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Mano dura: An experimental evaluation of military policing in Cali, Colombia

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Abstract
Governments across the Global South rely on their militaries for domestic policing operations. We experimentally evaluate the social and political consequences of a military policing intervention in Cali, Colombia, one of the world's most violent cities. The intervention, *Plan Fortaleza*, involved recurring, intensive military patrols randomized at the city block level. Our evaluation combines administrative crime and human rights data, surveys of more than 10,000 residents, a conjoint experiment, a "costly" behavioral measure, and qualitative interviews with 49 civil society leaders. Despite null or adverse effects on crime and human rights, we show that *Plan Fortaleza* improved citizen' attitudes toward the military and increased their demand for military involvement in domestic law enforcement. It also strengthened citizens' support for extrajudicial punishment and—alarmingly—for military coups in response to rising crime, potentially signaling a diminished commitment to democracy and the rule of law among the program's intended beneficiaries.

Scholars have long argued that a strict separation between the military and the police is necessary for transitions to democracy and democratic consolidation (Giddens, 1987; Huntington, 1957; Kraska, 2007). Yet across the Global South, democratically elected governments routinely deploy their militaries for domestic law enforcement, especially where crime is rampant and police forces are corrupt, inept, or resource-constrained (De Bruin, 2022). Nowhere is this strategy more common than in Latin America, the world's most violent region (Pion-Berlin & Carreras, 2017). Militaries now play a prominent role in domestic policing operations in Bolivia, Brazil, Colombia, Ecuador, El Salvador, Guatemala, Honduras, Mexico, Nicaragua, Paraguay, and Peru (Flores-Macías & Zarkin, 2023). Nor is the strategy unique to Latin America. Military policing is similarly common in South Africa, Indonesia, and the Philippines, among other countries.

Public support for military policing is widespread, especially in Latin America (Pion-Berlin & Carreras, 2017). Yet research on the consequences of this "*mano dura*" (iron fist) approach to domestic law enforcement remains surprisingly scarce. Most studies of military policing focus on former Mexican President Felipe Calderón's deployment of soldiers to combat drug cartels and its impact on homicides (Espinosa & Rubin, 2015; Flores-Macías & Zarkin, 2021), violence between rival drug trafficking organizations (Osorio, 2015), state capacity (Flores-Macías, 2018), and human rights abuses (Flores-Macías & Zarkin, 2023). Flores-Macías and Zarkin (2021) explore the consequences of military policing in Latin America more generally, with an emphasis on violence, human rights, and police reform.

These studies have taught us much about the repercussions of military policing in Latin America and Mexico in particular. But to our knowledge, no

study has tested the effects of military policing on the attitudes and behaviors of citizens most directly affected by it.[1] We theorize that military policing may reshape citizens' relationships with the armed forces, the police, the criminal justice system, and the government more broadly. Drawing on multiple literatures, we argue that military policing is likely to improve citizens' perceptions of the military and strengthen their demand for further military involvement in domestic law enforcement while diminishing their perceptions of the police. Exposure to military policing is also likely to increase citizens' support for the right-wing parties and politicians with whom these interventions are typically associated. Finally, military policing is likely to reinforce the troubling but increasingly pervasive belief that deterring criminals requires flouting due process protections and circumventing democratic norms and procedures (Zechmeister, 2014).

We test these predictions using a preregistered[2] randomized impact evaluation of the *Plan Fortaleza* military policing program in Cali, Colombia, the third largest city in the country. Despite improvements in recent years, Cali reported 46.7 homicides per 100,000 residents in 2018 (the year before our study began)—nearly double the homicide rate of Medellín (Colombia's second largest city) and more than triple that of Bogotá (the capital). In response, the government of Cali deployed recurring, intensive military patrols to two of the most violent *comunas* (communes) in the city. Given the seemingly intractable challenges of crime prevention in Cali and the city's long, bleak history of police corruption and abuse, municipal authorities believed military policing might reduce crime without undermining human rights protections. Military policing programs like *Plan Fortaleza* are a defining characteristic of *mano dura* policies in Latin America and beyond (Holland, 2013). We experimentally evaluated the intervention in collaboration with the Mayor's Office, the Third Brigade of the Colombian Armed Forces, and Innovations for Poverty Action (IPA) Colombia. (We address the ethics of our study below.)

In a companion study, Blair and Weintraub (2023) show that *Plan Fortaleza* had at best null and worst

adverse effects on crime and human rights.[3] We build on this study by exploring the social and political consequences of *Plan Fortaleza* at the micro level using surveys of more than 10,000 respondents; a forced-choice, image-based conjoint experiment; and a "costly" behavioral measure of demand for military policing based on texts and calls to a hotline we created for citizens to request additional military presence in their neighborhoods. We exploit municipal elections held in the middle of the intervention and massive nationwide protests that began immediately afterward to test *Plan Fortaleza*'s impact on political attitudes and behavior. We probe and contextualize our quantitative results using qualitative interviews with 49 civil society leaders in Cali and a desk review of the policy platforms of 224 mayoral candidates across the country.

Despite *Plan Fortaleza*'s null or adverse effects on crime and human rights, we show that the program nonetheless improved citizen' perceptions of the military both in absolute terms and (especially) relative to the police and strengthened their demand for military involvement in domestic law enforcement. Contrary to the literature on "penal populism" (Holland, 2013; Pratt, 2007), however, we find no evidence that the intervention boosted turnout for right-wing parties or candidates in Cali's municipal elections or that it diminished support for (or participation in) protests associated with left-wing unions, indigenous groups, and student organizations. Finally, we show that *Plan Fortaleza* strengthened citizens' support for extrajudicial punishment and— more alarmingly—for military coups in response to rising crime, potentially indicating a weakened commitment to democracy and the rule of law among the program's intended beneficiaries. We explore potential explanations for our findings in the discussion.

Our study contributes to research on hot spots policing (Blattman et al., 2021; Collazos et al., 2019; Braga & Bond, 2008; Weisburd & Telep, 2014), penal populism (Holland, 2013; Pratt, 2007), and the militarization of law enforcement in Latin America and beyond (Flores-Macías & Zarkin, 2021). Most studies of militarization focus on the impact of SWAT team deployments or military equipment transfers to US police departments (Bove & Gavrilova, 2017; Delehanty et al., 2017; Harris et al., 2017; Gunderson et al., 2021; Lowande, 2021; Mummolo, 2018). Other studies address militarization of the police globally (De Bruin, 2022) or in Latin America specifically (González, 2020; Magaloni & Rodriguez, 2020; Magaloni et al., 2020).

In contrast, relatively few scholars have tested the effects of military policing itself—that is, deployments

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[1] A partial exception is Flores-Macías and Zarkin (2022), who find that citizens' support for military policing is positively associated with confrontations between the Mexican military and drug trafficking organizations. While informative, this is a correlation based on highly aggregated municipal-level trends. Flores-Macías and Zarkin's (2022) study is nonetheless path breaking, and we draw on it in our theory and research design.

[2] Our pre-analysis plan (PAP) is available at https://osf.io/95cz3. We posted the PAP to the Evidence in Governance and Politics (EGAP) registry on October 9, 2019, nine days after the start of the intervention and eight days before the launch of our monitoring survey. We filed an amendment on December 13, 2019, roughly one month after the end of the intervention and roughly one month before the launch of our endline survey. The amendment specified several minor modifications to our empirical strategy and added a hypothesis about support for and participation in the massive nationwide protests that coincided with the end of the intervention.

[3] We reproduce some of these results on Supplementary Information (SI) pp. 27–30 for completeness.

of soldiers to conduct domestic policing operations. This gap is important not just because military policing is both very common (Flores-Macías & Zarkin, 2021) and very popular (Pion-Berlin & Carreras, 2017) but also because militaries differ from even the most heavily militarized police departments in the type and intensity of training they receive, in their more inflexible vertical command structures, and in the expectation that they will use physical and often lethal force to eliminate enemies on the battlefield. Moreover, all existing efforts to evaluate military policing rely on observational data (Espinosa & Rubin, 2015; Flores-Macías, 2018; Flores-Macías & Zarkin, 2021, 2022, 2023; Osorio, 2015). Because soldiers are typically only deployed when crime is rampant and police forces are overwhelmed, observational studies must overcome daunting obstacles to causal inference. Much of the debate over military policing remains impressionistic. To our knowledge, ours is the first experimental evaluation of the social and political consequences of this increasingly pervasive but still poorly understood approach to domestic law enforcement.

THEORETICAL FRAMEWORK

Military policing interventions may have profound effects on the attitudes and behaviors of citizens. We experimentally evaluate the impact of military policing on citizens' (1) perceptions of the police and military and demand for more aggressive military involvement in domestic law enforcement, (2) turnout for right-wing parties and politicians, and (3) commitment to democracy and the rule of law more generally. Our theoretical framework draws on insights from multiple literatures, including research on foreign aid, penal populism, and civil–military relations. The theory generates seven empirical predictions for us to test.

Attitudes toward the police and military

Military policing may increase citizens' support for the military and more aggressive military intervention in domestic law enforcement while decreasing their support for the police. Military policing is very popular with the public, especially in Latin America. According to the 2012 AmericasBarometer survey, 78% of respondents across 19 Latin American countries agreed or strongly agreed that soldiers should "participate in combating crime and violence" (Pion-Berlin & Carreras, 2017, p. 10). Of the 25 countries surveyed in the 2014 AmericasBarometer, respondents in all but one (Chile) expressed greater trust in the military than in the police, typically by double-digit margins. Results

from the 15 countries in the 2018 AmericasBarometer are consistent with this pattern. Indeed, besides the Catholic Church, no other institution is as widely trusted as the military in Latin America (Zechmeister, 2014, p. 114).

When citizens express support for military involvement in domestic law enforcement, they make a "comparative judgment call about the relative efficacy of military versus police conduct in domestic security roles" (Pion-Berlin & Carreras, 2017, p. 5). Militaries typically have better communication, transportation, and logistical capacity than police forces, and soldiers usually receive more (and more rigorous) training than police officers. Citizens are aware of these distinctions, which are arguably especially salient in Latin America, where most police forces are poorly funded, trained, and equipped (Flores-Macías, 2018; Flores-Macías & Zarkin, 2023; Kraska, 2007), and where many have been overpowered by "daunting" organized criminal syndicates (Zechmeister, 2014, p. 104).

Perhaps more surprisingly, citizens trust soldiers to deter criminals not just more effectively than police officers but also more "humanely," with greater respect for human rights and without placing "innocent civilians in harm's way to the same degree" (Pion-Berlin & Carreras, 2017, pp. 5, 18, 20). The standards governing police use of force are often weaker than the military's, while the consequences for transgressing use-of-force policies are typically less severe for police officers than for soldiers (Tecott & Plana, 2016; Wood, 2015). While impunity is common in Latin America, and while many of the region's militaries have reputations for abuse, the same or worse is often true of the police. Many Latin American police forces have been found to be complicit with organized crime (Flom, 2022), and assassinations, kidnappings, and torture have become "familiar police practices" throughout the region (Pion-Berlin & Carreras, 2017, p. 7).

The disparity in public perceptions of the military and police may partly reflect a gap in visibility: citizens are less likely to observe abuses perpetrated by soldiers on the battlefield than they are to witness misconduct committed by police officers in their own neighborhoods. But surveys have shown that citizens tend to express more trust in the military even in countries where soldiers have been involved in domestic policing operations for years. Reported rates of corruption and human rights violations are also higher for the police than for the military even in settings where soldiers have long been deployed for domestic law enforcement (Pion-Berlin & Carreras, 2017).

The proposition that military policing may legitimize the military at the expense of the police also resonates with concerns about "crowding out" in the literatures on foreign aid and international

intervention. By providing security and other services, third parties (e.g., UN peacekeepers or foreign donors) may "crowd out" and thus delegitimize government officials in the eyes of citizens (Blair & Winters, 2020; Lake, 2010). We theorize that crowding out of one government institution by another may have similarly delegitimizing effects, especially when the two institutions perform similar functions, one more capably than the other. Whether crowding out *actually* delegitimizes government institutions remains a matter of debate; some studies find that it does not (Blair, 2019; Blair & Roessler, 2021; Cruz & Schneider, 2017; Dietrich & Winters, 2015; Dietrich, Mahmud, & Winters, 2018). Nonetheless, if soldiers are perceived to be more effective, less corrupt, and less abusive than police officers, then military policing may bolster already favorable attitudes toward the military while eroding already unfavorable attitudes toward the police. This may generate demand for further military involvement in domestic policing operations:

Hypothesis 1. Military policing will improve citizens' perceptions of the military.

Hypothesis 2. Military policing will strengthen citizens' demand for military involvement in domestic law enforcement.[4]

Hypothesis 3. Military policing will diminish citizens' perceptions of the police.

Support for conservative candidates

Just as military policing may increase citizens' support for the military, so too may it increase their support for the right-wing parties and politicians with whom military policing is usually associated. The literature on "penal populism" characterizes military policing and other *mano dura* interventions as politically expedient strategies for winning votes, especially where crime is rampant (Pratt, 2007). Importantly, military policing need not *actually* improve security to benefit proponents at the ballot box. Instead, penal populists "allow the electoral advantage of a policy to take precedence over its penal effectiveness," given that the goal is to "win votes rather than to reduce crime or to promote justice" (Roberts et al., 2003, p. 5).

In Latin America, as in most parts of the world, penal populism has been associated more with right-wing parties and politicians than with the left (Holland, 2013). Conservatives have been more likely than their liberal opponents to promote *mano dura* policies (Cohen & Smith, 2016; Visconti, 2020)—although, as

we discuss below, this may not be the case in Cali. Penal populists reject the real or perceived "benign liberalism" of the criminal justice system in favor of a more iron-first approach to deterrence (Pratt, 2007, pp. 12, 20). Right-wing movements born of military dictatorships are especially likely to advocate for military policing and other *mano dura* interventions, using the "language, figures, and founding myths" of an authoritarian past to demonstrate a commitment to security "at all costs" (Holland, 2013, p. 52). Citizens exposed to military policing may become more receptive to these messages and to the right-wing parties and politicians that promote them:

Hypothesis 4. Military policing will increase support for right-wing parties and politicians.[5]

Hypothesis 5. Military policing will decrease support for left-wing parties and politicians.

Commitment to democracy and the rule of law

Finally, just as military policing may strengthen citizens' demand for military involvement in domestic law enforcement, so too may it increase their support for *mano dura* approaches to governance more generally, potentially inculcating values that are incompatible with democracy and the rule of law. Critics often worry about the repercussions of military policing for civil liberties and the consolidation of democracy at the macro level (Loveman, 1999; Stepan, 1986), and blurred boundaries between the military and the police are often interpreted as indicators of "repressiveness and lack of democracy" (Kraska, 2007, p. 501). Historically, when Latin American militaries were tasked with neutralizing domestic threats, they often did so by targeting journalists, activists, labor organizers, and political opposition leaders (Isacson, Olson, & Haugaard, 2004, p. 1). Skeptics have long warned that military involvement in domestic policing operations cannot be "consistent with both the maintenance of an authentic combat capability and democratic values" (Dunlap, 1999, p. 222).

We theorize that the micro-level consequences of military policing for citizens' commitment to democracy and the rule of law may be equally profound. Reliance on the military for domestic policing operations replaces the "traditional police orientation of 'protect and serve'" with a "military orientation of 'overwhelm and defeat'" (Campbell & Campbell, 2010, pp. 329–330). If citizens believe that preventing crime is a priority—as most Latin Americans do

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[4] In our PAP, we combined Hypotheses 1 and 2 into a single hypothesis. We separate them here since we test them in different ways.

[5] On SI p. 37, we test whether military policing increases support for right-wing causes more generally.

(Pion-Berlin & Carreras, 2017, p. 7)—and if they believe that soldiers are more effective than police officers at deterring criminals, then they may view this "military orientation" as necessary, even desirable. Indeed, penal populism developed partly in response to the public perception that democratic institutions and due process protections favor criminals over victims.

Moreover, citizens are often willing to trade off civil liberties for crime prevention, even in settings that are much less violent than Cali (Mondak & Hurwitz, 2012). *Mano dura* proponents embrace this trade-off, seeking to "curtail or abandon altogether many longstanding criminal justice rights" in order to re-prioritize "law-abiding community members" (Pratt, 2007, p. 29). Support for *mano dura* tends to be highest when its targets are perceived to be threats to citizens' safety (Morrison, 2024). Citizens who are exposed to military policing may become increasingly sympathetic toward *mano dura* policies, rejecting due process restrictions and embracing the use of extrajudicial punishment to deter crime. At the extreme, they may endorse more dramatic military interventions, including, potentially, a reestablishment of the same forms of military rule for which penal populists are often nostalgic (Holland, 2013, p. 52):

Hypothesis 6. Military policing will weaken citizens' commitment to democracy, in particular by increasing their support for military rule.[6]

Hypothesis 7. Military policing will weaken citizens' commitment to the rule of law, in particular by increasing their support for extrajudicial punishment.[7]

SETTING AND INTERVENTION

We test these hypotheses in Cali, perhaps best known as the home of the infamous Cali Cartel, which controlled much of the trafficking and transshipment of cocaine from Colombia in the 1980s and 1990s. Organized crime is weaker and more fragmented today, more focused on "micro-territories,"

and more dedicated to money laundering, extortion, and micro-trafficking of drugs for domestic consumption. Violence is driven primarily by disputes between gangs. Despite improvements in recent decades, crime remains pervasive, and Cali ranks among the most violent cities in the world.[8]

To address these problems, Cali's Security and Justice Secretariat deployed the military to patrol crime hot spots as part of an initiative known as *Plan Fortaleza*. While specific to Cali, *Plan Fortaleza* is part of a trend toward increasing military involvement in domestic law enforcement throughout Colombia. In 2016, the government signed a peace agreement with the *Fuerzas Armadas Revolucionarias de Colombia* (FARC), the country's largest rebel group. With FARC's demobilization, the military began to shift from counterinsurgency in the countryside to domestic policing operations in cities, including Bogotá, Medellín, Barranquilla, and, most prominently, Cali. The city is therefore an important test case for military policing in Colombia and, potentially, in Latin America more broadly. We discuss generalizability in further detail in the conclusion.

Plan Fortaleza involved intensive, repeated vehicular and foot patrols by soldiers from the Military Police and Special Forces; the latter are, on average, older, better equipped, and more experienced than the former. The intervention focused on two of Cali's 22 *comunas*—numbers 18 and 20—which have a combined population of approximately 215,000 residents. Both *comunas* are hot spots for crime. Seven to eight teams consisting of six to eight soldiers each patrolled more or less simultaneously between 5:00 p.m. and midnight on weekdays, checking IDs and business licenses, detaining or searching suspected criminals ("*requisas*"), mounting road blocks, and conversing with residents. We provide descriptive statistics on the patrols on SI p. 7.

We randomly assigned patrols at the *manzana* (city block) level. The military originally planned to patrol each treatment block for roughly 30 minutes every six days. Since most blocks are small relative to the size of the patrolling units, this target was ultimately revised down, and patrols lasted 11 minutes per block on average. The police were also present in both *comunas*. Importantly, as we show on SI p. 40, we observe no difference in police presence between treatment and control blocks, suggesting the police did not refocus their attention from the treatment to the control group (or vice versa) as a result of the intervention. Our evaluation began on September 30, 2019, and concluded on November 18, 2019, when the military redeployed to other sites in response to a wave of nationwide political protests.

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[6] This hypothesis was not prespecified, but it follows naturally from Hypotheses 1 and 2. Democracy is, of course, a complex concept. We focus on the aspect of citizens' commitment to democracy that we believe is most likely to be affected by military policing. Military rule is anathema to any plausible definition of democracy, even minimalist ones (e.g., Przeworski et al., 2000; Schumpeter, 1947).

[7] This hypothesis deviates somewhat from our PAP, where we hypothesized that military policing would increase support for retributive justice. We broaden this hypothesis to encompass commitment to the rule of law more generally, as we think this outcome is of greater political and normative concern. (On SI p. 37, we find no evidence that *Plan Fortaleza* increased support for retributive justice specifically.) We focus on the aspect of citizens' commitment to the rule of law that we believe is most likely to be affected by military policing. Extrajudicial punishment is anathema to any plausible definition of the rule of law, even minimalist or "procedural" ones (e.g., Raz, 1979; Waldron, 2008).

[8] According to data from the NGO *Seguridad, Justicia, y Paz*, Cali had the 21st highest homicide rate in the world at the time of our study.

While the intervention we evaluate was relatively short, it was also arguably quite strong, with large teams of soldiers patrolling small city blocks carrying assault rifles and wearing fatigues, helmets, and flak jackets. Patrols were scheduled for times when civilians tended to be at home or in the streets, making them more likely to observe the soldiers' presence. While Colombian cities have pursued increasingly militarized approaches to policing in recent years, the sight of soldiers patrolling residential neighborhoods remains rare. Moreover, the length of the program was not atypical of military policing interventions in Latin America, where soldiers usually participate in limited operations targeting hot spots for crime and drug trafficking (Flores-Macías & Zarkin, 2021, p. 526).

RESEARCH DESIGN

Randomization

The two *comunas* in our sample comprise 1,254 blocks nested within 30 *barrios* (neighborhoods). We stratified by neighborhood, then randomly assigned blocks to the treatment group with probability 1/6. We gain no additional statistical power for estimating treatment effects, and only marginal statistical power for estimating spillover effects when we increase the proportion of treated blocks. We provide power calculations on SI p. 3.

We recognized at the outset that some spillover was likely in these densely populated neighborhoods, and so assigned to a spillover group any control block that was adjacent to at least one treatment block. We thus make the simplifying assumption that the effects of the program may spill over from treatment to adjacent control blocks, but no further. We address the possibility of more complicated spillover dynamics, and of "fuzzy clustering" (Blattman et al., 2021), on SI p. 14. In total, 214 blocks were assigned to the treatment group, 765 to spillover, and 275 to control. Figure 1 maps the blocks in our sample by treatment assignment.

Civilian monitors accompanied the patrols to help locate treatment blocks, track treatment compliance, and document the soldiers' activities using smartphones equipped with customized software developed specifically for this project. As we discuss on SI p. 10, treatment compliance was generally very high. For safety reasons, the monitors wore bulletproof vests and were required to stay in their vehicles at all times. We did not receive any reports of threats against monitors at any point during the evaluation. We discuss our safety and ethics protocols in further detail below and on SI p. 1. We address the possibility of Hawthorne effects caused by the monitors' presence on SI p. 13.

Data

Our evaluation combines data from six sources. First, we collected administrative data on crime, which we timestamped and geocoded to the block level. Cali has unusually high-quality administrative crime data, due in part to the government's efforts to triangulate records between the police, the Coroner's Office, the Attorney-General's Office, and the Office of the Mayor. We also collected administrative data on human rights abuses from the Attorney-General's Office, which has jurisdiction over most cases of police and military misconduct in the city. These data consist of reports by victims and witnesses, which we again timestamped and geocoded to the block level. We describe the administrative data in further detail on SI p. 27.

Second, we conducted a survey of 2,096 residents and business owners between October 17 and December 19, 2019. The survey began during the intervention and continued for roughly a month afterward. We randomly sampled three residents and two business owners on each of 416 randomly selected blocks: 202 from the treatment group, 109 from spillover, and 105 from control. We over-sampled treatment blocks to monitor treatment compliance and document any abuses perpetrated by the soldiers during deployment. We call this our monitoring survey.

Third, we conducted a survey of 7,921 residents and business owners on all blocks. This survey was administered between January 17 and February 25, 2020, roughly two to three months after the intervention ended. We randomly sampled approximately five residents and two business owners per block, following the sampling protocol described on SI p. 10. We call this our endline survey. The survey also included a forced-choice, image-based conjoint experiment inspired by Flores-Macías and Zarkin (2022), and a costly behavioral measure of demand for military policing derived from a hotline we created for respondents to request additional military patrols in their neighborhoods. We describe both of these measurement strategies in further detail below.

Fourth, we collected GPS data and detailed first-hand observations from the civilian monitors hired to accompany the soldiers while on patrol. These data allow us to capture descriptive statistics on the patrols, including any acts of verbal or physical abuse by soldiers. (The monitors recorded no incidents of physical abuse and only one minor incident of verbal abuse[9] by a soldier over the course of the evaluation.) Fifth, we conducted qualitative interviews with 49 civil society leaders, randomly selected from a roster of 364 heads

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[9] This incident occurred on October 16, 2019, at 8:31 p.m. as soldiers were departing a treatment block, apparently at a moment of heightened insecurity. In a comment accompanying the report, the monitor explained that "for our security, the commanding officer told us to leave quickly." Subsequent interviews with the monitor suggest the incident was not serious.
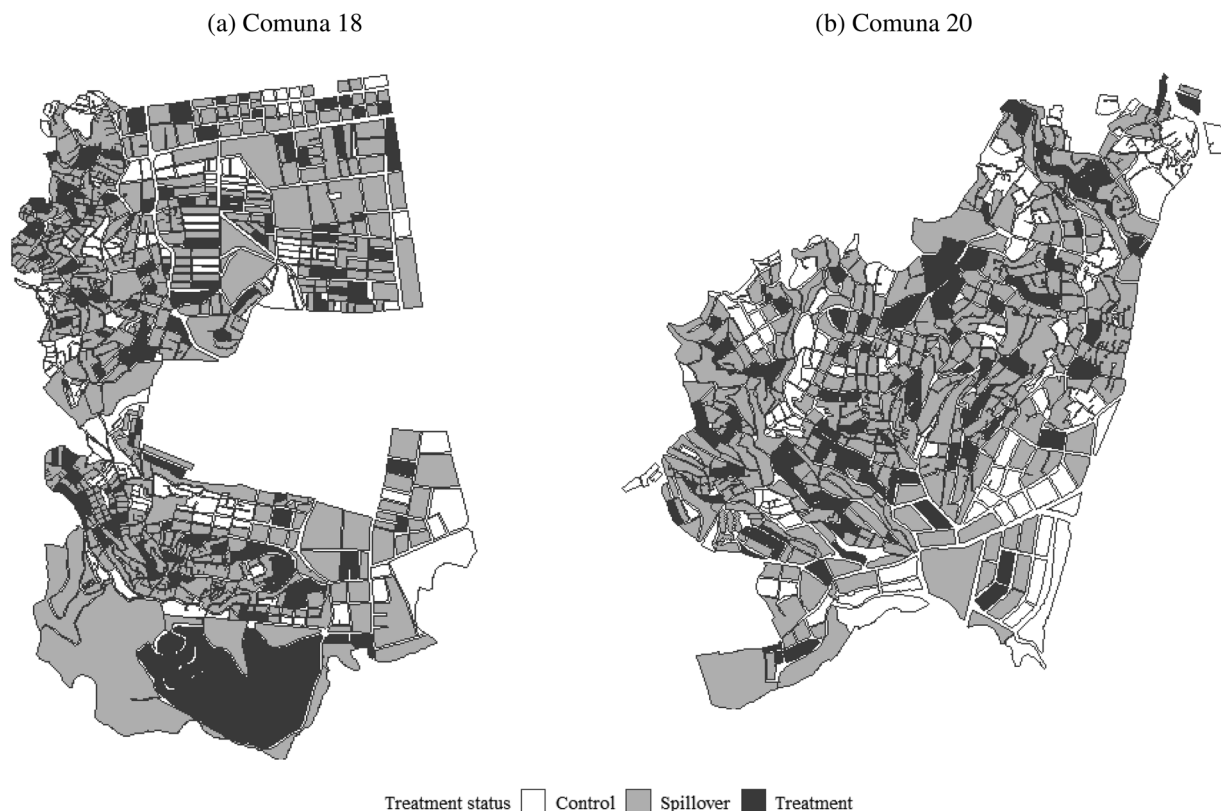
(a) Comuna 18   (b) Comuna 20

Treatment status ☐ Control ▧ Spillover ■ Treatment

FIGURE 1 Treatment assignments. *Note*: Distribution of treatment, spillover, and control blocks in *comuna* 18 (panel a) and 20 (panel b).

of local civil society organizations known as "communal action councils" (*juntas de acción communal* or JACs). (For more on JACs, see Blair et al., 2022.) We describe our qualitative interview sampling frame on SI p. 12. We use these interviews to better understand the program's effects on citizens' political beliefs and behaviors. Finally, we conducted a desk review of the policy platforms of 224 candidates for mayor in Colombia's 2019 municipal elections. We describe the desk review in further detail below.

Estimation

We estimate the treatment and spillover effects of the program in an intention-to-treat (ITT) framework with observations weighted by the inverse probability of assignment to their realized treatment status. To compute these weights, we bootstrap our randomization schedule and estimate the probability of assignment to each treatment condition at the block level across 1,500 replications.

For individual-level outcomes, we estimate

$$y_{ijk} = \theta t_{jk} + \lambda s_{jk} + \beta \mathbf{X_{jk}} + \delta \mathbf{Z_{ijk}} + \alpha_k + \varepsilon_{ijk}, \quad (1)$$

where y_{ijk} denotes the outcome for respondent i on block j in neighborhood k; t_{jk} denotes assign-

ment to treatment; s_{jk} denotes assignment to spillover; $\mathbf{X_{jk}}$ denotes prespecified block-level covariates;[10] $\mathbf{Z_{ijk}}$ denotes prespecified individual-level covariates;[11] α_k denotes neighborhood fixed effects; and ε_{ijk} is an individual-level error term, clustered by block. For block-level outcomes (e.g., texts and calls to the hotline), we aggregate the individual-level covariates up to the block level and do not cluster our standard errors.

Because of the way blocks are situated in space, some (though very few) had 0 probability of assignment to the spillover or control group.[12] Following Aronow and Samii (2017), we omit blocks with 0 probability of assignment to control from our treatment and spillover effect estimates, and omit blocks with 0 probability of assignment to spillover from our spillover effect estimates only. For safety reasons, we opted not to administer the endline survey on five of the 1,254 blocks in the sample. We drop these blocks from our analysis. We report results with multiple comparison corrections on SI p. 19 and discuss deviations from our PAP on SI p. 26.

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[10] We control for area of the block and distance to the nearest police station, military battalion, and public transportation hub based on administrative data.
[11] We control for age, gender, years living on the block, and years of education.
[12] One block had 0 probability of assignment to spillover and three had 0 probability of assignment to control.

Ethics

Military policing has been widely adopted but seldom evaluated in the Global South. We believed a randomized impact evaluation was necessary to rigorously test the claims of advocates (including residents and elected officials in Cali and elsewhere) about the benefits of military policing interventions. Given Cali's persistently high crime rate and its troubled history of police ineffectiveness and abuse, there were compelling reasons to believe military policing might improve human security in the city. Importantly, *Plan Fortaleza* predated our study and would have occurred with or without it. The government had already identified the *comunas* and neighborhoods where the program would be administered. We randomized only the city blocks where soldiers would patrol.

Nonetheless, we sought to anticipate and mitigate any risks that might arise as a result of the intervention and our evaluation of it. We summarize the precautions we took here and discuss them in more depth on SI p. 1. To mitigate the risk that military patrols might exacerbate human rights abuses, we used the observations of civilian monitors to document and report any abuses to Cali's Security and Justice Secretariat, which is responsible for overseeing the military's operations in the city. As noted above, the monitors recorded only one minor incident of verbal abuse involving a soldier during the study. We corroborated the monitors' reports with administrative data from the Attorney-General's Office, which received only one complaint from either of the two *comunas* in our sample over the course of the study. Given the nature of the incident,[13] we believe it was likely unrelated to *Plan Fortaleza*.

To mitigate the risk that respondents or enumerators would face repercussions for participating in our surveys, we arranged for enumerators to receive specialized security training and required them to complete data collection by 3:00 p.m. each day, with most surveys completed before noon. Both before and during the surveys, we consulted multiple local experts—including IPA staff and civil society representatives—to identify blocks that posed especially acute security concerns and adjust our data collection protocols accordingly (e.g., by dropping five especially dangerous blocks from the endline). We did not receive any reports of threats against respondents or enumerators during the study.

Finally, to mitigate the risk that civilian monitors would be recognized and harassed by criminal groups, we recruited monitors who lived outside the *comunas* in our sample and established a direct line of communication between the monitors, the military, and the Security and Justice Secretariat. As discussed above, the monitors were instructed to remain in their vehicles at all times and were outfitted with bulletproof vests and armbands clearly identifying them as civilians. We did not receive any reports of threats against the monitors during the study.

FIGURE 2 Flyer for our behavioral measure of demand for military policing. *Note*: A flyer was given to survey respondents with the number of a hotline they could text or call to request additional military patrols in their neighborhood.

RESULTS

Attitudes toward the police and military

In Table 1, we report the ITT of the *Plan Fortaleza* program on citizens' perceptions of the police and military and their demand for additional military involvement in domestic law enforcement. To construct the dependent variable in columns 1 and 2, we asked respondents if they believe the police and military (1) respond to and (2) investigate crimes effectively; (3) use excessive force; (4) are corrupt; and treat (5) rich and poor Colombians, (6) Afro-Colombians and non-Afro-Colombians, (7) the young and old, and (8) men and women equally. Responses were recorded on five-point Likert scales, which we aggregated into standardized additive indices.[14]

We use our behavioral measure to construct the dependent variable in column 3. At the end of the survey, respondents were given a flyer (depicted in Figure 2) with the number of a hotline they could text or call to request additional military patrols in

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[13] The incident occurred on October 29 at the intersection of two treatment blocks and a spillover block in *comuna* 18. A transit police officer was accused of unjustly restricting a citizen's freedom of movement, likely while enforcing the city's traffic laws.

[14] To create the indices, we first standardize each of the eight component Likert scales. We then add the standardized scales and standardize again. We report ITTs on separate standardized indices of police and military efficacy, abusiveness, and fairness on SI p. 30. These latter analyses were not prespecified.

TABLE 1 Treatment effects on attitudes toward the police and military and demand for military policing.

| | Survey data | | Behavioral data |
	Perceptions of police	Perceptions of military	Demand for military
Treatment	0.030	0.079†	0.053*
	(0.043)	(0.044)	(0.023)
Spillover	−0.021	−0.002	0.033*
	(0.034)	(0.036)	(0.016)
Individual-level controls	✓	✓	✗
Block-level controls	✓	✓	✓
Neighborhood fixed effects	✓	✓	✓
Observations	7,794	7,757	1,167
R^2	0.053	0.043	0.108
Control mean	−0.001	−0.014	0.028

Note: Intention-to-treat (ITT) effects on standardized additive indices of perceptions of the police (column 1) and military (column 2) in the endline survey and demand for military policing (column 3) using our behavioral measure. Columns 1 and 2 include neighborhood fixed effects and individual- and block-level controls. Column 3 includes neighborhood fixed effects and block-level controls. Observations are weighted by the inverse probability of assignment to their realized treatment status. Standard errors, clustered by block in columns 1 and 2, are in parentheses.
$^\dagger p < .10, ^* p < .05, ^{**} p < .01$.

their neighborhood. Each flyer listed a block ID that respondents were asked to include in their texts and calls, allowing us to track the number of requests originating from each block in the sample. Respondents were informed that the hotline was voluntary and anonymous and that there was no guarantee the government would respond with more military patrols. We interpret texts and calls as more "costly" measures of demand for military policing since respondents had to make an effort to communicate their preferences outside the context of the survey itself.

Consistent with Hypothesis 1, we find that *Plan Fortaleza* improved perceptions of the military by 0.079 standard deviations on treatment blocks (column 2), though this effect is only weakly statistically significant at conventional levels[15] ($p = 0.069$). We find no evidence that the program improved perceptions of the military on spillover blocks. Consistent with Hypothesis 2, we find that the program also increased demand for military policing using our costly behavioral measure (column 3). Compared to control blocks, the hotline received 0.053 more calls and texts from treatment blocks ($p = 0.023$) and 0.033 more from spillover blocks ($p = 0.045$). These constitute substantively large increases of 189.3% and 117.9%, respectively, over the control group mean (0.028). Contrary to Hypothesis 3, however, we find no evidence that the program diminished perceptions of the police (column 1).

As another proxy for citizens' attitudes toward the police and military, the endline also included a conjoint experiment inspired by Flores-Macías and Zarkin

(2022). Unlike the questions in Table 1, the conjoint asked respondents to compare the police and military directly to one another, rather than evaluate them independently. Respondents were presented with three pairs of images sequentially. Each image depicted either a soldier or a police officer carrying either a rifle or a pistol, as in Figure 3.[16] We varied the weapon to test whether respondents' perceptions were driven by a preference for the military specifically (in which case they should have preferred the soldier over the police officer regardless of the weapon they carried), militarization more generally (in which case they should have preferred the rifle over the pistol regardless of who was carrying it), or both. For each pair of images, respondents were asked which of the two individuals would (1) make them feel safer, and which was more likely to (2) deter crime, (3) commit abuses, and (4) be corrupt. We aggregate responses to these questions into a standardized additive index.[17]

Our use of images rather than text is advantageous because it more closely approximates the situations in which citizens encounter soldiers and police officers in the real world, given that many of these encoun-

[15] We did not prespecify confidence thresholds in our PAP. We describe results that are significant at the 5% level (or lower) as "statistically significant," and those that are significant at the 10% level as "weakly" or "marginally" statistically significant.

[16] Randomization was restricted such that no respondent saw the same pair of images more than once. To avoid ordering effects, the position of the images on the screen (left or right) was randomized as well. The average marginal component effects (AMCEs) are similar across the three rounds of the conjoint, suggesting there were no "carryover" effects (Hainmueller et al., 2014, p. 22). We omit these latter analyses for compactness, but they are available upon request. Importantly, since we derive ITT estimates from the conjoint using the interaction between the conjoint attributes and respondents' *Plan Fortaleza* treatment assignment, our inferences would only be biased if the intervention somehow made respondents more susceptible to ordering or carryover effects. This strikes us as very unlikely.

[17] We add the number of questions for which respondents selected the rifle over the pistol (or the soldier over the police officer), then standardize. We plot separate results for each index component on SI p. 30. These latter analyses were not prespecified.

(a) Military, rifle   (b) Military, pistol   (c) Police, rifle   (d) Police, pistol

FIGURE 3   Conjoint experiment images. *Note*: Conjoint experiment images depicting either a soldier (panels a and b) or a police officer (panels c and d) carrying either a rifle (panels a and c) or a pistol (panels b and d).

ters are passive, with little interpersonal interaction (Flores-Macías & Zarkin, 2022). Images also convey information that respondents might otherwise infer, confounding their responses (Dafoe et al., 2018). For example, if respondents believe soldiers are more likely than police officers to be male (which, in Colombia, they are), and if they prefer to be policed by men, then a stated preference for soldiers over police officers may be confounded by an unstated preference for men over women. Because the images in Figure 3 are identical except for the uniform and weapon, we eliminate many potential confounders, including age, gender, race, and physical stature. While it is possible to randomize these attributes in a text-based conjoint, images convey more information in a more easily digestible format.

Figure 4 plots the AMCE of each attribute in the conjoint (Hainmueller et al., 2014) as well as the coefficient on the interaction term between each attribute and each respondent's *Plan Fortaleza* treatment assignment.[18] From the top of the figure, we find that respondents expressed a strong preference for military policing overall and that this preference is driven by positive perceptions of the military specifically: while respondents preferred the rifle over the pistol, they preferred soldiers over police officers by a much larger margin. Consistent with Hypotheses 1 and 3, we also find that this preference for the military over the police was statistically significantly stronger on treatment blocks relative to control ($p = 0.049$).

Combined with columns 1 and 2 of Table 1, the results of the conjoint suggest that treatment block residents' preference for the military over the police was more pronounced when they were asked to compare the two directly (as in the conjoint), rather than evaluate each independently (as in Table 1). This is

TABLE 2   Treatment effects on turnout for right-wing candidates.

Treatment	−0.012
	(0.010)
Spillover	0.004
	(0.009)
Individual-level controls	✓
Block-level controls	✓
Neighborhood fixed effects	✓
Observations	7,908
R^2	0.024
Control mean	0.077

Note: Intention-to-treat (ITT) effects on turnout for right-wing candidates based on the endline survey. Specification includes neighborhood fixed effects and individual- and block-level controls. Observations are weighted by the inverse probability of assignment to their realized treatment status. Standard errors, clustered by block, are in parentheses.
[†] $p < .10$, [*] $p < .05$, [**] $p < .01$.

consistent with Pion-Berlin and Carreras's (2017, p. 5) observation that public support for military policing reflects a "comparative judgment call" about the relative capabilities of soldiers and police officers. Residents' preference for the military over the police was stronger on spillover blocks as well, and their preference for the rifle over the pistol was stronger on both treatment and spillover blocks, but these interaction effects are not statistically significant at conventional levels.

Support for conservative candidates

In Table 2, we report the ITT of the intervention on support for right-wing candidates in municipal elections held on October 27, 2019, roughly one month after *Plan Fortaleza* began. These were Colombia's first municipal elections since the 2016 peace accord. Our dependent variable is a dummy

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[18] For compactness, we omit the coefficients on the two treatment assignment base terms, since we derive ITT estimates from the conjoint using the interaction terms only.
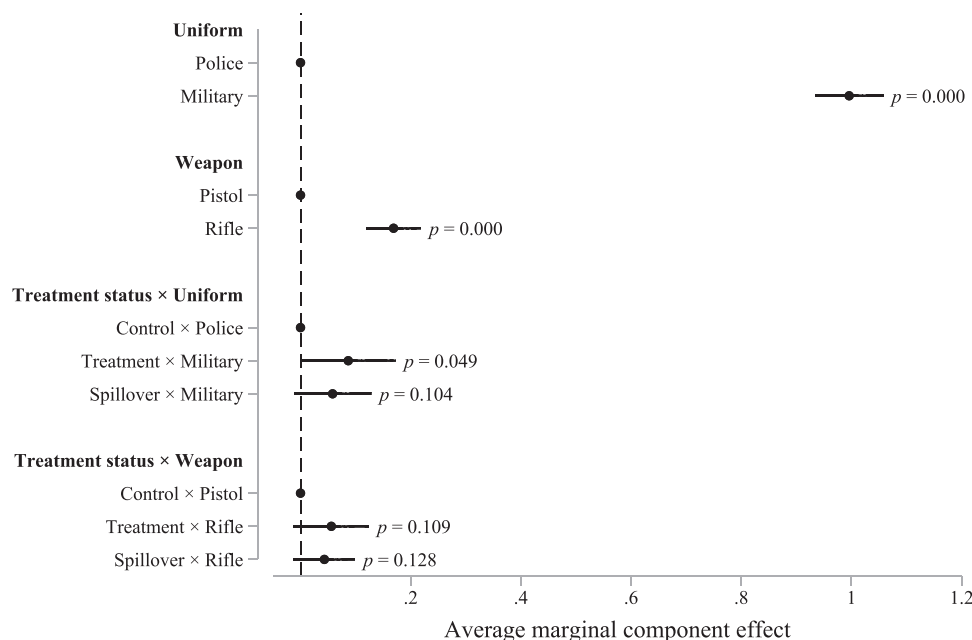
FIGURE 4 Treatment effects on perceptions of the police and military using conjoint experiment. *Note*: Average marginal component effects (AMCEs) in our conjoint experiment. Specification includes neighborhood fixed effects and individual- and block-level controls. Observations are weighted by the inverse probability of assignment to their realized treatment status. Standard errors are clustered by block. Lines denote 95% confidence intervals.

indicating whether respondents reported voting for one of Cali's right-wing mayoral candidates.[19] We identified right-wing candidates using party registration data and official campaign sources and press releases.[20] Contrary to Hypothesis 4, we find no evidence that the program increased turnout for right-wing candidates. As we discuss below, this may reflect a (perhaps surprising) feature of Cali's 2019 municipal elections, in which both right- and left-wing candidates were perceived to be equally tough on crime and equally supportive of military policing. We corroborate this interpretation through our interviews with civil society leaders. On SI p. 37, we show that *Plan Fortaleza* similarly had little to no effect on support for, or participation in, left-wing protests after the intervention was complete.

Commitment to democracy and the rule of law

Finally, in Table 3, we report the ITT on citizens' commitment to democracy and the rule of law. The dependent variables in columns 1 and 2 are dummies

indicating whether respondents agree that military coups are sometimes justified in response to rampant crime (column 1) or pervasive corruption (column 2).[21] These questions were taken from the Latin American Public Opinion Project (LAPOP) survey. To construct the dependent variable in column 3, we asked respondents on a five-point Likert scale whether they believe it is acceptable to bypass the legal system in order to "immediately address" crimes.[22] For column 4, we asked them on a three-point Likert scale whether they believe it is justifiable for civilians to beat criminals rather than call the police.[23] Importantly, this latter question explicitly refers to vigilante violence by civilians; the former question does not and instead encompasses extrajudicial punishment more broadly (including, potentially, by state security forces). We standardize both scales for ease of interpretation.

Consistent with Hypotheses 6 and 7, we find that *Plan Fortaleza* appears to have weakened respondents' commitment to democracy and the rule of

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[19] To avoid conditioning on a posttreatment variable, we code this dummy as a 0 for respondents who did not vote. Our results are substantively similar if we condition on voting.

[20] Two candidates were coded as right wing—Roberto "El Chontico" Ortiz and Danis Rentería—and the rest as left wing. Our results are robust to alternative coding rules, for example, if we reclassify additional marginal candidates as right wing.

[21] Specifically, respondents were asked: "*Algunas personas dicen que en ciertas circunstancias se justificaría que los militares de este páis tomen el poder a través de un golpe de estado.¿ En su opinión se justificaría un golpe de estado cuando hay mucha delincuencia¿ Cuando hay mucha corrupción?*"

[22] "*Por favor, dígame qué tan de acuerdo está con la siguiente afirmación: 'A veces es bueno olvidarse de la ley para responder a un acto delictivo, y así arreglar el problema de inmediato en lugar de esperar a que la ley arregle las cosas'.*"

[23] "*Imagínese que alguien entra la casa de algún vecino de su manzana con una pistola y se lleva el televisor. Cuando él está huyendo, sus vecinos cogen al hombre y quieren azotarlo, en lugar de llevarlo a la policía, porque creen que el hombre terminaría en libertad.¿ Diría que las acciones de la comunidad están nada justificadas¿ Algo justificadas¿ Totalmente justificadas?*"

TABLE 3   Treatment effects on support for military coups and extrajudicial punishment.

	Support for military coups		Support for vigilantism	
	In response to crime	**In response to corruption**	**Bypassing the legal system**	**Bypassing the police**
Treatment	0.048*	0.014	0.086*	−0.014
	(0.020)	(0.020)	(0.040)	(0.043)
Spillover	0.048**	0.007	0.061[†]	0.006
	(0.017)	(0.017)	(0.035)	(0.035)
Individual-level controls	✓	✓	✓	✓
Block-level controls	✓	✓	✓	✓
Neighborhood fixed effects	✓	✓	✓	✓
Observations	7,806	7,830	7,882	7,868
R^2	0.042	0.036	0.027	0.029
Control mean	0.474	0.664	−0.032	0.020

Note: Intention-to-treat (ITT) effects on dummies indicating support for military coups in response to crime (column 1) and corruption (column 2), and on indices of support for bypassing the legal system (column 3) and using vigilante violence (column 4) based on the endline survey. All specifications include neighborhood fixed effects and individual- and block-level controls. Observations are weighted by the inverse probability of assignment to their realized treatment status. Standard errors, clustered by block, are in parentheses.

[†] $p < .10$, * $p < .05$, ** $p < .01$.

law, though the effects vary by outcome. Residents of both treatment and spillover blocks were 4.8 percentage points more likely to support military coups in response to rampant crime ($p = 0.018$ and $p = 0.006$ on treatment and spillover blocks, respectively), but not in response to pervasive corruption. This is perhaps unsurprising, as *Plan Fortaleza* aimed to deter criminals, not to eliminate government corruption (though it may have shaped perceptions of *police* corruption specifically, as we discuss below). Benchmarking these ITT estimates against the 2018 LAPOP survey, we find that residents of these two *comunas* were more likely to support coups in response to crime than LAPOP respondents, both in Colombia and in Latin America as a whole, and that the gap is especially wide in the treatment and spillover groups, to the point that an outright majority of treatment and spillover block residents endorsed coups to combat crime.[24]

Likewise, relative to residents of control blocks, residents of treatment and spillover blocks were, respectively, 0.086 standard deviations and 0.061 standard deviations more likely to support bypassing the legal system to "immediately address" crimes, though the latter effect is only marginally statistically significant at conventional levels ($p = 0.034$ and $p = 0.083$ on treatment and spillover blocks, respectively). Treatment and spillover block residents were no more likely to support vigilante violence. This combination of results suggests that while *Plan Fortaleza* may

have diminished respondents' support for due process protections, it did not instill a belief that civilians themselves should brutalize perpetrators of crime.

DISCUSSION

A small but growing literature suggests that military policing does not deter criminals or improve human rights (Espinosa & Rubin, 2015; Flores-Macías, 2018; Flores-Macías & Zarkin, 2021, 2022; Osorio, 2015). Consistent with these findings, Blair and Weintraub (2023) show in a related study that Cali's *Plan Fortaleza* program did not reduce crime or mitigate human rights abuses. In this paper, we find that *Plan Fortaleza* nonetheless improved citizens' attitudes toward the military, increased their demand for military involvement in domestic law enforcement, and strengthened their support for extrajudicial punishment and military coups. Yet despite the popularity of military policing in these two *comunas*, we find no evidence that *Plan Fortaleza* strengthened citizens' support for the right-wing parties and politicians with whom military policing is usually associated (Pratt, 2007), or that it diminished their support for civil disobedience associated with left-wing groups (as we show on SI p. 37).

Taken together, these results pose at least two puzzles that the existing literature cannot easily resolve. First, if military patrols were ineffective or even counterproductive, why did residents respond to them by endorsing rather than rejecting the military's role in domestic policing operations—and, indeed, in governance more generally? Second, if residents embraced military policing despite its apparent ineffectiveness,

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[24] According to the 2018 LAPOP survey, 39.11% of all respondents—and 25.78% of Colombians—said a coup was justified in response to crime. Computing predicted values from column 1 of Table 3, an estimated 45.84% of control block residents in our sample said a coup was justified in response to crime. An estimated 50.10% of treatment and spillover block residents in our sample said the same.

why did they not also embrace the right-wing parties and politicians with whom military policing is typically associated? Here we use a combination of quantitative and qualitative data to probe potential explanations.

We consider three possible solutions to the first puzzle. First, it is possible that *Plan Fortaleza* improved residents' subjective perceptions of safety even if it did not mitigate the objective prevalence of crime. Citizens' subjective experiences of security often diverge dramatically from the objective reality (e.g., Quinney, 1970), and it is possible that *Plan Fortaleza* widened this gap in Cali. As we show on SI p. 28, however, we find no evidence that *Plan Fortaleza* improved residents' subjective perceptions of safety. Indeed, if anything, it appears to have diminished them, though the ITT is not statistically significant. An improvement in subjective perceptions of safety thus seems unlikely to explain our first puzzling combination of results.

Second, it is possible that residents perceived soldiers as more effective than police officers at preventing crime, even if they were not, and that *Plan Fortaleza* reinforced this perception. Recent research from the United States suggests that citizens who trust and respect the police are more likely to embrace militarized policing strategies (Lockwood et al., 2018; Moule et al., 2019a, 2019b); likewise, recent studies from Latin America suggest that citizens who trust the military are more likely to endorse military policing (Pion-Berlin & Carreras, 2017). *Plan Fortaleza* may have magnified citizens' already favorable perceptions of the military's efficacy relative to the police, which may in turn have strengthened their demand for further military involvement in domestic policing operations.

Our results in Table 1 and Figure 4 are consistent with this explanation. Our qualitative interviews are consistent with it as well. We conducted qualitative interviews with 49 civil society leaders from the two *comunas* in our sample between June 26 and July 15, 2020, roughly seven months after the end of the intervention. Due to the COVID-19 pandemic, we conducted all interviews by phone. We confirmed the *comuna* and neighborhood in which each leader lived and worked, but to maintain anonymity we did not ask for their addresses, and so do not know whether their blocks were assigned to treatment, spillover, or control. Nonetheless, the qualitative data help us understand and contextualize our quantitative results. We describe the sampling frame for our qualitative interviews on SI p. 12.

Qualitative interview respondents almost invariably described the military as more effective than the police at deterring criminals. As one respondent explained, "here in Cali, young people have lots of respect for soldiers. The police they don't respect. The hooligans on the street listen to the military."[25] Another observed that soldiers "get much more respect when they walk down the street."[26] When criminals encounter the police, "they throw rocks at them;"[27] but "do you think they would dare mess with a soldier?"[28] Of the 49 leaders we interviewed, only two opposed military patrols, and these two also opposed patrols by the police.[29] Nearly all others voiced strong, unequivocal support for military policing, and even the few who expressed some ambivalence nonetheless described military patrols as necessary to prevent crime.[30] As one of these respondents explained, "do I like it [military policing]? No. But yes, I think it's necessary."[31]

Finally, it is possible that residents viewed military policing partly or even primarily as a mechanism for curbing police misconduct rather than reducing crime, and that *Plan Fortaleza* reinforced this view. This would be consistent with recent research in the United States suggesting that, while support for militarized policing programs tends to be high among police officers—higher even than among the elected officials responsible for authorizing these programs in the first place (Turner & Fox, 2019)—even they do not necessarily think militarization is effective for deterring criminals (Meitl et al., 2020). Residents of treatment and spillover blocks in Cali may have similarly prioritized the potential benefits of military policing beyond crime prevention alone.

Importantly, this explanation may hold even if *Plan Fortaleza* actually *exacerbated* police misconduct, as Blair and Weintraub (2023) suggest that it did. Soldiers and police officers "coproduced" security in Cali,[32] potentially creating more opportunities for police corruption and abuse. Police officers may have also perceived militarized policing tactics as more "elite" (Kraska & Kappeler, 1997), and so may have aggressed against civilians to demonstrate their own bellicosity. Recent studies from the United States suggest that support for militarization tends to be especially high among police officers who express predatory or discriminatory views toward civilians (Welch et al., 2023); military policing in Cali may have inadvertently encouraged misconduct among precisely those police officers who were already most prone to it. Ironically, as long as residents blamed the police rather than the military for any increase in police corruption and abuse, *Plan Fortaleza* may have strengthened support for military policing as a way to mitigate a problem that the intervention itself exacerbated.

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[25] ID 234, June 27, 2020, *comuna* 20, El Cortijo.

[26] ID 358, July 14, 2020, *comuna* 18, Desconocido.

[27] ID 312, June 27, 2020, *comuna* 18, Caldas.

[28] ID 018, July 2, 2020, *comuna* 18, Alto Melendez.

[29] ID 254, July 6, 2020, *comuna* 18, Sector Los Mandarinos Alto Jordán; ID 359, July 14, 2020, *comuna* 18, Desconocido.

[30] For example, ID 326, July 1, 2020, *comuna* 18, Francisco Eladio.

[31] ID 355, June 30, 2020, *comuna* 20, El Cortijo.

[32] Under Colombian law, while soldiers can temporarily detain suspected criminals, only police officers can legally effect arrests. This helps explain why residents of treatment and spillover blocks were more likely than control block residents to witness police officers making arrests, as we show on SI p. 39.

Consistent with this explanation, we show on SI pp. 29 and 38 that, relative to residents of control blocks, residents of treatment and spillover blocks were, respectively, 13.6% ($p = 0.088$) and 17.3% ($p = 0.008$) more likely to describe the police as corrupt (though the former effect is only weakly statistically significant at conventional levels), and residents of treatment blocks were 250% ($p = 0.057$) more likely to witness police corruption while the intervention was ongoing (though this effect is again only weakly statistically significant). They were no more likely to describe the military as corrupt, and no more likely to witness corruption by soldiers either during or after the intervention.

Moreover, residents of treatment blocks were 246.7% more likely to witness police abuse during the intervention ($p = 0.001$), and residents of spillover blocks were 26.3% more likely to witness police abuse after the intervention was complete ($p = 0.011$). (The effects on police abuse are positive and substantively large but not statistically significant on spillover blocks during the intervention and on treatment blocks afterward.) We also show on SI p. 29 that residents of treatment blocks were more likely than residents of control blocks to witness abuses by soldiers during the intervention. But this effect is only weakly statistically significant ($p = 0.071$), and reports of military abuse in the monitoring survey were exceedingly rare: just 10 out of 2,085 monitoring survey respondents—0.48% of the sample—witnessed either verbal or physical abuse by a soldier.

Our qualitative interviews are also consistent with this explanation. As one civil society leader observed, "there are many citizens who are more afraid of the police than of criminals themselves."[33] Another explained, "of course, yes, I support them [military patrols]. I live in fear of the police."[34] Even respondents who were aware of the military's deficiencies nonetheless tended to describe soldiers as less corrupt and abusive than the police. As one respondent observed, "nobody respects the police because of all the corruption they've been involved in. It's not that there's no corruption in the military; obviously there is. But soldiers are less likely to be corrupt."[35]

Our qualitative interviews offer a potential solution to the second puzzle as well. While we find that *Plan Fortaleza* improved attitudes towards the military and generated demand for additional military policing, we find no evidence that it increased support for right-wing parties and politicians. One possible explanation for this puzzling combination of results is that the partisan divide on military policing is not especially salient in Cali. When asked about

the 2019 municipal elections, most leaders we interviewed were unfamiliar with the candidates' platforms and were unsure which ones supported military policing. Fifteen respondents—a plurality—simply did not know the candidates' positions on the issue. Thirteen respondents associated military policing with left-wing candidates, and 12 associated it with right-wing candidates. Four associated it with *all* candidates. As one respondent explained, "all of the candidates supported them [military patrols]";[36] another claimed that "all the candidates" and "all good people and the whole of the city [supported the patrols]… It's not about one side or another."[37]

A detailed desk review of the platforms and related local and national news coverage of the top four mayoral candidates suggests that their positions on this issue were, indeed, indistinguishable. Only one of the top four candidates—Danis Rentería, who served more than 24 years in the army—included military involvement in domestic policing operations as an explicit component of his platform.[38] The platforms of the other top three candidates did not mention military policing. This helps explain why most respondents either did not know which candidates supported military policing or believed that all candidates supported it equally. It also helps explain why we find no evidence of increased turnout for right-wing candidates in response to *Plan Fortaleza*.

This absence of a partisan divide on military policing is not unique to Cali. We replicated our desk review for 220 additional mayoral candidates across 30 Colombian departmental capitals that held municipal elections in 2019.[39] Of these 220 candidates, only 32 articulated an unambiguous position regarding the military's involvement in domestic policing operations: 24 candidates were in favor, while 8 were against. Tellingly, we find little to no evidence of an ideological divide between those who supported and opposed military policing. Of the 24 candidates who supported it, eight were affiliated with left-wing parties and 10 with right-wing parties, while the remainder were independents. Of the eight candidates who opposed military policing, three were affiliated with the left and three with the right. The remainder were independents. In other words, to the extent that candidates in Colombia's 2019 municipal elections articulated a stance on military policing—and most

[33] ID 159, June 26, 2020, *comuna* 18, Los Chorros.
[34] ID 168, June 26, 2020, *comuna* 18, Belén.
[35] ID 358, July 14, 2020, *comuna* 18, Desconocido.

[36] ID 122, June 26, 2020, *comuna* 18, Lourdes
[37] ID 071, June 27, 2020, *comuna* 18, Polvorines.
[38] Rentería proposed "integrating the military with the police" (see https://www.elpais.com.co/elecciones-2019/conozca-las-propuestas-de-danis-renteria-para-llegar-a-la-alcaldia-de-cali.html).
[39] Arauca, Armenia, Barranquilla, Bogotá, Bucaramanga, Cartagena, Cúcuta, Florencia, Ibagué, Inírida, Manizales, Medellín, Mitú, Mocoa, Montería, Neiva, Pasto, Pereira, Popayán, Providencia, Puerto Carreño, Quibdó, Riohacha, San José del Guaviare, Santa Marta, Sincelejo, Tunja, Valledupar, Villavicencio, and Yopal.

did not—their support or opposition appears to have been mostly orthogonal to their or their party's place on the ideological spectrum.

CONCLUSION

Governments throughout the Global South rely on their armed forces to engage in domestic policing operations. Yet rigorous empirical evidence on the impact of military policing on citizens remains limited. Evaluating military policing poses severe inferential challenges, given that these interventions almost always target areas where crime is rampant and police forces are overwhelmed. We help fill this gap with what we believe to be the first experimental evaluation of the social and political effects of this increasingly common *mano dura* approach to domestic law enforcement at the micro level.

Using a combination of surveys, a conjoint experiment, a costly behavioral measure, and qualitative interviews, we find that military policing in Cali improved perceptions of the military both in absolute terms and (especially) relative to the police. It also strengthened demand for further military involvement in domestic law enforcement. We find little to no evidence that the intervention boosted turnout for right-wing candidates or eroded support for left-wing causes. We do find, however, that the intervention weakened citizens' commitment to democracy and the rule of law. We use qualitative interviews and a desk review of the policy platforms of Colombian mayoral candidates to help illuminate and contextualize these quantitative results.

Our findings have important implications for our understanding of the increasingly blurred distinction between the military and the police in ostensibly democratic countries. This distinction has long been considered a "preeminent feature of the modern nation-state" (Kraska, 2007, p. 501; see also Giddens, 1987; Huntington, 1957), and scholars have expressed concern about the macro-level repercussions of military policing for democracy and the rule of law (Dunlap et al., 1999; Ricks, 1997). Our results suggest that the micro-level consequences may be equally important, reshaping citizens' understanding not just of domestic law enforcement, but of governance more generally. Military policing is usually framed as a necessary temporary measure to combat crime. But fusing the roles of the military and the police in the short term may generate demand for greater fusion in the long run, even where soldiers prove no more effective than police officers at deterring criminals.

As with any study focused on a single site, we cannot be sure how far our results will generalize. As noted above, however, Cali is an important test case in and of itself, and it shares a number of similarities with other cities where military policing is currently being implemented, including extreme socioeconomic inequality; a concentration of crime in the city's most disadvantaged neighborhoods; and a police force plagued by accusations of corruption, abuse, and collusion with organized criminal syndicates. While Cali suffers from especially high crime rates, this is true of most cities in which military policing is currently being implemented or considered. Indeed, it is precisely these high crime rates that make military policing seem necessary in the first place.

But ultimately, we can only speculate as to the generalizability of our results to settings that are very different from Cali or to interventions that are much longer (or shorter) than *Plan Fortaleza*. It is possible that more prolonged exposure to military policing would further increase support and demand for it. Flores-Macías and Zarkin (2022), for example, show that after more than a decade of constabularization in Mexico, citizens living in areas that witnessed recurring confrontations between drug traffickers and the Mexican Armed Forces tended to express more favorable perceptions of the military and military policing, despite other studies suggesting that these confrontations exacerbated violence and human rights abuses (Espinosa & Rubin, 2015; Flores-Macías & Zarkin, 2023; Osorio, 2015). It is possible that prolonged exposure to an intervention like *Plan Fortaleza* would have similar effects. Alternatively, it is possible that longer, more routine deployments would have induced Colombian soldiers to behave more like their police counterparts, exposing civilians to military corruption and abuse and potentially reversing the improvement in perceptions of the military that we detect in our data.[40] We cannot be sure.

Nonetheless, while the program we evaluated was short, and while it targeted only a subset of the city's neighborhoods, it was similar to many military policing interventions in its temporal and geographical scope (Flores-Macías & Zarkin, 2021, p. 526). Our evaluation is also analogous to studies of hot spots¿ policing, which similarly focus on "place-based" interventions involving a concentration of resources on specific neighborhoods within particular cities for a limited amount of time (Blattman et al., 2021; Braga et al., 2019; Collazos et al., 2019). Our finding that military policing may shape citizens' attitudes and behaviors in ways that are detrimental to democracy and the rule of law adds to a nascent empirical literature on the adverse unintended consequences of military policing interventions (Espinosa & Rubin, 2015; Flores-Macías, 2018; Flores-Macías & Zarkin, 2021, 2023; Osorio, 2015). Taken together, these studies

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[40] On SI p. 17, we find little to no evidence of "dosage" effects among the blocks in our sample, but these results are only suggestive and are not causally identified.

provide reason for caution as policymakers consider similar interventions in Colombia, throughout Latin America, and beyond.

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Additional supporting information can be found online in the Supporting Information section at the end of this article.