpassed 0.3 standard deviations. This implies that each matched pair consisted of observations with no recent police stop (up to one time period), with similar prior levels of perceptions of police (fairness, effectiveness, overpolicing, or legitimacy, depending on the model), similar age, similar self-reported prior offending behavior, and similar levels of fear of the police; the crucial difference is that one observation of each matched pair would then be exposed to treatment — the experience of a police stop or the experience of a police stop at gunpoint.

Finally, the third step involved calculating the difference-in-differences. Because only within-person change over time was taken into account, time-constant confounders are not a threat. The causal quantity of interest in this study is the ATT_{change} specified in Eq. 3, so $F = 0$ leads were pre-specified in order to focus on contemporaneous, short-term effects of police stops and police stops at gunpoint on four aspects of perceptions of police. To handle statistical uncertainty around the point estimates, standard errors were obtained with 1000 bootstrap iterations. 2.5% and 97.5% quantiles of the bootstrapped estimates are reported.

Figure 2 displays the results of four models estimating the impact of a recent police stop on perceptions of procedural fairness (hypothesis 1.1), police effectiveness (hypothesis 1.2), overpolicing (hypothesis 1.3), and police legitimacy (hypothesis 1.4). All four point estimates are close to zero: the effects on procedural fairness and police legitimacy are just negative, and on police effectiveness and overpolicing, just positive. However, all four point estimates are largely uncertain, with bootstrapped estimates covering both negative and positive values, which implies little evidence that police stops affect perceptions of police. While it could be possible that the experience of being stopped and questioned by law enforcement agents leads to changes in attitudes towards the police, this study's imprecise estimates do not permit any conclusions.[16]

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[16] I employ the expression "imprecise" here because non-significant estimates do not necessarily imply lack of an effect; it could simply be the case that the data are insensitive to identifying an effect (i.e., little evidence for the null or alternative hypothesis). Rather than suggesting that police stops do not have an effect on perceptions of police, I conclude that this study's estimates consist of little evidence of such effects. For a lively discussion about how to interpret statistical significance, see Benjamin et al. (2018); McShane et al. (2019).
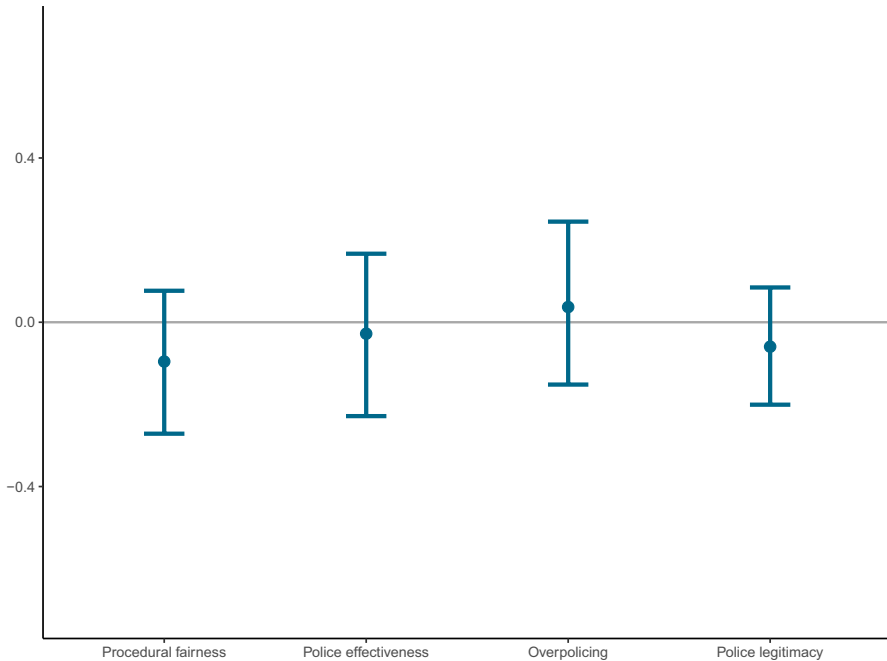
Fig. 2 Effects of a recent police stop on perceptions of police. Note: Four separate models estimated. Point estimates are ATT_{change} (contemporaneous effects). Standard errors obtained with 1000 bootstrap iterations. 2.5% and 97.5% quantiles of the bootstrapped estimates reported. Mahalanobis distance was used to create the matched sets (based on one lagged dependent variable, age, prior offending behavior, and fear of the police). Treated observations were permitted five closest control units. Models were estimated using R's `PanelMatch` package Imai et al. (2021)

Figure 3 displays the results of another set of four models estimating the impact of police stops *at gunpoint* on perceptions of procedural fairness (hypothesis 2.1), police effectiveness (hypothesis 2.2), overpolicing (hypothesis 2.3), and police legitimacy (hypothesis 2.4). Results are now substantially different. The experience of being stopped and questioned at gunpoint leads to negative changes in perceived procedural fairness — a reduction of .30 standard deviations in the trait scores, with 95% of the bootstrapped estimates yielding negative values — and beliefs about the legitimacy of the police — a reduction of .16 standard deviations, also with 95% of the bootstrapped estimates yielding negative values. Police stops at gunpoint also seem to impact perceptions overpolicing: an increase of .21 standard deviations in trait scores reflecting the degree to which people expect officers to repeatedly intrude upon their lives, with 95% of the bootstrapped estimates yielding positive values. This study, therefore, finds empirical support for hypotheses 2.1, 2.3, and 2.4: a recent police stop at gunpoint seems to have a contemporaneous effect on perceptions of procedural fairness, overpolicing, and police legitimacy. The only exception is hypothesis 2.2: while results indicate an expected decrease in the scores of perceived police effectiveness, bootstrapped estimates yield both positive and
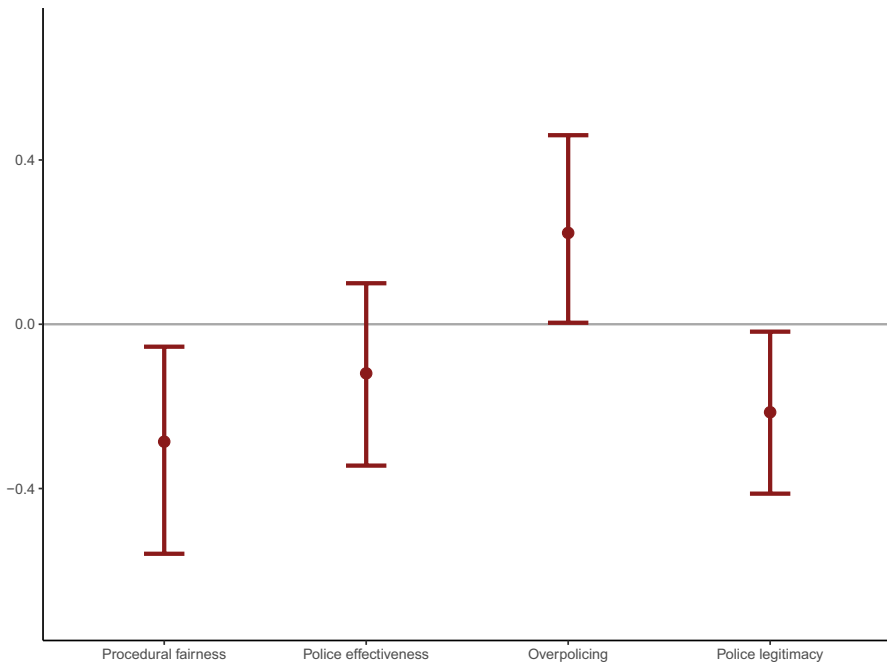
Fig. 3 Effects of a recent police stop at gunpoint on perceptions of police. Note: Four separate models estimated. Point estimates are ATT_{change} (contemporaneous effects). Standard errors obtained with 1000 bootstrap iterations. 2.5% and 97.5% quantiles of the bootstrapped estimates reported. Mahalanobis distance was used to create the matched sets (based on one lagged dependent variable, age, prior offending behavior, and fear of the police). Treated observations were permitted five closest control units. Models were estimated using R's `PanelMatch` package Imai et al. (2021)

negative values, suggesting little evidence of a non-zero effect. While it is possible that the experience of an aggressive interaction with a police officer pointing a gun leads to changes in perceptions of police effectiveness, this study does not have empirical evidence to make such claims.

Discussion

Countries like the USA and Brazil have increasingly adopted aggressive policing methods that rely on stopping members of the public, checking their identities and searching their possessions whenever police officers suspect of some ongoing or future criminal conduct (Fagan et al. 2016; Lima et al. 2016). Sometimes, law enforcement agents make use of stop-and-frisk powers even when they have no reason for suspicion (Rios 2011; Epp et al. 2014). This type of confrontational policing strategy can be associated with improvements in public welfare through some crime reduction; however, according to Manski and Nagin (2017), whether such improvements outweigh the social costs involved in increased coercive police powers is not

clear. I contribute to this literature assessing unintended consequences of aggressive policing practices in terms of undermined beliefs about police trustworthiness and legitimacy in the high-crime, low-trust city of São Paulo, one of the largest metropolitan areas in the Global South.

Legitimacy is important. Legal institutions need public support to function properly. When people believe in the legitimacy of legal authority, they tend to engage in self-regulation, voluntarily complying with legal directives and cooperating with legal authorities (Sunshine and Tyler 2003; Papachristos et al. 2012); on the other hand, when people start questioning the legitimacy of the law and the police, they tend to develop more favorable attitudes towards the acceptability of the use of violence (Jackson et al. 2013; Oliveira 2022) and engage in criminal conduct (Kirk and Papachristos 2011). Policing methods that harm legitimacy beliefs can therefore be counterproductive if their goals involve ensuring public safety — considering the undermine of public legitimacy beliefs, it is possible that the potential benefits in crime reduction of aggressive policing practices do not outweigh their social costs.

To evaluate the impact of aggressive policing practices on perceptions of police, in this study I analyzed the effects of a recent police stop and, fundamentally, of a recent police stop during which the officer pointed a gun at the citizen on perceptions of procedural fairness, police effectiveness, overpolicing, and police legitimacy. The scene of the study was the city of São Paulo, Brazil, a context in which police stops often involve direct threats of violence (Jackson et al. 2022): police officers constantly carry firearms, and almost half of all police stops self-reported by respondents in this study occurred at gunpoint. People expect the police to use violence in most situations, which means that even the experience of being stopped and questioned by law enforcement agents could undermine police trustworthiness and legitimacy — that was the premise of hypotheses 1.1–1.4. Crucially, however, this study centered around the potential causal effects of police stops *at gunpoint* undeniably aggressive practices — on perceptions of police (hypotheses 2.1–2.4).

Nagin and Telep (2017, 2020) have recently highlighted that the procedural justice literature is yet to demonstrate evidence of a causal relationship between police-citizen interactions and attitudes towards the law and the legal institutions. Although previous work has demonstrated the impact of procedural justice training on legal attitudes (Mazerolle et al. 2013; Sahin et al. 2017), the causal effect of negative contacts on trust and legitimacy has not been demonstrated (Thompson and Pickett 2021). This is tricky because police mistreatment cannot be randomly assigned, so the effects need to be estimated using observational data. I have partly addressed some of these gaps in this study. Using three waves of longitudinal survey data representative of adults residing in eight neighborhoods in São Paulo between 2015 and 2018, I discussed how we can leverage panel data to engage in causal inference. I discussed the causal effects of the experiences of being stopped by the police and being stopped by the police at gunpoint on perceptions of procedural fairness, police effectiveness, overpolicing, and police legitimacy. I used Imai et al. 's (2021) recently developed matching methods for time-series cross-sectional data in the context of a multi-period difference-in-differences design. Under a number of modeling and theoretical assumptions, this method exploits longitudinal data to overcome both methodological issues

identified by Nagin and Telep (2017, 2020): the threats of reverse causality and third common causes.

One of Imai et al. 's 2021 key contributions was an explicit definition of causal estimands in terms of *change in treatment status*. Applied to the effects of police stops and police stops at gunpoint, the causal quantity of interest in this study, the ATT_{change}, was the effect of a recent police stop or police stop at gunpoint (e.g., over the previous 1.5 year) among people with no previous experiences of being stopped by the police or being stopped by the police at gunpoint (e.g., over the previous 3 years). Observations were first matched according to identical treatment history, then the matched sets were further refined based on lagged dependent variable (e.g., prior levels of perceived procedural fairness, police effectiveness, overpolicing, and police legitimacy, respectively for each model), age, prior offending behavior, and fear of the police using Mahalanobis distance, before applying the difference-in-differences estimator of the ATT_{change}.

First, I assessed the impact of a recent police stop on attitudinal change, and results of four separate models displayed in Fig. 2 all yielded non-significant results. This means that the difference in change scores of perceptions of police among people who were and people who were not recently stopped by the police cannot be distinguished from zero, implying that hypotheses 1.1–1.4 lack empirical evidence. This study does not imply that police stops do not affect attitudinal change; it just implies that the estimates are too imprecise to make such conclusions. Yet, results make substantive sense. We know very little about how each of those police stops actually developed; it is possible, for instance, that some respondents — despite being involuntarily stopped by a police officer — were satisfied with the officers' exert of power, whereas others were dissatisfied. Given that this first treatment variable only takes into account *whether* people had previous experiences of police stops, and neither *how* the interaction occurred nor *how many* police stops experiences they had in the period, it is not surprising that I found no statistical effects on attitudinal change. The experience of being stopped and questioned by police officers, therefore, possibly does not contribute to changes in public perceptions of procedural fairness, police effectiveness, overpolicing, and police legitimacy.

Second, I assessed the impact of a recent police stop in which an officer pointed their gun at the citizen on attitudinal change. Results of four different models are displayed in Fig. 3. As tacitly aggressive interactions, police stops at gunpoint consist of a decent *proxy* for the type of aggressive policing practices commonly adopted by law enforcement agents in São Paulo. Respondents who were recently stopped and questioned by officers pointing their firearms had increased expectations that the police repeatedly intrude upon people's lives and decreased expectations that the police usually exert their power with procedural fairness. Imperatively, the recent experience of a police stop at gunpoint also decreased people's beliefs about the legitimacy of the police. Therefore, hypotheses 2.1, 2.3, and 2.4 are empirically supported by this study. The one exception was perceived police effectiveness: while it is possible that police stops at gunpoint also affect change in people's expectations that officers perform their duties effectively, estimates from this study are too imprecise to make any conclusions.

So, why do recent police stops at gunpoint lead to attitudinal change in terms of perceived procedural fairness, overpolicing, and police legitimacy, but potentially not in terms of perceived police effectiveness? According to PJT, people evaluate the normative appropriateness of power during encounters with police officers, and update their expectations about how legal authorities usually behave; to the extent that people expect power to be exercised in normatively appropriate ways — in ways that make them feel like they belong to the superordinate group represented by legal authority — they enhance their beliefs about the legitimacy of the law and the legal institutions (Tyler et al. 2014; Tyler and Jackson 2014). A large body of research suggests that people evaluate the procedural fairness communicated by law enforcement agents during police-citizen encounters, which signals group status and value (Lind and Tyler 1988; Tyler and Huo 2002). More recently, research has also suggested that people evaluate how intrusive officers are when they exert power, which communicates autonomy, marginalization and oppression (Tyler et al. 2015; Trinkner et al. 2018; Oliveira 2022). While this study did not focus on exploring legitimating norms — i.e., I did not assess which task-specific assessments of police conduct transmit the effects of police stops at gunpoint onto police legitimacy — it is possible to speculate that people do not update their expectations of police effectiveness after police stops at gunpoint because effective performance does not signal relational messages that make people feel included or excluded from society. Even though high performance is key to crime control, perceptions of police effectiveness are based on instrumental rather than normative reasoning (Sunshine and Tyler 2003).

It is important to stress what the effects suggested by this study mean. Under a conditional parallel trends assumption (conditioned on time-varying covariates such as lagged dependent variable, prior offending behavior, age, and fear of the police), this study estimates several ATT_{change}'s — the effect of a recent change in treatment status. In other words, I am comparing the change scores of perceptions of police between respondents who had no experience of police stop at gunpoint in the previous three years (i.e., untreated observations at $t-1$) but were stopped by the police at gunpoint at some point in the past 18 months (treated at t) with the change scores of perceptions of police that those respondents would have, had they not been stopped by the police at gunpoint in the past 18 months (i.e., had they remained untreated at t). The focus of this study is on contemporaneous effects — "immediate" changes in legal attitudes affected by a recent police stop at gunpoint. This does not imply that the effects outlined here are only short-term, dying out after some time; while this remains a theoretical possibility, considering three waves of longitudinal survey data and the methods applied in this study, no conclusions about potential patterns of decay in the impacts of police stops at gunpoint can be made. While my focus here is on recent experiences of aggressive behavior from law enforcement agents, this study does not cover other equally interesting causal quantities — such as the cumulative effects of multiple police stops at gunpoint and long-term effects.

Yet, the focus of this study is on the social costs of aggressive policing practices. Manski and Nagin (2017) suggested that effective policing must balance the conflicting objectives of public safety and community trust. While confrontational proactive policing methods could improve public welfare through crime reduction, social costs usually

involve increased intrusion on the privacy of members of the public. I contribute to this literature offering another important social cost of aggressive policing: the undermine of public beliefs in the legitimacy of the law and the legal institutions. In a city where street-level policing is conducted by militarized officers who frequently draw their weapons and threaten to use violence against citizens, I showed that people who were recently stopped and questioned by officers at gunpoint have decreased perceptions of police: lower expectations of procedural fairness, higher expectations of overpolicing, and undermined beliefs of police legitimacy.

Limitations

Limitations should, of course, be acknowledged. First, in this study I engaged with modern causal inference methods; yet, making causal claims with observational data is not an easy task. The most common strategy to leverage longitudinal data for causal purposes is the difference-in-differences design, which relies upon a strong parallel trends assumption. I drew on the analytic strategy developed by Imai et al. (2021), who relaxed this assumption by developing matching methods for panel data; the estimator now relies on a conditional parallel trends assumption, as matched sets are created based on treatment history, lagged outcome, and other time-varying covariates. This is an important improvement, as parallel trend is now a credible assumption. However, it remains an untestable assumption. The threat posed by potential time-varying covariates is assumed to be low, but remains a possibility.

Second, I focused on a very specific causal estimand — the effect of a recent change in treatment status — but other causal quantities are of great interest too. This study makes no claims regarding cumulative effects, long-term effects and different patterns of decay. Longer survey designs and analytic strategies that allow for the identification of cumulative and long-term effects would be welcome (Schomaker et al. 2019). Legal socialization is a process that occurs during the life course (Fagan and Tyler 2005), and it is reasonable to expect that single interactions with police officers would do little to substantially alter pre-existing levels of attitudes towards the police which are likely historically produced by neighborhood structural conditions and the cumulative legacy of police mistreatment in some communities (see Kirk and Papachristos 2011; Bell 2017; Nagin and Telep 2020). Identifying the cumulative impact of multiple experiences with law enforcement, especially during childhood and adolescence, would permit a better characterization of the social costs of aggressive policing approaches.

Third, I did not address one of Nagin and Telep 's 2017; 2020 core critics of procedural justice studies: the impact of *perceptions* of police-citizen encounters on attitudinal change (Jackson and Pósch 2019). While police stops at gunpoint constitute an undeniably aggressive use of power, it is possible that people had different perceptions about the power appropriateness (e.g., procedural justice) enacted by legal agents during those interactions. Future research should attempt to examine the causal effect of evaluations and perceptions of public-police contact on attitudes towards legal authority. Relatedly, while I adopted a relatively objective measure — the experience of a recent police stop at gunpoint (as opposed to perceptions

or evaluations of recent interactions) — this study still relied on self-reported data. We do not know how those police stops actually developed, or even if officers indeed threatened citizens with their guns. While self-reporting that officers pointed their weapons at the citizen during a stop is in/of itself an important measure, it is possible that results are also partly influenced by measurement errors.

Fourth, little information is known about each specific police stop and why officers decided to draw their guns. Not every section of the Brazilian society is at equal risk of such intimidatory policing, and it is possible that this is a policing tactic that appeals to, and commands legitimacy among, other audiences (see Jackson et al. 2022; Bottoms and Tankebe 2012). Viewed this way, a possible avenue for future research might be an exploration of how the police view and legitimize this tactic to themselves (see Tankebe 2019; Bradford and Quinton 2014).

Finally, results presented in this study are only true in the context of eight neighborhoods in the city of São Paulo, Brazil, between 2015 and 2018. This is a specific scenario in the Global South in which fear of both crime and police violence is high (Jackson et al. 2022). The extent to which results can be extended to other contexts is not clear. For instance, the fact that almost half of all self-reported police stops involved officers pointing a gun at the citizen does not relate to most Western contexts. Yet, if police stops at gunpoint can damage public attitudes towards legal authority even in a social setting where this threatening experience is not so uncommon, one could expect an even larger impact of other experiences of aggressive policing when people do not expect to be treated with aggression. More research about the relationship between aggressive policing and attitudinal change in other contexts is necessary, especially in other cities in the Global South and some high-crime, low-trust contexts in the USA.

Conclusion

I started this paper with the idea that effective policing in modern democracies needs to balance the sometimes conflicting objectives of crime control and community trust (Manski and Nagin 2017). While confrontational policing methods such as stop-and-frisk practices might offer some social benefits via crime reduction, they usually do so with heavy social costs (Sharkey 2018; Legewie and Fagan 2019; Geller et al. 2014). I contribute to the literature assessing the impact of aggressive policing practices on one important outcome: public judgements about police trustworthiness and legitimacy (Sunshine and Tyler 2003). While previous research has suggested strong correlations between police-citizen encounters and attitudes towards legal institutions (Jackson 2018), there is little evidence of causal relationships (Nagin and Telep 2017, 2020) — and the few studies that did claim causal effects mostly focused on positive rather than negative interactions with police officers (Thompson and Pickett 2021). Crucially, most previous works on the relationship between police conduct and perceptions of police are focused on Global North scenarios, especially the USA. Using data from a three-wave longitudinal survey of

adult residents in the city of São Paulo, Brazil, I highlight the importance of addressing the social costs of aggressive policing practices in the Global South.

To make causal claims using panel data, I engaged with recent debates on the possibilities and limitations of the multi-period difference-in-differences design (Goodman-Bacon 2018; Callaway and Sant' Anna 2020; Imai and Kim 2020). I explicitly defined a causal estimand that takes *change in treatment status* into account — the ATT_{change}, based on recent work by Imai et al. (2021). Using Imai et al. 's 2021 recently developed analytic strategy that augments matching methods for time-series cross-sectional data with the difference-in-differences estimator and considering a conditional parallel trends assumption, I estimated the effects of a recent police stop and a recent police stop at gunpoint on four aspects of perceptions of police: perceptions of procedural fairness, police effectiveness, overpolicing, and police legitimacy.

I found little evidence that the experience of being recently stopped and questioned by police officers in São Paulo leads to attitudinal change. While that could be the case, this study's estimates are too imprecise to make any conclusions. However, this study shows that a recent experience of being stopped by the police *at gunpoint* — an undeniably aggressive and threatening exert of power — leads to decreases in perceptions of procedural fairness, increases in perceptions of overpolicing, and, crucially, decreases in beliefs about the legitimacy of the police. In a city in the Global South where authority relations are based on fear and the threat of violence (Jackson et al. 2022), where militarized police officers are trained for war-like situations (Zanetic et al. 2016), and where policing policy is heavily based on aggressive and coercive methods (Oliveira 2022), this study contributes to a growing list of empirical evidence on the social costs of aggressive policing. I show that the persistent reliance on aggressive policing practices and confrontational methods that treat most citizens as potential criminals has important negative consequences in terms of undermined legitimacy beliefs, compromising public recognition of the law and the legal institutions as the rightful authority in society.

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