After Rebel Governance: A Field Experiment in Security and Justice Provision in Rural Colombia*

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Abstract

How can states prevent armed groups from exploiting local governance gaps to (re)establish territorial control during transitions to national peace? We report results from an experimental evaluation of the *ComunPaz* program, a scalable, inexpensive intervention that sought to displace mechanisms of rebel governance by harnessing complementarities between state and communal authorities and improving the quality of security and justice provision in areas once dominated by FARC, Colombia's largest rebel group. We find that *ComunPaz* improved the quality of dispute resolution at the local level, increased citizens' trust in (some) state institutions, and strengthened coordination between state and communal authorities. We also find suggestive evidence that the program reduced citizens' trust in, and reliance on, armed groups. The program did not, however, increase reliance on either state or communal authorities to resolve disputes, nor did it increase citizens' trust in communal institutions. We discuss the implications of our findings for peacebuilding and statebuilding in countries transitioning out of civil war.

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Statebuilding in conflict and post-conflict settings is a slow, arduous process. States transitioning out of civil war typically struggle to (re)establish authority in areas previously governed by armed groups. They usually have limited physical infrastructure and operate under severe financial and human capital constraints. These constraints are compounded by citizens' distrust, which is often the result of state repression or neglect. If states remain weak, then governance gaps that emerge as rebel groups demobilize may exacerbate crime and conflict at the local level. They may also create opportunities for new or existing armed groups to consolidate territorial control, thus inhibiting statebuilding. How can states recovering from civil war avoid an escalation of local disputes during transitions to national peace? How can they prevent armed groups from exploiting local governance gaps to seize territories abandoned by their newly demobilized rivals?

Statebuilding in areas previously governed by armed groups depends crucially on the provision of mechanisms to adjudicate crimes and resolve disputes. For states recovering from civil war, "the quality of dispute institutions embodies the quality of local governance more generally" (Arjona 2016, 69). Because these states are weak, however, their most viable strategy for resolving disputes fairly and efficiently is to partner with communal institutions (Baldwin 2015)—the localized, informal mechanisms for sustaining order that often emerge or proliferate during civil war. Examples abound: *shuras* in Afghanistan (Murtazashvili 2016), secret societies in Liberia (Blair, Karim and Morse 2019), clan houses in East Timor (Hohe 2003), etc. Communal institutions often have local legitimacy and access to inside information that states lack; states (even weak ones) often have coercive capacity that communal institutions lack. We argue that by exploiting these complementaries, states can reduce the risk of local conflict escalation and prevent the emergence of new modes of rebel governance.

We test this argument in Colombia, where the demobilization of the *Fuerzas Armadas Revolucionarios de Colombia* (FARC), the country's largest rebel group, has created an opportunity for the state to project authority into rural regions for the first time in more than 50 years. FARC and other armed groups coopted or created a variety of mechanisms for maintaining order in the territories they controlled, including, crucially, mechanisms of dispute resolution (Arjona 2016). Now

that FARC has demobilized, the state must fill the resulting governance gap before FARC dissidents and splinter groups, competing rebel groups (especially the *Ejército de Liberación Nacional*, or ELN), neo-paramilitaries, or other criminal organizations intervene to take FARC's place.

We experimentally evaluate an inexpensive, scalable intervention designed to fill this governance gap by exploiting complementarities between the state and communal institutions, some of which were harnessed by FARC during the conflict. The *ComunPaz* program sought to help state authorities understand the comparative advantages and legal roles and responsibilities of their communal counterparts, and vice versa; create opportunities for them to build trust with each other, and with citizens; and develop strategies to increase communication and coordination between them. The program focused on police officers, Police Inspectors, and *Juntas de Acción Communal* (JACs)—communal institutions that are key to local governance in rural Colombia (Vargas Castillo 2019). Many JACs were integrated into FARC's local governance arrangements during the civil war; most continue to function even in communities that FARC has abandoned, but without the coercive capacity that FARC provided. *ComunPaz* sought to exploit these legacies of rebel governance for purposes of statebuilding. Our evaluation occurred at a critical transitional moment, after FARC had demobilized but before either the state or competing armed groups had taken its place.

Our sample consists of 149 communities across four rural regions of Colombia, all of which were severely affected by the civil war and most of which were previously governed by FARC or other armed groups. 72 communities were randomly assigned to participate in *ComunPaz*, which was administered in four modules over the course of eight months. We evaluate the impact of the program using surveys of residents, JAC leaders, police officers, and Police Inspectors. The residents survey includes endorsement and list experiments designed to measure support for, and reliance on, armed groups—a potentially sensitive topic. We combine the surveys with costly behavioral measures designed to operationalize residents' willingness to petition for closer coordination between state and communal authorities, and JAC leaders' willingness to act on those petitions. We corroborate and contextualize our quantitative results using detailed qualitative field

reports from *ComunPaz* facilitators.

We find that *ComunPaz* reduced the prevalence of unresolved and violent disputes at the community level. Consistent with our theoretical framework and pre-analysis plan (PAP),[1] we also find suggestive evidence that the program decreased reliance on armed groups, especially among residents, and diminished perceptions of armed groups among residents as well. Importantly, we find that reliance on armed groups was already rare in these communities, and perceptions of them were already unfavorable. This is unsurprising, since FARC had already largely demobilized by the time of our study. But we also find evidence of multiple additional armed groups competing to supplant FARC. *ComunPaz* appears to have driven both perceptions of and reliance on these armed groups nearly to zero, potential floor effects notwithstanding. The program also appears to have improved perceptions of some state authorities, especially those with the most frequent and direct contact with their communal counterparts.

Also consistent with our theoretical framework, we find that the program increased coordination between the state and communal institutions, and improved the cohesiveness and functionality of communal institutions themselves. Interestingly, and contrary to our expectations, the program appears to have *weakened* demand for additional coordination between state and communal authorities as captured by our costly behavioral measures. We interpret this as evidence that the program helped satisfy demand for coordination that already existed in both treatment and control communities, as reflected in the very large number of petitions filed in all communities, regardless of treatment status. If *ComunPaz* helped satisfy this demand, then it is unsurprising that we find lower demand in treatment communities after the program was complete.

More surprisingly, we find no evidence that *ComunPaz* improved perceptions of communal institutions or increased understanding of their roles and responsibilities under Colombian law. Nor do we find evidence that the program strengthened state or communal authorities' awareness of the most serious disputes in treatment communities, or that it fostered consensus around how those disputes should be resolved. Perhaps most surprisingly, we find no evidence that the program

[1] Our PAP can be found at https://osf.io/yw7ts/.

increased reliance on either state or communal authorities to resolve disputes. In the discussion we combine our quantitative and qualitative data to explore several potential explanations for this result, including the program's emphasis on direct dialogue as a mechanism for resolving disputes.

Our study contributes to multiple bodies of research. First, we contribute to the literature on rebel governance by testing mechanisms for preventing armed groups from filling local governance gaps during transitions to national peace (Arjona 2016; Arjona, Kasfir and Mampilly 2015; Mampilly 2011; Stewart 2018). Second, we contribute to research on cooperation and contestation between state and communal authorities by exploring ways to leverage the complementarities between them (Baldwin 2015; Blair 2019; Bodea and LeBas 2016; Van der Windt et al. 2019). Third, we contribute to studies on security and justice provision in conflict and post-conflict countries. Despite the complementarities between states and communal institutions, almost all of these studies focus exclusively on one or the other. This is problematic because strengthening the state at the expense of communal institutions (or vice versa) has been shown to have unanticipated adverse consequences (Blair, Karim and Morse 2019; Blattman, Hartman and Blair 2014). Finally, we contribute to the literature on peacebuilding, statebuilding, and security sector reform by showing how states can benefit from cooperation with communal institutions that are often marginalized or ignored during statebuilding processes (Call and Wyeth 2008; Paris and Sisk 2009).

THEORETICAL FRAMEWORK

COMPLEMENTARITIES BETWEEN STATES AND COMMUNAL INSTITUTIONS

Armed groups often create or coopt institutions to adjudicate crimes, resolve disputes, and sustain local order amidst the national upheaval of civil war. They do this not just for short-term instrumental reasons—for example, to facilitate the extraction of resources—but also to advance long-term political goals. Resolving disputes reliably and effectively is one of the most basic ways that armed groups can address the quotidian challenges of communal life during civil war. By providing these services, they can win civilians' "hearts and minds," prevent defection, and consolidate territorial

control (Arjona 2016; Arjona, Kasfir and Mampilly 2015; Mampilly 2011; Stewart 2018).

When rebel groups disarm, they leave behind governance gaps that states must fill in order to prevent local conflicts from escalating into regional or national crises and to impede other armed actors from seizing newly abandoned territories. But filling these governance gaps is not easy. The same factors that facilitate insurgency, such as low population density and "rugged terrain," also tend to impede the projection of state power after insurgents disarm. State infrastructure may be dilapidated, and state bureaucracies may be too weak and dysfunctional to serve local (especially rural) populations (Dasgupta and Kapur 2020). Legal systems that guarantee rights and freedoms may limit states' ability to threaten violence against civilians, which is precisely what makes armed group dispute resolution so efficient, if also despotic (Kasfir 2015). Communities that are accustomed to autonomy may also resist the (re-)imposition of state rule.

We argue that states can overcome these challenges by leveraging underutilized complementarities with communal institutions—the localized and often informal mechanisms of governance that typically arise or proliferate during periods of civil war. Communal institutions tend to be locally legitimate and well informed about the most important sources of disputes among citizens. They can relieve the burden on the state by adjudicating non-violent crimes and petty domestic conflicts that might otherwise overwhelm the justice system. If states could leverage these comparative advantages, they could extend their authority without incurring the prohibitive costs of developing the infrastructure and bureaucracy necessary to serve remote, sparsely populated areas (Baldwin 2015).

But communal institutions may suffer from pathologies of their own. As noted above, during civil war, they may be coopted by armed groups to facilitate rebel governance. In Mozambique, for example, the Resistência Nacional Moçambicana (RENAMO) relied on traditional chiefs known as *régulos* to resolve disputes, organize the provision of food and other supplies to RENAMO fighters, and ensure civilians' adherence to RENAMO's decrees. Similarly, the Sudan People's Liberation Movement (SPLM) exploited customary leaders to maintain order and enforce compliance with

SPLM dictates (Arjona 2016).[2] After armed groups demobilize, the communal institutions they coopted may continue to function, but without the benefit of the coercive capacity that armed groups once provided, rendering their decisions unenforceable. Communal authorities may also resolve disputes in ways that contravene state laws and due process protections (Hariri 2012).

We argue that peace processes create windows of opportunity for states to resolve these pathologies and foster symbiotic relationships with the same communal institutions that once served as appendages of rebel rule. Communal institutions can provide the local legitimacy and inside information that states lack; they can help identify conflicts before they escalate and bring disputants to the bargaining table (Blattman, Hartman and Blair 2014). States can provide the coercive capacity that communal institutions lack; they can ensure that communal authorities' decisions are enforceable, and that they are reached in accordance with legal rules and procedures. By exploiting these complementarities, states can project power, improve the quality of dispute resolution at the local level, and prevent new or existing armed groups from gaining a foothold in communities recently abandoned by their newly demobilized rivals.

THREE OBSTACLES TO LEVERAGING THE COMPLEMENTARITIES

Exploiting the complementarities between states and communal institutions requires overcoming at least three obstacles related to (1) *information*, (2) *trust*, and (3) *coordination*. First, state authorities may be unaware of the legal roles and responsibilities of their communal counterparts, and vice versa. Citizens, too, may be unaware of the legally circumscribed division of labor between states and communal institutions. In many developing countries, communal authorities routinely (if inadvertently) exceed the legal limits of their powers, thus undermining the state's jurisdictional claims (Blair 2019). For their part, state authorities often do not know the most important sources of disputes at the local level, thus limiting their ability to respond to citizens' complaints.

Second, in countries recovering from civil war, citizens may distrust state institutions, may

[2] In other cases, communal institutions may help citizens mobilize to resist rebel rule. In Peru, for example, *rondas campesinas* that were organized to adjudicate disputes in the 1970s were instrumental in repelling incursions by the Shining Path (Arjona 2016).

view them as corrupt and biased, and may (rightly) fear that wartime patterns of state predation or neglect will persist into the post-conflict period. Communal authorities may share this distrust, and may have "no reason to support increased administrative intrusiveness" by the state, "even if it is justified in terms of an external threat" (Leander 2002, 9). Conversely, in areas previously controlled by armed groups, state authorities may (accurately) perceive their communal counterparts as accessories to rebel rule, and thus as illegitimate. If distrust continues into the post-conflict period, the state may seek to marginalize or eradicate communal institutions, rather than accommodate them.

Third and related, state and communal systems of dispute resolution may develop in isolation, with few or no mechanisms to coordinate their activities. Coordination problems can arise even when state and communal authorities understand and trust one another, and even when their interests are aligned. Coordination problems may also afflict citizens and communal institutions. Even citizens who agree on the legal division of labor between the state and its communal counterparts may disagree about how, exactly, they should seek redress for grievances in any given case. Moreover, the inclusiveness and consensus inherent to much local-level decision-making may leave communal institutions especially vulnerable to shirking, dissension, and discord within their ranks.

These three obstacles reinforce a suboptimal equilibrium in which states and communal institutions operate independently of, or in conflict with, one another. These dynamics are common throughout the Global South. In Mozambique, "widely divergent interpretations" of statutes delineating the division of labor between state and communal authorities foment jurisdictional competition and undermine dispute resolution (Lubkemann, Kyed and Garvey 2011, 41). In Guatemala, distrust of the state underlies citizens' continued reliance on local "civil patrols" to provide redress for grievances, often through vigilantism (Bateson 2013). In Iraq, "vague provisions" in the law allow for coordination between state and communal authorities, but mechanisms for facilitating coordination are weak or non-existent (Asfura-Heim 2011, 270). The intervention we evaluate aimed to help state and communal authorities resolve precisely these problems in Colombia.

SETTING AND INTERVENTION

Colombia is the site of the world's longest civil war. In 2016, after more than half a century of conflict, the government signed a peace agreement with FARC, the country's largest rebel group. But as with previous rebel and paramilitary demobilizations in the mid 1980s and early 2000s, the transition to peace has been tumultuous, with a variety of armed groups vying to fill governance gaps left in FARC's wake. Indeed, according to our qualitative data (described in further detail below), nearly one-third (23) of the 72 treatment communities in our sample experienced active armed group presence at the time of our study. Armed group presence was most pervasive in the region of Nordeste Antioqueño, where the ELN had already begun to make incursions into communities previously controlled by FARC's 36th front.[3] But throughout our sample, communities reported the presence of large armed groups linked to drug cartels with (ostensibly) political agendas (for example the *Autodefensas Gaitanistas* and *Los Rastrojos*),[4] smaller groups that are more clearly criminal (for example, *Los Pacheli*),[5] and FARC *disidencias*, some of which had already begun to use kidnapping and other forms of intimidation to interfere with state security and justice provision.[6]

The Colombian government is currently pursuing multiple strategies to consolidate state authority, prevent local conflict escalation, and impede the establishment of new forms of rebel governance in territories previously controlled by FARC, many of which are now governed by communal institutions that were created by, or adapted to, rebel rule. The intervention we evaluate focuses on three actors in particular: police officers, Police Inspectors, and *Juntas de Acción Comunal* (Community Action Councils, or JACs). Police officers are responsible for investigating serious crimes; Police Inspectors are mandated to adjudicate petty crimes and coordinate with the police when serious crimes occur.[7] Police Inspectors are believed to be critical for improving citi-

[3]Facilitator report for Module 2 (November 27, 2018), community #36; facilitator report for Module 2 (December 15, 2018), community #29.
[4]Facilitator report for Module 2 (December 7, 2018), community #39.
[5]Facilitator report for Module 2 (November 16, 2018), community #44.
[6]Facilitator report for Module 3 (February 26, 2019), Ariari Region, municipality #5.
[7]Administratively, and somewhat confusingly, Police Inspectors fall under the mayor's office, and are not members

zens' perceptions of the Colombian government in regions affected by the civil war. But they are often deployed to large, sparsely populated areas, and in many cases they struggle to serve all or even most of the communities in their jurisdictions.

JACs are communal institutions that resolve disputes, organize the provision of local public goods, and more generally sustain order in rural Colombian communities. In most cases they are inclusive, and typically count half or more of the population of any given community as "members." JACs enjoy high levels of local legitimacy, are accessible and affordable, and have a presence in almost all communities. But they receive little if any training, and many are unfamiliar with the duties, powers, and constraints imposed on them under Colombian law. During the conflict, many of them established symbiotic relationships with FARC (Vargas Castillo 2019), which demanded that all households enroll in JACs,[8] enforced attendance at JAC meetings,[9] and ensured participation in collective labor days convened by JACs.[10] While many JACs enjoyed some degree of autonomy under rebel rule, they also benefited from FARC's coercive capacity, which made their decisions easier to enforce. Communities in our sample reported that conflicts have become more pervasive and harder to resolve now that FARC has demobilized.[11] These communities have also become more susceptible to incursions by rival armed groups.

The relationships between police officers, Police Inspectors, and JACs are beset by all three of the obstacles discussed above, related to *information*, *trust*, and *coordination*. As part of our endline survey (described below), we asked JAC leaders seven relatively simple factual questions about the extent and limits of their authority under Colombian law. The modal JAC leader in the control group answered fewer than half of these questions correctly. Fewer than one-third of control group residents expressed trust in Police Inspectors at endline, and fewer than one-third expressed a belief that Police Inspectors resolve disputes fairly and effectively. Perceptions

of the police force, though they interact frequently with the police and are considered an important mechanism for expanding police presence.

[8] Villager #3. Interviewed by Andrés Vargas, Ariari Region, June 2016; facilitator report for module 2 (December 15, 2018), community #29.

[9] Communal leader #4 and Villager #16. Interviewed by Andrés Vargas, Eastern Antioquia region, August 2016.

[10] Community leader #3. Interviewed by Andrés Vargas, Ariari Region, June 2016.

[11] Facilitator report for module 2 (December 15, 2018), community #29; facilitator report for module 3, Centro del Valle del Cauca, municipality #3.

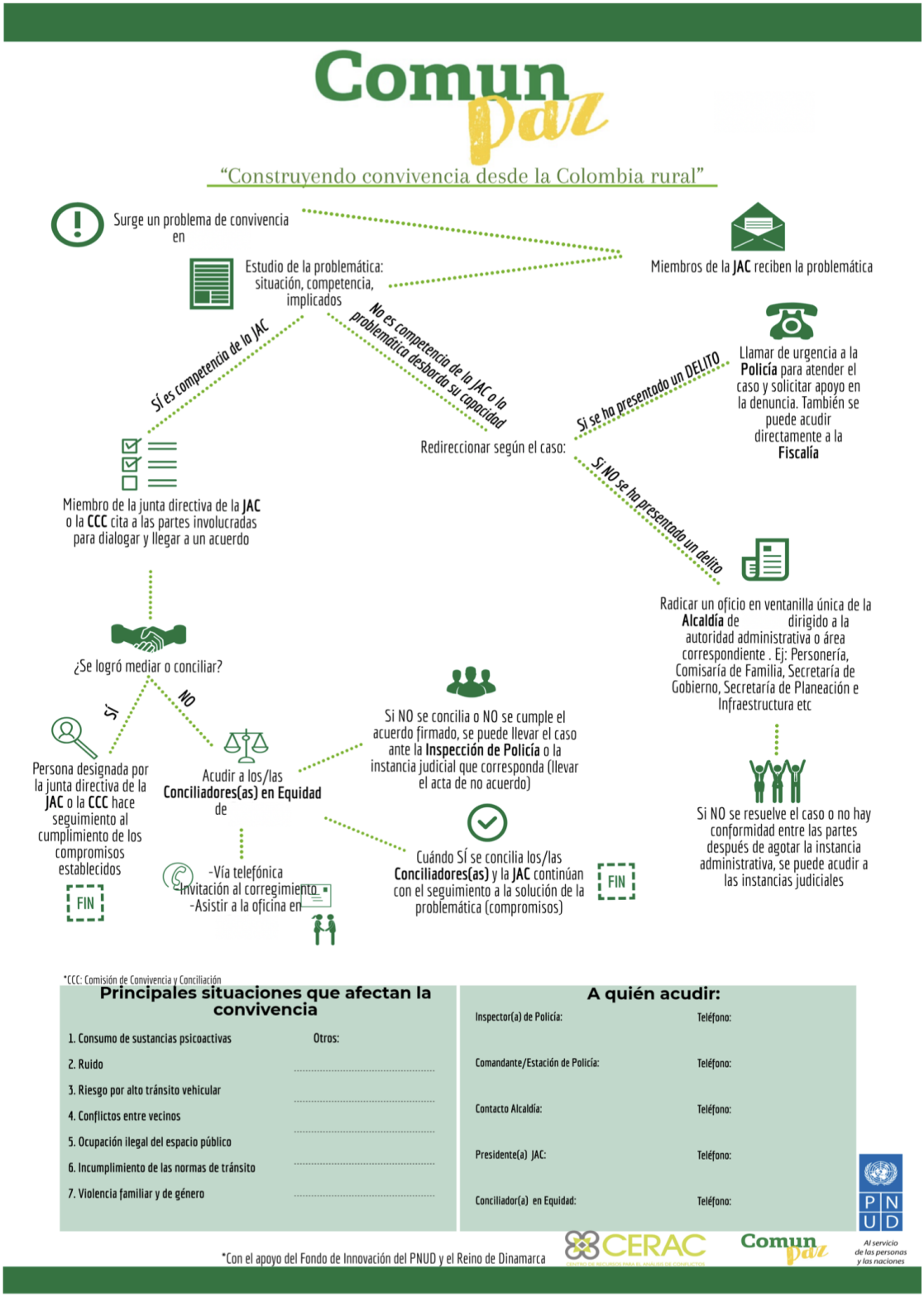
were more positive among JAC leaders, but still unfavorable. Police Inspectors and JACs have limited contact with one another, and few mechanisms for coordinating their efforts. Only 12.5% of control group JAC leaders even knew how to contact a Police Inspector at endline, and just 17% reported that Police Inspectors "actively support" the JAC's work (though one-quarter reported some cooperation between the JAC and Police Inspectors in the preceding six months).

THE *ComunPaz* PROGRAM

Following FARC's demobilization, two suboptimal outcomes loom large in rural Colombia: first, a breakdown of order, with JACs issuing unenforceable decisions that are ignored by residents, thus incentivizing vigilantism and heightening the risk of escalation; and second, the emergence of another armed actor capable of consolidating territorial control and reinforcing JACs' decisions through violence. We evaluate an inexpensive, scalable intervention designed to prevent these outcomes by inducing closer coordination between police officers, Police Inspectors, and JACs. The goal was to replace FARC's coercive capacity while avoiding the pitfalls of forcibly imposing state laws and institutions on communities long accustomed to autonomy or rebel rule. Crucially, while many treatment communities in our sample reported armed group presence, very few reported armed group dispute resolution. Our evaluation thus occurred at a pivotal transitional moment: after FARC demobilized and as rival armed groups were beginning to establish a physical presence, but before they were able to erect more sophisticated mechanisms of governance and territorial control.

The *ComunPaz* program comprises four modules involving a combination of lectures, discussions, group work, and Q&A. Each module lasts one day, for a total of four days per treatment community, implemented over the course of three months. Module 1 targets police officers and Police Inspectors; Module 2 targets JACs. Both modules aim to help participants understand the legal division of labor between state and communal authorities; identify the comparative advantages of state and communal approaches to dispute resolution; locate the most important sources of conflict in the communities under their jurisdictions; and devise mechanisms to coordinate more

Figure 1: **An anonymized response route**



effectively in the future. Module 3 targets police officers, Police Inspectors, and JACs, and aims to build trust among participants while helping them develop concrete, actionable proposals for collaborating to resolve disputes like those identified in Modules 1 and 2. Module 4 targets police officers, Police Inspectors, JACs, and citizens, and again aims to build trust among participants while disseminating information about the proposals developed in Module 3. The program culminates with the promulgation of a *ruta de atención* ("response route") for resolving disputes, as in Figure 1. Response routes are tailored to each community.

Taken together, the four modules aim to provide *information* about the roles and responsibilities of state and communal authorities under Colombian law; build *trust* between citizens and both state and communal authorities by creating opportunities for them to interact in a structured and secure environment; and improve *coordination* between state and communal authorities through the design and dissemination of "response routes." *ComunPaz* was developed by a Colombian consultant with 15 years of experience creating similar programs for various Colombian government agencies, including the police. The design was informed by a survey that we administered in 2015 to 100 *centros poblados* ("populated centers") across four regions of Colombia—Ariari-Guayabero, Oriente Antioqueño, Centro del Valle del Cauca, and Sur de Bolívar—and by focus groups that we conducted with 40 Police Inspectors in the department of Meta in July 2015. The program was implemented between October 2018 and April 2019 by the United Nations Development Programme's (UNDP) Colombia office and the Conflict Analysis Resource Center (CERAC), a Bogotá think tank. We discuss the structure of the intervention in further detail in Appendix A, and the ethics of the program and our evaluation of it in Appendix B.

RESEARCH DESIGN

SITE SELECTION

Our sample consists of 149 communities distributed across 24 municipalities in four purposefully selected regions of Colombia: Ariari-Guayabero, Oriente Antioqueño, Nordeste Antioqueño, and

Centro del Valle del Cauca.[12] Our primary site selection criterion was historical FARC presence. Areas with historical FARC presence are especially high priorities for the Colombian government, and are more likely to exhibit the institutional legacies of rebel governance that *ComunPaz* sought to leverage—in particular, strong JACs (Vargas Castillo 2019). In some regions, such as Oriente Antioqueño, the government destroyed FARC militarily; in others, such as Ariari-Guayabero, FARC withdrew following the signing of the 2016 peace agreement. The four regions also vary along a number of other dimensions, including state presence, political history, and settlement patterns.

Within each region we purposively selected five to six municipalities where we were most confident that FARC had established uncontested territorial control at some point during the civil war. We relied on a combination of primary and secondary sources to identify these municipalities, including NGO reports, academic studies, and government analyses. Importantly, municipalities that were either (a) violently contested or (b) fully controlled by an armed group were excluded due to security concerns, although we did encounter active armed group presence in at least 23 of our treatment communities, as discussed below. We also excluded municipalities where extremely poor road conditions made it prohibitively expensive to implement the intervention. Within these municipalities we sampled all *centros poblados* ("populated centers") with fewer than 5,000 residents, following the protocol described in Appendix C. We discarded populated centers with more than 5,000 residents in order to reduce heterogeneity in cluster size, and because the intervention was intentionally designed to target relatively small communities. For compactness we refer to these populated centers as "communities" throughout.

RANDOMIZATION

Treatment was randomly assigned at the community level. We stratified by region and blocked by population to mitigate the bias that can arise when average cluster size varies between the treatment and control groups (Imai, King and Nall 2009). We then randomly assigned four communities to

[12] This latter region was somewhat artificially created for purposes of our study. It consists of municipalities in Centro del Valle that fall within the area of operation of FARC's 46th Front.

treatment in each block in Oriente Antioqueño and Nordeste Antioqueño, and two communities in each block in Ariari-Guayabero and Centro del Valle del Cauca, for a total of 72 treatment communities. A detailed description of our blocking procedure is in Appendix D.1, maps of treatment and control communities are in Appendix D.3, balance tests are in Appendix D.4, and manipulation checks are in Appendix F.1. We also randomized the order in which municipalities were treated.

LIMITATIONS

Our study is not without limitations. One is scope. *ComunPaz* targeted communities that were previously governed by FARC, but where ongoing violence or armed group control would not pose a threat to facilitators' or participants' safety. We cannot be sure how our results might generalize to communities that were never controlled by armed groups in the past, or that are sites of ongoing violence or rebel governance in the present. That said, there is variation in both historical and contemporaneous rebel governance even within our sample, and despite our precautions, we documented the presence of armed groups in 23 treatment communities. Nor are the dynamics of violence and rebel governance in our sample unique to these regions, or to Colombia as a whole (Arjona 2016).

A second limitation is the "bundled" nature of the treatment. *ComunPaz* comprises four modules that were implemented in all (or at least most) treatment communities. We cannot disentangle the effects of each module in isolation from the others, and it is possible that a shorter, simpler program might have similar effects—though, given the initial wariness with which many residents greeted increased police presence in their communities, we are skeptical. A third limitation is the absence of administrative data on crime and violence at such a low level of aggregation in Colombia. We instead rely on surveys to measure the prevalence and severity of disputes, but surveys may be susceptible to non-random recall and social desirability bias. We took a number of precautions to mitigate these problems, including the use of survey experiments and costly behavioral measures. Moreover, while *ComunPaz* had the expected beneficial effect on some outcomes that are susceptible to social desirability bias (reliance on armed groups, for example), it had no

or even adverse effects on others (perceptions of JACs, for example). This suggests that social desirability alone is unlikely to explain our results.

EMPIRICAL STRATEGY

HYPOTHESES

In our PAP we hypothesized that the *ComunPaz* program would reduce the prevalence of (H1) unresolved and (H2) violent disputes. We also hypothesized that the program would decrease reliance on (H3) armed groups to resolve disputes, while increasing reliance on (H4) JACs and (H5) police officers and Police Inspectors. Our theoretical framework posits three potential mechanisms through which these effects might occur. First, we hypothesized that the program might mitigate problems of information by increasing (M1) understanding of the extent and limits of JACs' authority under Colombian law and (M2) understanding of the most important sources of disputes at the community level. Second, we hypothesized that the program might mitigate problems of trust by improving perceptions of (M4) JACs and (M5) police officers and Police Inspectors while diminishing perceptions of (M6) armed groups. Third, we hypothesized that the program might mitigate coordination problems by increasing (M6) consensus around how disputes should be resolved, (M7) the degree of coordination between JACs, police officers, and Police Inspectors, and (M8) the cohesiveness and functionality of JACs themselves.[13]

Also following our PAP, we test for heterogeneous treatment effects (HTEs) along four dimensions: (1) victimization during the civil war; (2) strength of historical rebel governance; (3) strength of historical paramilitary governance; (4) and connectedness to local and municipal political power. Because these hypotheses were exploratory, we did not specify the direction of effects. For compactness, we present HTEs for the outcomes that are most central to the program: the

[13] We present these hypotheses in a different order here than in our PAP, and we replace the distinction between "primary" and "secondary" hypotheses with a distinction between outcomes and mechanisms. In our PAP we also hypothesized that the program would increase respect for state authority in cases that fall under state jurisdiction. We relegate this hypothesis to Appendix F.4, as it proved very difficult to test in the context of our survey.

prevalence of unresolved and violent disputes, and reliance on JACs, police and officers Police Inspectors, and armed groups to resolve disputes. These results are in Appendix F.8, and are almost uniformly null.

QUANTITATIVE DATA

We test our hypotheses using multiple sources of data. First, we conducted an endline survey roughly six months after the end of the *ComunPaz* program. We surveyed 18 randomly selected residents and 8 purposively selected leaders in each community in our sample. The residents survey included an endorsement experiment designed to measure support for the police, JACs, and armed groups, and a list experiment designed to measure reliance on armed groups to resolve disputes. We also surveyed one police commander and one Police Inspector in each municipality. Because police commanders and Police Inspectors have jurisdiction over multiple communities, for most questions we gave them a list of communities in their jurisdiction and asked about each individually. The survey was implemented by Proyectamos, a Colombian NGO. Descriptive statistics and further details on our sampling frame are in Appendix E.

Second, we administered two costly behavioral measures. The first was designed to operationalize demand for coordination between police officers, Police Inspectors, and JACs. At the end of the residents survey, respondents were given a petition requesting additional involvement of municipal authorities in local dispute resolution. Residents were instructed to deliver the signed petition to the JAC president within seven days of the survey. We then called each JAC president to ask how many petitions they received, if any. The second behavioral measure was designed to operationalize actual coordination between police officers, Police Inspectors, and JACs. At the end of the leaders survey, respondents were encouraged to create a WhatsApp group to facilitate coordination with municipal authorities. To that end, they were also given the name and phone number of the police commander and Police Inspector in their jurisdiction. (We secured consent from all participating police commanders and Police Inspectors beforehand.) We then called each JAC president a week later to ask whether they had created a WhatsApp group.

QUALITATIVE DATA

We corroborate and contextualize our quantitative results with qualitative data gleaned from detailed field reports compiled by *ComunPaz* facilitators. Facilitators documented any discussions held, questions asked, and outputs produced during each activity. They also recorded information about implementation (e.g. the number of participants at each workshop), as well as their own subjective appraisals of group dynamics and individual attitudes and behaviors. Because these reports are only available for treatment communities, we cannot use them to test our hypotheses. Nonetheless, they provide valuable context and rich (if only suggestive) evidence regarding mechanisms. We describe our approach to coding the qualitative data in Appendix G.

ESTIMATION

Most of our outcomes comprise clusters of dependent variables. Following our PAP, we estimate the Average Effect Size (AES) across all dependent variables in each cluster to reduce the number of hypotheses we test and control the false discovery rate.[14] AES coefficients are interpreted in terms of standard deviations from the control group mean. For some outcomes we have just one dependent variable and use OLS instead. We include individual-level controls for age, gender, household size, educational attainment, employment status, and two proxies for socioeconomic status (quality of walls and floors). These controls were measured in the endline survey; while they are post-treatment, they either cannot (e.g. age) or are very unlikely (e.g educational attainment) to be affected by treatment assignment. We also include community-level controls for population, distance to the nearest arterial road, and distance to the departmental capital, measured using administrative data from the the Instituto Agustín Codazzi and the planning office of each municipality. We estimate the intention-to-treat effect (ITT) on all outcomes.

[14]The AES across J related dependent variables is given by $\tau = \frac{1}{J}\sum_{j=1}^{J}\frac{\pi_j}{\sigma_j}$, where π_j is the average treatment effect on each dependent variable and σ_j is the standard deviation of dependent variable j in the control group. To test the null hypothesis of no average effect, the π_j are jointly estimated in a seemingly unrelated regression framework. The J dependent variables are stacked to compute a variance-covariance matrix for testing the statistical significance of τ, the AES. For further details see Clingingsmith, Khwaja and Kremer (2009).

Dependent variables from the residents and leaders surveys are operationalized at the individual level. Dependent variables from the police and Police Inspectors surveys are operationalized at the community level. Our two behavioral measures are operationalized at the community level as well. For individual-level outcomes, we cluster our standard errors at the community level. For community-level outcomes, we omit individual-level controls and do not cluster our standard errors. Because the probability of treatment assignment varied across blocks, we weight each community by the inverse of the probability of assignment to its realized treatment status. The probability of assignment to treatment ranged from one-third to two-thirds, implying that no community received a weight more than twice that of any other. All specifications include block fixed effects as well.

POTENTIAL THREATS TO INFERENCE

SPILLOVER

Information conveyed to participants during the *ComunPaz* program may spill over from treatment to control communities through word of mouth. For most if not all of our outcomes, this would bias our ITT estimates towards the null. Disputes themselves may also spill over—for example, if residents of control communities believe their disputes will be more effectively resolved in treatment communities. Again, this would likely bias our ITT estimates towards the null. Spillover of this kind strikes us as unlikely, given that JACs typically only resolve disputes originating within their own communities. Moreover, the average treatment community in our sample is located more than 8 kilometers from the nearest control community. This is a long way in rural Colombia, where roads are rough and often impassable, and where few residents own cars.

Police Inspectors and police officers are municipal authorities, and their jurisdictions encompass both treatment and control communities. If the program improves the performance of municipal authorities generally (for example, by encouraging them to coordinate with JACs), then all communities will benefit, regardless of treatment assignment. Again, this would bias our ITT

estimates towards the null. Spillover of this kind strikes us as similarly unlikely: based on focus groups conducted with Police Inspectors before the start of the program, municipal authorities rarely travel to the communities in their jurisdictions, and are most likely to do so with the encouragement of a third party.

Finally, it is possible that participation in *ComunPaz* may cause police officers and Police Inspectors to redirect effort away from control communities and towards treatment communities. In this case, our ITT estimates would be a function not just of improved coordination between state and communal authorities in treatment communities, but also of *diminished* coordination in control communities. We view this risk as relatively minor: state institutions have limited physical presence in these remote rural areas, and the amount of effort that could plausibly be redirected is small. Floor effects are likely to mitigate the risk of negative spillover of this kind.

NON-COMPLIANCE

Participation in the *ComunPaz* program was voluntary, raising the risk of one-sided non-compliance. We took precautions to minimize this risk, including sending individually addressed letters about the program from the Colombian government's National Planning Department to municipal authorities; visiting each municipal capital to coordinate timing and logistics of the program with municipal authorities; and visiting each treatment community to coordinate timing and logistics with JACs as well. We also sent a letter from the National Planning Department to the Planning Director of the Colombia National Police, who then sent individually addressed letters authorizing the participation of police commanders in the 24 municipalities in our sample.

Even with these precautions, some non-compliance did occur. We discuss non-compliance in detail in Appendix D.2. Fortunately, there are only three treatment communities that we can classify as "full" non-compliers, because they were not treated at all. Other communities are more appropriately classified as "partial" non-compliers, since they received some (but not all) modules of the intervention. We administered the endline survey in all communities, and we include both full and partial non-compliers in the treatment group for purposes of our ITT analyses. This should

Table 1: **Prevalence of unresolved and violent disputes**

	Any unresolved disputes		**Any violent disputes**	
	(1) Residents	(2) Leaders	(3) Residents	(4) Leaders
Assigned to treatment	-0.027	-0.093**	0.001	-0.051*
	[0.033]	[0.041]	[0.010]	[0.026]
Observations	2673	1182	2673	1182
Individual controls	Yes	Yes	Yes	Yes
Community controls	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes
Estimator	OLS	OLS	OLS	OLS

Notes: All specifications include individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors, clustered by community, are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

bias our ITT estimates towards the null. As a robustness check, we computed (approximate) upper and lower bounds on the complier average causal effect (CACE) by first classifying partial non-compliers as compliers and estimating the CACE, then re-classifying them as non-compliers and re-estimating the CACE. Neither approaches yields CACE estimates that are substantively different from the ITT. For compactness, we report the ITT alone.

RESULTS

FEWER UNRESOLVED OR VIOLENT DISPUTES

Table 1 reports the ITT of the *ComunPaz* program on unresolved (columns 1 and 2) and violent (columns 3 and 4) disputes. Residents were asked if they or another household member had been involved in a dispute over any of nine issues in the past six months, including theft, public consumption of drugs or alcohol, noise complaints, improper garbage disposal, contested land boundaries, or negligent management of pets or livestock. Residents were then asked whether these disputes resulted in any physical or verbal aggression, and whether they were resolved. Leaders were asked if any member of their community had been involved in a dispute over any of the same

nine issues in the past six months, whether these disputes resulted in aggression, and whether they were resolved. We code dummies for any unresolved or violent disputes at the household (columns 1 and 3) and community (columns 2 and 4) level.[15]

We find that the program reduced the prevalence of both unresolved and (weakly) violent disputes at the community but not the household level. This discrepancy between the household- and community-level results is perhaps unsurprising, given that disputes are much more common when operationalized at the community level. Treatment group leaders were 9.1 percentage points less likely to report an unresolved dispute in their community (column 2), a reduction of 15.7% relative to the control group mean (0.577). Residents were 2.6 percentage points less likely to report an unresolved dispute involving a household member household—a statistically insignificant reduction of 7.6% relative to the control group mean (0.337). Treatment group leaders were also 5 percentage points less likely to report a violent dispute in their community (column 4). While this effect is only marginally statistically significant, it represents a substantively large 24.5% reduction relative to the control group mean (0.202). Residents in the treatment group were no more or less likely to report a violent dispute (column 3), though this may be an artifact of floor effects, as only 6.4% of control group residents reported a violent dispute in the past six months.

SUGGESTIVE EVIDENCE OF REDUCED RELIANCE ON ARMED GROUPS, BUT NO CHANGE IN RELIANCE ON STATE OR COMMUNAL AUTHORITIES

Table 2 reports the ITT on residents' (columns 1, 3, and 5) and leaders' (columns 2, 4, and 6) reliance on armed groups (columns 1 and 2), JACs (columns 3 and 4), and police officers and Police Inspectors (columns 5 and 6) to resolve disputes. We measure reliance on armed groups in four ways. First, respondents were read four hypothetical scenarios of conflict and crime, ranging in severity from a dispute over improper garbage disposal to an assault with a firearm. They were

[15] In our PAP we pre-specified that we would analyze the residents and leaders samples both separately and pooled. For compactness and to reduce the number of hypotheses we test, we omit the pooled results here. Given the differences between sampling frames and, in some cases, outcome measurement strategies, we believe the separate results are more informative.

Table 2: **Reliance on armed groups and state and communal authorities to resolve disputes**

	Reliance on armed groups		**Reliance on JACs**		**Reliance on police and PIs**	
	(1) Residents	(2) Leaders	(3) Residents	(4) Leaders	(5) Residents	(6) Leaders
Assigned to treatment	-0.056**	-0.001	-0.028	0.039	-0.028	-0.022
	[0.027]	[0.037]	[0.051]	[0.055]	[0.055]	[0.057]
Observations	2673	1182	2673	1182	2673	1182
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Community controls	Yes	Yes	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes	Yes
Estimator	AES	AES	AES	AES	AES	AES

Notes: All specifications include individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors, clustered by community, are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

then asked which authority (if any) they would report the incident to first, and which authority they believed should be responsible for providing a "final and definitive" resolution. We code a dummy for respondents who selected an armed group for any of these questions. Second, for each of the actual disputes captured in Table 1, respondents were asked if they or a household member had approached an armed group for help resolving the dispute. Residents were also asked if they or a household member had been a victim of any of seven crimes in the past six months—including burglary, robbery, assault, and extortion—and, if so, whether they had reported the crime to an armed group. We code a dummy for residents who reported any crime or dispute to an armed group in the past six months.[16]

Third, respondents were read another four hypothetical scenarios of crime and conflict and asked if they believed other community members would seek assistance from armed groups. We code a dummy for respondents who believed other community members would seek armed group assistance in any hypothetical scenario. Finally, respondents were asked if they believe other community members report actual disputes to armed groups. We code a dummy for respondents who agreed or strongly agreed that they do. By eliciting beliefs about other community members,

[16]To avoid conditioning on a post-treatment variable, we code this dummy as 0 for residents who were involved in a crime or dispute but did not report it to an armed group, and also for residents who were not involved in a crime or dispute. Our results are substantively similar if we exclude this dummy from the AES estimator.

we hoped to mitigate social desirability bias that might arise when answering questions about oneself and one's family. We measure reliance on JACs, police officers, and Police Inspectors using the first three of these four measures. We explore alternative approaches to operationalizing these outcomes in Appendix F.9.

We find that *ComunPaz* reduced reliance on armed groups among residents (column 1) but not leaders (column 2). While our measure of reliance on armed groups is potentially susceptible to social desirability bias, the negative AES in the residents survey is driven in particular by a 27.2% reduction in the belief that *other* community members would seek the help of armed groups in hypothetical scenarios, and a 48.4% reduction in the belief that *other* community members report actual disputes to armed groups. These measures are likely to be less susceptible to social desirability concerns. In Appendix F.3 we report the ITT on residents' reliance on armed groups using a list experiment. We find no evidence of reduced reliance on armed groups in the list experiment, though this analysis is underpowered and should be interpreted with caution. We also find no evidence of social desirability bias in residents' reports of reliance on armed groups in the survey, which lends some additional credence to the results in Table 2.

Importantly, reliance on armed groups was already very rare in the control group, ranging from less than 1% (the proportion of control group residents who had sought the help of an armed group to resolve an actual dispute) to 6.2% (the proportion who believed that other community members would solicit armed group assistance in hypothetical scenarios of conflict and crime) in the survey. Reliance on armed groups was no more common in the list experiment. This is informative in itself, as it suggests that rebel governance in these communities was already weak at the time of data collection. It is also perhaps unsurprising, as *ComunPaz* targeted communities where the FARC had demobilized but where rival armed groups had not yet established territorial control.

Given the rarity of the dependent variable, we interpret this result somewhat cautiously, as its statistical significance will inevitably be sensitive to relatively minor changes in the data. Nonetheless, in Table 2 we do find some suggestive evidence that the program reduced already

Table 3: **Information about communities and Colombian law**

	Understanding of JACs' authority		**Understanding of most important disputes**		
	(1) Residents	(2) Leaders	(3) Leaders	(4) Police	(5) PIs
Assigned to treatment	0.015	0.014	-0.026	0.007	0.040
	[0.034]	[0.031]	[0.039]	[0.041]	[0.041]
Observations	2620	1171	1182	149	149
Individual controls	Yes	Yes	Yes	No	No
Community controls	Yes	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes
Estimator	AES	AES	OLS	OLS	OLS

Notes: Specifications in columns 1-3 include individual- and community-level controls, block fixed effects, and inverse probability weights. Columns 4 and 5 exclude individual-level controls. Standard errors are in brackets, and are clustered by community in columns 1-3. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

low levels of reliance on armed groups nearly to zero, potential floor effects notwithstanding. Conversely and more surprisingly, we find no evidence that the program increased reliance on JACs, police officers, or Police Inspectors among either residents or leaders (columns 3–6). We return to this result in the discussion.

NO CHANGE IN INFORMATION ABOUT COMMUNITIES OR COLOMBIAN LAW

Table 3 reports the ITT on residents' (column 1) and leaders' (column 2) understanding of the extent and limits of JACs' authority under Colombian law, as well as the ITT on leaders' (column 3), police commanders' (column 4), and Police Inspectors' (column 5) understanding of the most serious sources of disputes in the communities under their jurisdictions. To measure understanding of JACs' authority, both residents and leaders were asked the same seven factual questions about the classes of cases that JACs are and are not authorized to adjudicate under Colombian law, ranging in severity from noise complaints to possession of a firearm to domestic violence. We code dummies for correct answers to each of these questions.

To measure understanding of the most serious sources of disputes, residents were first asked which of 12 potential sources of disputes they believed to be most important in their community.

We take the mode across the 12 randomly selected residents of each community. Leaders were asked the same question about the most serious sources of disputes in their community; police commanders and Police Inspectors were asked the same question about each treatment and control community under their jurisdiction. We code dummies indicating whether leaders', police commanders', and Police Inspectors' responses to this question match the modal response among residents.[17] This is in some respects a hard test, since leaders, police commanders, and Police Inspectors may be aware of residents' assessments of the most serious sources of disputes in their communities, but may disagree with those assessments. Our survey does not capture these nuances.

We find no evidence that the program increased understanding of JACs' authority under Colombian law. The AES for both residents (column 1) and leaders (column 2) is small and not statistically significant at conventional levels. The ITT is similarly small and statistically insignificant for all but two of the component dependent variables in the residents sample,[18] and for all component dependent variables in the leaders sample. Nor do we find evidence that the program increased understanding of the most serious sources of disputes among leaders (column 3), police commanders (column 4), or Police Inspectors (column 5). Importantly, there is much agreement among residents and leaders about the sources of disputes in their communities, though the extent of this agreement is no higher in the treatment group than in the control group.[19]

[17] In some communities there are multiple modes. In these cases, we code a dummy indicating whether leaders,' police commanders', and Police Inspectors' responses match any of the modal responses among residents.

[18] Treatment group residents were 4.8 percentage points more likely to know that JACs are not authorized to adjudicate cases of domestic violence, but they were also 6.3 percentage points less likely to know that JACs *are* authorized to resolve cases of improper garbage disposal.

[19] Across communities, the first and second most serious sources of disputes among residents were improper garbage disposal and public drug consumption. Leaders similarly identified improper garbage disposal and public drug consumption as the first and second most serious sources of disputes across communities, but their responses were no more likely to match the modal resident response in the treatment group than in the control group. Police commanders identified noise complaints, public drug consumption, and rumors as the most serious sources of disputes; Police Inspectors identified contested land boundaries and noise complaints as the most important. Interestingly, a large plurality of Police Inspectors (44.8%) identified contested land boundaries as the most serious source of disputes, even though only a small minority of residents (3.25%) agreed with this assessment.

Table 4: **Perceptions of armed groups and state and communal authorities**

	Perceptions of armed groups		**Perceptions of JACs**			**Perceptions of police and PIs**	
	(1) Residents	(2) Leaders	(3) Residents	(4) Police	(5) PIs	(6) Residents	(7) Leaders
Assigned to treatment	-0.083**	-0.074	-0.023	0.012	0.187**	0.105*	0.068
	[0.037]	[0.046]	[0.062]	[0.074]	[0.085]	[0.064]	[0.066]
Observations	2673	1182	2673	149	149	2673	1182
Individual controls	Yes	Yes	Yes	No	No	Yes	Yes
Community controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Estimator	AES	AES	AES	OLS	OLS	AES	AES

Notes: Specifications in columns 1-3, 6, and 7 include individual- and community-level controls, block fixed effects, and inverse probability weights. Columns 4 and 5 exclude individual-level controls. Standard errors are in brackets, and are clustered by community in columns 1-3, 6, and 7. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

LESS FAVORABLE PERCEPTIONS OF ARMED GROUPS, MORE FAVORABLE PERCEPTIONS OF STATE AUTHORITIES

Table 4 reports the ITT on perceptions of armed groups (columns 1 and 2), JACs (columns 3-5), and police officers and Police Inspectors (columns 6 and 7). To measure perceptions of armed groups, both residents and leaders were asked whether they think other members of their community believe that armed groups resolve disputes fairly and effectively. We code a dummy for respondents who agreed or strongly agreed with this statement. Respondents were also asked whether they themselves believe that armed groups resolve disputes fairly and effectively, whether they trust armed groups, and whether they believe armed groups know and understand the problems afflicting their communities. We code dummies for respondents who answered somewhat, quite a bit, or a lot to each of these questions. Respondents were asked these latter three questions about JACs, police officers, and Police Inspectors as well.

We measure perceptions of JACs among police commanders and Police Inspectors by asking whether they trust the JAC of each community under their jurisdiction. We code dummies for respondents who answered that they did.[20] This latter analysis was not pre-specified, as we

[20]These questions were scaled from 1 to 5. Unlike the questions used to measure perceptions of armed groups, we did

did not anticipate including questions about perceptions of JACs in our survey of police commanders and Police Inspectors. The analysis does, however, follow immediately from our theoretical framework. Importantly, in some cases police commanders and Police Inspectors reported having no contact with one or more of the JACs under their jurisdiction. To avoid conditioning on a post-treatment variable, we code these cases as 0s on our measure of perceptions.[21]

We find that *ComunPaz* diminished residents' perceptions of armed groups (column 1). The negative and statistically significant AES among residents is driven in particular by a 1.6 percentage point (47.1%) reduction in trust in armed groups, a 1.7 percentage point (45.2%) reduction in the perception that armed groups resolve disputes fairly and effectively, and a 6.9 percentage point (38%) reduction in the belief that other community members perceive armed groups as fair and effective. The program diminished leaders' perceptions of armed groups as well (column 2), though this effect falls just short of (weak) statistical significance at conventional levels ($p = 0.103$). It is, however, similar in magnitude to the effect among residents, and is driven in particular by a substantively large and statistically significant 3.9 percentage point (77.3%) reduction in trust in armed groups.

Importantly, perceptions of armed groups were already quite unfavorable among control group respondents. Only 3.4% of residents and 3.9% of leaders in the control group expressed trust in armed groups; only 3.9% of residents and 4.3% of leaders expressed a belief that armed groups understand the problems afflicting their communities; only 3.8% of residents and 3.7% of leaders expressed a belief that armed groups resolve disputes fairly and effectively; and only 6.9% of residents and 4.1% of leaders thought that other community members shared this belief. Again, this is important in itself, as it suggests that rebel governance was relatively unpopular in these communities. In some cases the program appears to have reduced support for armed groups to 0 along these dimensions, potential floor effects notwithstanding.

not specify what each number on the scale was intended to mean. To avoid ambiguity, we code a dummy for police commanders and Police Inspectors who answered 4 or 5. Our results are substantively similar if we include those who answered 3.

[21] In this sense, we implicitly assume that police commanders and Police Inspectors cannot trust JACs with which they have never interacted. Restricting our sample to JACs that have interacted with police commanders and Police Inspectors would likely induce bias, since these interactions are at least partly a function of treatment assignment.

We also find that the program (weakly) enhanced perceptions of police officers and Police Inspectors in the residents survey (column 6). The positive AES is driven by an improvement in perceptions of Police Inspectors specifically. Treatment group residents were 6.1 percentage points (16.9%) more likely to express trust in Police Inspectors, and 6.9 percentage points (19.2%) more likely to believe Police Inspectors resolve disputes fairly and effectively. Perceptions of police officers were more favorable among treatment group residents as well, but not statistically significantly so. The AES on perceptions of police officers and Police Inspectors is positive among leaders (column 7), but is only half the magnitude of the AES among residents, and is imprecisely estimated. This positive but statistically insignificant AES is driven in particular by a 7.1 percentage point (17.4%) increase in leaders' belief that Police Inspectors resolve disputes fairly and effectively.

We find no evidence that the program improved perceptions of JACs among either residents (column 3) or police commanders (column 4). The program did, however, improve perceptions of JACs among Police Inspectors, who were 19.1 percentage points more likely to express trust in treatment group JACs—a substantively large and statistically significant 65.7% increase over the mean for control group JACs (0.291). A possible explanation for the discrepancy between the null effect on trust in JACs among police commanders and the positive effect among Police Inspectors is that the latter generally have much more contact with rural Colombian communities than the former. Police Inspectors are also more likely to cooperate with JACs to resolve the sorts of localized, quotidian disputes ("*problemas de convivencia*") that typically arise in these communities. This may also help explain why we observe a more pronounced change in perceptions of Police Inspectors than in perceptions of police officers among leaders and especially residents.

Table 5 reports the effect of the program on perceptions of armed groups, JACs, and the police using an endorsement experiment. Residents were asked to consider three policies that might be adopted in their communities: a ban on the public consumption of alcohol; a mandate that motorcycle taxi drivers and their passengers wear helmets while riding; and a requirement that pack animals ("*animales de carga*") wait at the entrance to the community in order to avoid

Table 5: **Perceptions of armed groups and governmental and communal institutions among residents using endorsement experiment**

	Index of approval
Assigned to treatment	-0.136
	[0.085]
JAC endorsement	-0.090
	[0.061]
Police endorsement	-0.094
	[0.061]
Armed group endorsement	-0.965***
	[0.133]
Assigned to treatment × JAC endorsement	0.166*
	[0.087]
Assigned to treatment × police endorsement	0.212***
	[0.080]
Assigned to treatment × armed group endorsement	0.057
	[0.183]
Observations	2673
Individual controls	Yes
Community controls	Yes
Block FE	Yes
Weights	Yes
Estimator	OLS

Notes: Specification includes individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors, clustered by community, are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

the accumulation of excrement and other animal waste in the streets. The three policies were associated with one of three actors at random: armed groups, the police,[22] or JACs. Endorsement was randomly assigned at the respondent level, meaning that each respondent was assigned to hear an endorsement from the same actor across all three policies. A control group was asked to consider the same three policies without an endorsement. Respondents were then asked how much they would support each policy. We aggregate their responses into a standardized additive index. We estimate the effect of *ComunPaz* by interacting assignment to the program with assignment to each of the three endorsements.

Consistent with our results in Table 4, we find that *ComunPaz* increased support for policies associated with the police. It also (weakly) increased support for policies associated with JACs. We find no evidence that the program decreased support for policies associated with armed groups, though this may be an artifact of floor effects. Support for all three policies was very high even without an endorsement, with a large plurality of control group respondents (43.6%) expressing the highest possible level of support across the three policies. Similarly large pluralities expressed the highest possible level of support when the policies were associated either with the JAC (38.1%) or with the police (42.9%). In contrast, a smaller but still sizable plurality of control group respondents (22.7%) expressed the *lowest* possible level of support when the policies were associated with armed groups. Taken together, these patterns account for the substantively large and highly statistically significant negative effect of the armed group endorsement in the control group—an effect that is between 12 and 17 times larger than the effect of the JAC or police endorsement. This is consistent with low levels of support for armed groups that we observe among residents more generally, and may help explain the null when we interact assignment to *ComunPaz* with the armed group endorsement.

[22] For reasons of statistical power we did not distinguish between endorsement by the police and endorsement by Police Inspectors.

Table 6: **Coordination between and among governmental and communal institutions**

	Consensus around dispute resolution		**Coordination between JACs, police, and PIs**			**Cohesiveness of JACs**
	(1) Residents	(2) Leaders	(3) Leaders	(4) Police	(5) PIs	(6) Leaders
Assigned to treatment	-0.049	-0.040	0.093*	0.028	0.249**	0.153**
	[0.035]	[0.030]	[0.056]	[0.129]	[0.114]	[0.062]
Observations	2673	1182	1168	149	149	1135
Individual controls	Yes	Yes	Yes	No	No	Yes
Community controls	Yes	Yes	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes	Yes
Estimator	AES	AES	AES	AES	AES	AES

Notes: Specifications in columns 1-3 and 6 include individual- and community-level controls, block fixed effects, and inverse probability weights. Columns 4 and 5 exclude individual-level controls. Standard errors are in brackets, and are clustered by community in columns 1-3 and 6. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

More coordination between and within governmental and communal institutions

Table 6 reports the ITT on the degree of consensus around methods and mechanisms of dispute resolution (columns 1 and 2), the degree of coordination between JACs, police officers, and Police Inspectors (columns 3-5), and the degree of coordination *within* JACs themselves (column 6)—in particular, their cohesiveness and functionality. To measure consensus around dispute resolution, we leverage the same four hypothetical scenarios of conflict and crime that we used to measure reliance on armed groups, JACs, police officers, and Police Inspectors. Recall that for each hypothetical scenario, respondents were asked which authority they would report to first, and which they believed should provide a "final and definitive" resolution. We take the modal response to each of these questions within each community, then code a dummy indicating whether each individual response matches the communal mode.

We measure coordination separately for leaders, police commanders, and Police Inspectors. Leaders were asked whether they had the phone number of a police commander or Police Inspector stored on their phone; whether they believed that coordination between JACs, police officers, and Police Inspectors improves the quality of dispute resolution; whether they believed that police

officers and Police Inspectors were "actively" supporting the JAC in their community; and whether the JAC in their community had contacted a police officer or Police Inspector for help with dispute resolution in the past six months. We code dummies for affirmative answers to each of these questions.

Police commanders and Police Inspectors were similarly asked if they had the phone number of a JAC member from each treatment and control community stored on their phones; whether the JAC from each treatment and control community had contacted them for help with dispute resolution in the past six months; and whether they had visited each treatment and control community under their jurisdiction in the past six months. We code dummies for affirmative answers to each of these questions. Finally, to measure the cohesiveness and functionality of JACs, leaders were asked whether they believe there exists an "atmosphere of trust" among JAC members; whether JAC meetings devolve into fights and disagreements; and whether the JAC meets at least monthly. We code Likert scales ranging from 1 to 5 for the first two questions. We code a dummy for affirmative answers to the third.

We find no evidence that the program increased consensus around methods and mechanisms of dispute resolution among residents (column 1) or leaders (column 2). We find some evidence that the program increased coordination between state and communal authorities according to leaders (column 3), though this result is only weakly statistically significant and is sensitive to specification.[23] While we find no evidence that the program increased coordination with police officers (column 4), it did increase coordination with Police Inspectors (column 5). This is consistent with our results in Table 4, and again suggests that the program was most effective in improving relations and encouraging coordination with Police Inspectors rather than police officers, perhaps because the former have more contact with communities than the latter. We find that the program improved coordination within JACs as well (column 6), in particular by inculcating an atmosphere

[23] In our PAP we pre-specified that leaders would also be asked whether they knew the name of a police officer or Police Inspector. Because participants in the *ComunPaz* program exchanged names during the workshops, we believe this is more appropriately interpreted as a manipulation check. Interestingly, we find that treatment group leaders were no more likely to know the name of a police officer or Police Inspector than control group leaders, perhaps because the survey was administered six months after the end of the program. When we include knowledge of names as proxies for coordination, the AES is positive but no longer statistically significant at conventional levels (p =0.184).

Table 7: **Demand for coordination between state and communal authorities using behavioral measures**

	Any petitions	**# of petitions**	**WhatsApp group**
	(1)	(2)	(3)
Assigned to treatment	-0.178**	-1.139	-0.038
	[0.089]	[0.725]	[0.074]
Observations	117	117	117
Individual controls	No	No	No
Community controls	Yes	Yes	Yes
Block FE	Yes	Yes	Yes
Weights	Yes	Yes	Yes
Estimator	OLS	OLS	OLS

Notes: All specifications include community-level controls, block fixed effects, and inverse probability weights. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

of trust among JAC members.

Finally, Table 7 reports the ITT on our costly behavioral measures of demand for (columns 1 and 2) and actual coordination among (column 3) police, Police Inspectors, and JACs. We measure demand for coordination using the number of petitions signed in each community (column 2). Because the distribution of this variable is highly skewed,[24] we also code a dummy for any petitions signed (column 1). We measure actual coordination with a dummy indicating whether leaders reported forming a WhatsApp group with police officers or Police Inspectors in their jurisdiction (column 3).

All three of these measures suffer from missing data. When we attempted to contact JAC leaders a week after the survey was complete, we found that many were unreachable, despite multiple attempts. As a result, we are missing data on petitions and WhatsApp groups for 32 of the 149 communities in our sample. Fortunately, as we show in Appendix F.10, missingness is not statistically significantly correlated with treatment,[25] or with any of our community-level controls. This should mitigate (but not eliminate) concerns about bias due to non-random missingness. With this caveat in mind, we find that if anything, residents of treatment communities were *less* likely to

[24]The number of petitions signed ranges from a low of 0 (36.8% of communities) to a high of 18 (1.7% of communities). In the latter communities, literally every single resident respondent signed a petition.

[25]We are missing data on petitions and WhatsApp groups for 19.4% of treatment communities and 23.4% of control communities. These proportions are not statistically different from each other, as we show in Appendix F.10.

sign petitions requesting greater coordination between state and communal authorities (columns 1 and 2). Treatment group JACs were 17.8 percentage points less likely than control group JACs to receive any petitions, and they received 1.25 fewer petitions overall. These represent substantively large reductions of 24.9% and 28.8%, respectively, relative to the control group means (72.3% and 4.33).

One possible explanation for this finding is that there was high demand for greater coordination between the state and communal institutions in all communities, and that *ComunPaz* helped meet this demand in treatment communities only. Consistent with this interpretation, 63.2% of communities overall (and 71.1% of communities in the control group) received at least one petition, with a mean of 3.62 petitions per community. If the intervention helped satisfy existing demand by improving coordination between state and communal authorities—as our results in Table 6 suggest that it did—then it is unsurprising that demand was lower in the treatment group after the intervention was complete. Fewer communities (20.5%) established WhatsApp groups, and we find no evidence that treatment communities established them at a higher (or lower) rate than control communities (column 3).

DISCUSSION

Our results suggest that the *ComunPaz* program reduced the prevalence of unresolved and violent disputes at the community level; diminished residents' already limited reliance on armed groups, and their already unfavorable perceptions of armed groups; increased the cohesiveness and functionality of communal institutions; strengthened mechanisms of coordination between communal institutions and their state counterparts; and improved residents' perceptions of state authorities, especially Police Inspectors, who tend to have closer and more continuous contact with rural residents than other municipal actors. These results are consistent with our theoretical framework, and our PAP.

Contrary to our expectations, however, we find no evidence that the program improved per-

ceptions of communal institutions, or that it increased understanding of the extent and limits of communal institutions' authority under Colombian law. We similarly find no evidence that the program improved awareness of the most serious disputes within communities, or that it fostered consensus around how those disputes should be resolved. Perhaps most surprising, we find no evidence that the program induced greater reliance on either state or communal authorities when crimes are committed or disputes arise. This is especially surprising given our finding that the program reduced the prevalence of unresolved or violent disputes, and also decreased reliance on armed groups. If the program decreased reliance on armed groups but did not increase reliance on either state or communal authorities, then the reduction in unresolved or violent disputes is a puzzle. What explains this apparently paradoxical combination of results?

We cannot answer this question definitively. Nonetheless, our qualitative data suggests at least two potential explanations. First, the program may have increased participants' willingness and ability to resolve conflicts themselves, without recourse to a third party. Our qualitative data indicates that direct dialogue between residents was an important first step in dispute resolution in nearly one-third (20) of all treatment communities. The qualitative data also suggests that in at least 11 treatment communities, *ComunPaz* facilitators helped residents resolve conflicts "on the spot," during implementation of the intervention itself. In one village in the Ariari region, for example, *ComunPaz* facilitators helped residents devise a new communal garbage collection system to address complaints about accumulated waste in the streets. If residents resolved conflicts through direct dialogue or with the help of program facilitators, this might explain why we observe a decrease in unresolved or violent disputes without a corresponding increase in reliance on either state or communal authorities.

Second, it is possible that the program created response routes that were so complex that they did not lend themselves to reliance on any particular authority. From our qualitative data, the modal response route in treatment communities involved three different authorities, with a minimum of two and a maximum of six. The modal response route also involved seven "steps," with a minimum of three and a maximum of 14: each step specified a different mechanism that residents could use

to resolve their disputes. Our quantitative data captures some but not all of these mechanisms. For example, trained mediators known as *conciliadores en equidad* and municipal federations of JACs known as ASOJUNTAS appear to play important roles in dispute resolution in at least 18 treatment communities, but we did not measure reliance on either of these actors in the survey. This might explain why the reduced prevalence in unresolved or violent disputes was not accompanied by increased reliance on either state or communal authorities. This might also explain why we do not observe greater consensus around how disputes should be resolved, or improved understanding of the legal limits of JACs' authority. If response routes were complicated and involved a variety of authorities, then some confusion around these legal and procedural questions is not surprising. Crucially, for all their complexity, *ComunPaz* response routes never included rebel groups.

These two explanations are not mutually exclusive, and they are difficult to disentangle empirically. There is, however, an additional observable implication of the first explanation that we can test with our survey. If residents of treatment communities resolved disputes through direct dialogue or mediation by program facilitators, such that JAC leaders never became aware of these disputes in the first place, then we should observe a reduction not only in the prevalence of *unresolved* disputes at the community level, but also in the prevalence of *all* disputes. In Table A.15 we explore this possibility by testing the ITT of the program on dispute prevalence at the household (column 1) and community (column 2) levels, regardless of whether those disputes were resolved, and regardless of whether they resulted in violence.

These analyses were not pre-specified, and should be interpreted as exploratory. But the results are nonetheless revealing. Leaders in the treatment group were 9.7 percentage points less likely to report a dispute in their community in the past six months (column 2), a statistically significant reduction of 13.5% relative to the control group mean (0.719). Treatment group residents were also 3.6 percentage points less likely to report a dispute involving members of their own household (column 1), an imprecisely estimated reduction of 9.2% relative to the control group mean (0.393). This discrepancy between the household- and community-level results is again unsurprising, given that disputes were almost twice as common when measured at the community

level. The discrepancy is also consistent with the first explanation described above: if residents resolved disputes without the help of JAC leaders, then they should have reported the existence of disputes that JAC leaders simply were not aware of.

The results in Table A.15 suggest that *ComunPaz* may have decreased reliance on armed groups not by inducing a substitution towards state or communal authorities, but rather by diminishing the "stock" of disputes available for armed groups to resolve. Substitution effects may have been especially unlikely given the sheer complexity of the response routes that program participants devised. *ComunPaz* response routes involve complicated networks of state and communal authorities with overlapping roles and responsibilities. The program seems not to have increased reliance on (or knowledge of) any one authority, even as it improved coordination between them. But the program does appear to have relegated armed groups to the margins of these networks, helping to prevent the restoration of armed group governance in areas previously subject to rebel control.

CONCLUSION

Armed groups often create or coopt institutions to maintain local stability during periods of national upheaval. These institutions may continue to function even after armed groups demobilize, but without the coercive capacity necessary to enforce potentially unpopular decisions. The resulting governance gaps heighten the risk that local conflicts will escalate. They also create opportunities for dissidents, splinter groups, and other armed actors to supplant their newly demobilized rivals by establishing modes of governance that satisfy citizens' demand for local order. We argue that states can lower the risk of local conflict escalation and impede the restoration of rebel or criminal group rule by offering fair, efficient dispute resolution services of their own. They can do this by exploiting complementarities with communal institutions, including, paradoxically, those that once served as (willing or unwilling) appendages of armed group governance. We argue that these complementarities make it possible to strengthen state and communal institutions simultaneously,

thus expediting both statebuilding and peacebuilding processes.

We test our argument through an experimental evaluation of the *ComunPaz* program in Colombia. Co-production between the state and communal institutions is often hindered by incomplete information, distrust, and coordination problems. *ComunPaz* was designed to overcome these obstacles. Using a combination of surveys, survey experiments, and costly behavioral measures, we find that the program reduced the prevalence of unresolved and violent disputes at the community level, increased citizens' trust in (some) state authorities, and strengthened coordination between the state and communal institutions. We also find suggestive evidence that the program reduced citizens' trust in, and reliance on, armed groups. This is especially striking given that the intervention was short (four modules of activities lasting a total of approximately 10 hours per community) and inexpensive (approximately $1,000 USD per community). We find no evidence, however, that the program increased reliance on either state or communal authorities to resolve disputes, or that it increased citizens' trust in communal institutions.

How durable are these effects likely to be? Potential shocks to the environment in which the program was implemented, including the resurgence of violence between armed groups and the state (or among armed groups themselves), may reverse some of the program's gains. Nonetheless, nearly six months passed between the end of the intervention and endline data collection. This, combined with the Colombian government's commitment to continue expanding into territories previously governed by armed groups, suggests the possibility of more lasting change. We argue that this expansion is more likely to succeed if state institutions continue leveraging complementarities with their communal counterparts. *ComunPaz* offers one potential model for the Colombian government—and other governments—to follow.

Accommodating and empowering communal institutions is especially important given the normative implications of statebuilding in areas previously (or currently) governed by armed groups. In these settings, there is often a trade-off between enabling the state to provide services tolerably well on the one hand, and allowing armed groups to provide the same services more effectively on the other. Extending the reach of the state is normatively desirable if it improves the welfare of

citizens. If states displace rebels but neglect to fill the resulting governance gap, they may cause inadvertent but potentially serious harm to already vulnerable populations. Moreover, any effort to alter the norms and practices of dispute resolution at the local level is likely to have significant distributional consequences (Hartman, Blair and Blattman 2019). We argue that statebuilding is more likely to improve citizens' welfare if state authorities complement and incorporate their communal counterparts, rather than marginalizing or eradicating them altogether.

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APPENDIX

A INTERVENTION

The *ComunPaz* program was implemented in four modules over the course of three months, with one week between Modules 1 and 2, seven weeks between Modules 2 and 3, and four weeks between Modules 3 and 4. Implementation occurred between October 2018 to May 2019. The first module consisted of a one-day workshop targeting police commanders and Police Inspectors, with one workshop per region. Facilitators began by eliciting participants' general perceptions of the roles and responsibilities of police and Police Inspectors in Colombia, as well as their specific experiences in the field. They then divided participants into groups and provided them with flashcards listing the range of functions assigned to both governmental and communal authorities under Colombian law. Participants affixed these flashcards to posters hung on the wall, each with the name of one authority at the top (e.g. JACs, Police Inspectors, police officers, etc.).

This exercise was followed by a discussion in which facilitators helped participants correct mistakes and misperceptions, and more generally reflect on the division of labor between governmental and communal institutions in Colombia. The workshop concluded with an exercise in which participants first listed the most common conflicts they are asked to adjudicate in their municipalities, then mapped the steps they typically take to respond to those conflicts. This was followed by a discussion in which facilitators helped participants identify procedures that deviate from Colombian law and consider ways to integrate JACs into previously established dispute resolution routines.

The second module targeted JACs, with one workshop per treatment community. The workshop began with an exercise in which participants were asked to identify the most frequent conflicts in their communities, the methods they most often use to resolve those conflicts, and the challenges they typically face in reaching and enforcing a resolution. Participants were then given flash cards listing a variety of potential mechanisms for dispute resolution under Colombian law (including referral to Police Inspectors or the police), then asked to construct a complete "response route" ("*ruta de atención*") for specific hypothetical conflicts that might arise in their communities. This was

followed by a discussion in which facilitators helped participants identify discrepancies between legally prescribed response routes and those more commonly used in practice. The workshop concluded with an exercise encouraging participants to brainstorm ways to improve the services they provide to their communities, including through collaboration with governmental authorities.

Drawing on the challenges and opportunities identified by governmental and communal authorities in the previous two modules, the third module aimed to increase the degree of coordination between them. The module consisted of a one-day workshop in which JAC leaders from treatment communities were invited to the municipal capital to meet with Police Inspectors, police commanders, and other municipal authorities. Facilitators brought to the workshop the information they had gathered in Modules 1 and 2 on the conflicts, challenges, and response routes that are most prevalent in treatment communities. Facilitators then used this information to help participants generate concrete proposals for collaborating more directly with one another, again within the bounds of Colombian law.

Importantly, facilitators allowed these proposals to vary across municipalities and communities. In this sense, the intervention did not assume the existence of a single "correct" way to induce cooperation among governmental and communal institutions. Instead, it assumed that an effective, sustainable division of labor requires leveraging local knowledge and tailoring response routes to conditions and capacities on the ground, which may vary from one community to the next. For example, a JAC with a large, experienced and highly effective conflict resolution committee may only require police assistance in the most severe cases of crime and violence, thereby preserving the JAC's autonomy while still abiding by restrictions imposed under Colombian law. In contrast, a weaker, smaller JAC may benefit from more regular police presence. Module 3 allowed this variation to guide the agreements that participants reach.

Finally, Module 4 took the agreements reached in Module 3 back to communities themselves. The goal was to inform the population about the newly agreed upon procedures for conflict resolution and further cement the nascent communication channels and cooperative relationships between JACs and Police Inspectors. (To simplify logistics, police officers were not included in

Module 4.) The module included both formal and pedagogical components, including a facilitated discussion with residents designed to help them understand the contents of the new agreements, as well as a "signing ceremony" at which the agreements were officially validated by the two sets of participating authorities. Module 4 thus aimed to foster a convergence of expectations among citizens about how different conflicts would be resolved in the future, while increasing buy-in for the new response routes by explaining in a transparent way how they were developed. An example of the publicly-posted response routes established for each community—which also included contact information for governmental and communal authorities—can be found in Figure A.1.

Following the conclusion of the program, facilitators followed up with Police Inspectors and JAC leaders multiple times in person and by phone to reinforce the messages of the workshop and encourage further coordination. During Modules 3 and 4, police officers, Police Inspectors, and JACs made a series of commitments to one another. The nature of these commitments varied across communities, but generally involved pledges of greater communication and coordination, often in relation to specific issues or disputes. *ComunPaz* facilitators made three rounds of follow-up phone calls to encourage participants to adhere to these commitments, and to measure the extent of their adherence. Phone calls were made between September and November 2019, roughly five to seven months after the end of the intervention.

B ETHICS

One of our primary concerns in implementing and evaluating the *ComunPaz* program was to avoid increasing security risks for communities, participants, and facilitators. The demobilization of an armed group rarely results in the immediate reestablishment of the state's monopoly of violence. More often, the means to produce violence and control territory remain divided among the state, preexisting armed actors, and emerging splinter groups. Competition among these groups exacerbates the risk of local violence during transitions to national peace. Although the *ComunPaz* program targeted post-conflict areas, armed groups continue to operate in rural regions of Colom-

bia. Mitigating the risks associated with armed group presence was a key ethical concern when designing and implementing the program.

We sought to mitigate four risks in particular: first, that armed groups would perceive communal authorities as "informants" due to their increased contact and coordination with governmental institutions (especially the police); second, that armed groups would surveil, attack, or kidnap governmental authorities as they traveled to and from communities in order to participate in the program, as governmental authorities rarely have a presence in all communities in these regions; third, that armed groups would perceive the program itself as a threat to their territorial control, and would retaliate against facilitators or participants; and fourth, that armed groups would misidentify facilitators as collaborators or even spies for the government. Addressing these risks is especially important given ongoing armed group aggression against civil society leaders and organizations in rural Colombia.

In consultation with our partners, CERAC and UNDP, we considered these risks carefully when designing and implementing the program, and took a variety of precautions to mitigate or eliminate them. Most important, we excluded from our sampling frame municipalities and communities with active armed group presence at the time the program began. FARC had recently demobilized from most of the regions in our sample,[1] creating a window of opportunity to implement the program and fill the resulting governance gap before competing armed groups either emerged or established a foothold. To assess the extent of armed group presence, we consulted the Colombian government's National Planning Department, the Colombian National Police, and other experts, including the facilitators themselves. We also sought the advice of Proyectamos, a local NGO that assisted us with endline data collection. Finally, we monitored regional and national media for reports of armed group activity. *ComunPaz* facilitators then travelled to the capital of each of the selected municipalities to collect more detailed, localized information from municipal authorities about the security situation at the community level. Communities that were found to have active armed group presence were removed from the sample.

[1]The exception was Eastern Antioquia, where FARC was defeated militarily around 2007–2008.

Excluding communities with active armed group presence was necessary but not sufficient to guarantee the safety of facilitators and participants. The regions in our sample are characterized by fluid and rapidly shifting security conditions. Communities themselves are the most reliable source of information on these conditions, and UNDP and CERAC empowered communal authorities to decide whether, when, and how the program would be implemented in their communities. For example, at the request of JAC members, Police Inspectors were excluded from Modules 3 and 4 in seven communities, and police officers were excluded from Modules 3 and 4 in another seven communities. Governmental authorities exercised similar discretion over whether, when, and how they would participate in the program. For example, in five municipalities – encompassing 17 communities – Police Inspectors did not participate in Module 1.

Facilitators were explicitly instructed not to try to persuade either communal or governmental authorities to participate in the program, or to travel themselves if they expressed any security concerns. In this way we sought to leverage nuanced local understandings of local security conditions to complement and validate our own extensive vetting through primary and secondary sources. Overall, 23 treatment communities reported some form of armed group presence over the course of the program. Nonetheless, extensive consultation with governmental and communal authorities seems to have successfully mitigated any resulting threats, as none of the actors involved in the program reported any kind of security risk during program implementation.

A secondary ethical concern was to avoid creating or legitimizing local-level dispute resolution practices that might be at odds with Colombian law, or otherwise inconsistent with liberal rights and values. This concern emerged due to the implementers' goal of developing locally-tailored "response routes," which we expected would be more effective, but which we also worried might incorporate illiberal or illegal mechanisms for resolving disputes. We were especially concerned that JACs might attempt to negotiate "informal" solutions to criminal complaints that should be adjudicated within the formal justice system, in particular regarding gender-based violence. To address this concern, the program featured a number of activities aimed at clearly communicating the legal limits on dispute resolution by JACs and other communal institutions. In so doing we

sought to ensure that all response routes were consistent with Colombia's legal framework, which itself enshrines and protects liberal rights and values. *ComunPaz* facilitators also documented the response routes devised in each community, which allowed us to confirm that they were indeed consistent with the legal roles and responsibilities of JACs under Colombian law.

C SITE SELECTION

The 24 municipalities in our study comprise a total of 240 "populated centers." The Colombian National Statistics Agency (DANE) defines a populated center as a community with at least 20 households located in a rural area but with urban characteristics (e.g. clearly defined internal roads or pedestrian walkways). To construct our sampling frame we merged the lists of populated centers maintained by DANE and the Geographical Institute Agustín Codazzi (IGAC). During exploratory field visits, we cross-checked this list against information from municipal administrators, and later with Google Maps. This process yielded 162 populated centers whose existence and location we were able to confirm. From this sampling frame we discarded communities with more than 5,000 residents in order to reduce heterogeneity in cluster size, and because the intervention was intentionally designed to target relatively small communities. This left us with a list of 149 eligible populated centers. For compactness, henceforth we refer to these populated centers as "communities."

D RANDOMIZATION

D.1 BLOCKING

In Oriente Antioqueño, we created three blocks of seven communities and one block of six (27 communities in total). In Nordeste Antioqueño, we created four blocks of six communities (24 communities in total). In Ariari-Guayabero, we created seven blocks of four communities and three blocks of five (43 communities in total). And in Centro del Valle del Cauca, we created five

blocks of five communities and five blocks of six (55 communities in total). We created these blocks using the blockTools package in R, which generates blocks of equal size with a remainder. In Ariari-Guayabero and Valle del Cauca, we reassigned each remainder community to the block whose average population size was most similar to the population of the community itself.

D.2 Compliance

In three treatment communities, JACs did not participate in the program at all, either because they refused or because they had disbanded by the time the program began. In five municipalities, encompassing 17 treatment communities, Police Inspectors did not participate in Module 1 of the program. In another two municipalities, encompassing eight treatment communities, police officers did not participate in Module 1. Importantly, there is no overlap between these two groups: *either* Police Inspectors *or* police officers participated in Module 1 in all municipalities.

As we discuss in detail in Appendix B, to minimize potential security risks for participants, JACs were empowered to decide whether, when, and how the program would be implemented in their communities. In two treatment communities, JAC members expressed to us that they would refuse to participate in Modules 3 or 4 if Police Inspectors participated. In seven communities, JAC members expressed to us that they would refuse to participate in Module 3 if police officers participated. There is again no overlap between these two groups: JAC members were willing to participate with *either* Police Inspectors *or* police officers in all treatment communities.

D.3 Maps of treatment and control communities

Figures A.2, A.3, and A.4 display the distribution of treatment and control communities across the four regions in our sample. Red dots denote control communities, green dots denote treatment communities.

D.4 BALANCE

Table A.1 reports results from a balance test for the *ComunPaz* program. We test balance by regressing a dummy for treatment assignment on six community-level characteristics: population; distance to the nearest arterial road in kilometers; distance to the municipal capital in both minutes and kilometers; the sum of all satellite-detected coca cultivation within 15km of each community in 2018; and a dummy indicating whether the community fell within the boundaries of the *Programa Nacional Integral de Sustitución de Cultivos Ilícitos* (PNIS), a voluntary coca substitution program established by the 2016 peace agreement, in 2018. We test the individual and joint significance of these community-level characteristics. We find no evidence of imbalance.

E ENDLINE SURVEY

E.1 SAMPLING FRAME

To sample residents, survey enumerators first created a map of all blocks (*manzanas*) in each community; when available, these maps were cross-checked against satellite images. Enumerators then randomly selected three blocks per community. Since most blocks consist of only a few households, in most cases enumerators surveyed all households on the selected blocks. If the total number of households on the selected blocks resulted in fewer than 18 respondents, enumerators randomly selected a fourth block to survey. If the total number of households resulted in more than 18 respondents, enumerators randomly selected households on each block, with the number of households selected proportional to the number of households on the block. Enumerators then randomly selected one adult resident (18+ years of age) from each selected household.

We surveyed 8 JAC leaders in each community, defined as any person serving in a position of responsibility in the JAC. All JACs have a board composed of a president, vice president, secretary and treasurer. We surveyed all four board members plus four leaders serving in committees or working groups whose existence varies across JACs. To construct the latter portion of the sample,

we first sampled JAC leaders serving on the CCC where one exists, and then leaders of other committees until we fulfilled the sampling quota. Finally, we surveyed one Police Inspector and one police commander per municipality. In municipalities where there was more than one Police Inspector and/or police commander, we selected the respondent at random.

E.2 Descriptive statistics

Tables A.2 and A.3 report individual- and community-level descriptive statistics, respectively. Individual-level descriptive statistics are from the resident and leader surveys. Community-level descriptive statistics on population are from the planning office of each municipality in the sample. Community-level descriptive statistics on distance to the nearest arterial road and distance to the municipal capital are from the Instituto Agustín Codazzi. Community-level descriptive statistics on coca cultivation and substitution are from the United Nations Office on Drugs and Crime (UNODC).

F Ancillary analyses and robustness checks

F.1 Manipulation check

Table A.4 reports the ITT of the *ComunPaz* program on awareness of any dispute resolution program in the community (columns 1 and 2) and awareness of the *ComunPaz* program specifically (columns 3 and 4). We code dummies for awareness of these programs among residents (columns 1 and 3) and leaders (columns 2 and 4). We find that residents of treatment communities were 4.1 percentage points more likely to be aware of any dispute resolution program in their community, and 2.9 percentage points more likely to be aware of the *ComunPaz* program specifically. We find that leaders were 9.5 percentage points more likely to be aware of any dispute resolution program in their community, and 11.9 percentage points more likely to be aware of the *ComunPaz* program specifically. The discrepancy between residents and leaders is unsurprising given that

leaders played a more central role in the program.

F.2 MULTIPLE HYPOTHESIS TESTING

Tables A.5, A.6, A.7, A.8, and A.9 replicate our results in Tables 1, 2, 3, 4, and 6, respectively, with Benjamini-Hochberg and Holm-Bonferroni corrections for multiple hypothesis testing. Each table reports the unadjusted p-value on our treatment assignment indicator, as well as the corresponding Benjamini-Hochberg q-value and Holm-Bonferroni threshold. Following (Anderson 2008), the Benjamini-Hochberg q-value is the smallest false discovery rate at which the null hypothesis will be rejected. The Holm-Bonferroni threshold is the adjusted p-value threshold below which the null hypothesis will be rejected at significance level $\alpha = 0.05$. We apply each correction within (but not across) "families" of hypotheses.[2] Thus, for example, Table A.8 amounts to a test of the hypothesis family that *ComunPaz* affected perceptions of any of the actors in our theoretical framework—armed groups, communal institutions, and the state. We omit the list experiment, endorsement experiment, and behavioral measure from these calculations, since the structure of these hypothesis tests differs dramatically from the structure of the others.

F.3 RELIANCE ON ARMED GROUPS TO RESOLVE DISPUTES USING LIST EXPERIMENT

Reliance on armed groups is a potentially sensitive subject. In an attempt to mitigate social desirability bias, we complemented the direct questions in the survey with a list experiment. List experiments allow respondents to answer sensitive questions indirectly (Tourangeau and Yan 2007). In our survey, respondents were read a list of mechanisms that communities can use to resolve "basic problems of coexistence (*convivencia*)," then asked to count how many of those mechanisms are used by residents of their own communities. The control group list included four items: (1) report to the JAC; (2) write a letter to the mayor; (3) resolve the problem directly; or (4) report to an

[2] In our PAP we pre-specified that we would apply multiple comparisons corrections across our "primary" and "secondary" hypotheses. This distinction between primary and secondary hypotheses turned out to be unhelpful and misleading, and we abandon it here.

indigenous leader. Because our sample did not include any indigenous communities, we assumed that respondents would be very unlikely to select the fourth item, thus reducing the risk of floor effects (Blair and Imai 2012). The treatment group list included an additional sensitive item: (5) report to an armed group.

Table A.10 reports item counts from the list experiment. Comparing the item counts in the treatment and control groups, we find no evidence of reliance on armed groups in the list experiment. The difference in means is just 0.014, and is not statistically significant at conventional levels. This does not appear to be an artifact of design effects. Using the diagnostic proposed in Blair and Imai (2012), we fail to reject the null of no design effect ($p = 0.414$ before a Bonferroni correction for multiple comparisons, $p = 0.827$ after). Our list experiment yields similar (and in some cases even lower) estimates for reliance on armed groups than our direct questions, suggesting that (1) reliance on armed groups is indeed rare, but also that (2) it is not an especially sensitive subject, or at least is not especially susceptible to social desirability bias.

These caveats notwithstanding, Table A.11 reports the ITT on residents' reliance on armed groups in the list experiment using a linear estimator[3] with the same controls, block fixed effects, and inverse probability weights as before. We find no evidence that *ComunPaz* reduced reliance on armed groups in the list experiment. This, however, may be an artifact of a lack of statistical power. Again, we find little to no evidence of social desirability bias in the survey, and list experiments are inherently much noisier than direct questions (Blair, Coppock and Moor 2020). This noise likely limits our ability to detect treatment effects of the *ComunPaz* program on responses to the list experiment.

F.4 Respect for government authority

Table A.12 reports the ITT of the *ComunPaz* program on respect for government authority in cases that fall unambiguously under government jurisdiction (hypothesis P5). To measure respect

[3] Blair and Imai (2012) show that this estimator is less efficient than a maximum likelihood estimator. Unfortunately, the latter can only accommodate a very small number of regressors. (Indeed, in our case the maximum likelihood estimator fails to converge even when the only regressor is assignment to the *ComunPaz* program.) We therefore report results using the linear estimator instead.

for government authority, we use the same hypothetical scenarios of conflict and crime and the same actual disputes that we used to measure reliance on governmental institutions, but we focus exclusively on hypothetical scenarios and actual disputes over which the government claims both original and ultimate jurisdiction (e.g. domestic violence, robbery, and aggravated assault).

Unfortunately, this outcome proved very difficult to measure in the context of our survey. In most cases residents of rural Colombian communities report conflicts and crimes to communal institutions first, regardless of the severity of the incident. As we learned during data collection, this is only sometimes indicative of disrespect for government authority. In some cases residents report to communal institutions first because they do not know whether or not their case merits a criminal complaint, because they do not understand how to access the police and courts and want communal authorities to serve as liaisons, or because they wish to keep communal authorities apprised of problems in the community; in other cases they report to communal institutions because they wish to avoid the police and courts altogether. Only the last of these possibilities is consistent with disrespect for government authority, and even in these cases it is hard to disentangle disrespect from fear or distrust. With these caveats in mind, we find no evidence that the program increased respect for government authority as measured in our survey.

F.5 Prevalence of disputes

[ADD DISCUSSION OF RESULTS]

F.6 Reliance on direct mediation to resolve disputes

[ADD DISCUSSION OF RESULTS]

F.7 Reliance on multiple authorities to resolve disputes

[ADD DISCUSSION OF RESULTS]

F.8 HETEROGENEOUS TREATMENT EFFECTS

Tables A.16 through A.21 report HTEs of the *ComunPaz* program on the prevalence of unresolved and violent disputes (Tables A.16 through A.18) and reliance on armed groups and governmental and communal institutions to resolve disputes (Tables A.19 through A.21). We test for treatment effect heterogeneity by connectedness to local and municipal political power (Tables A.16 and A.19); histories of governance by rebel and paramilitary groups (Tables A.17 and A.20); and degree of exposure to violence perpetrated by government, rebel, and paramilitary forces during the civil war (Tables A.18 and A.21).

To measure connectedness to political power, residents were asked they are members of the JAC in their community; whether they are members of the JAC's board of directors; whether they are related to a JAC member; whether they are related to the mayor; and whether they are related to a city councilor. We code a dummy for residents who answered any of these questions affirmatively. We measure connectedness among leaders using the last two of these five questions. To measure rebel and paramilitary governance, residents and leaders were asked whether there was ever a time when rebel or paramilitary groups controlled the entrances and exits to their community, made and enforced rules in the community, or resolved disputes in the community. We code the proportion of residents and leaders who answered any of these questions affirmatively in each community. Finally, to measure exposure to violence, residents and leaders were asked whether they or a family member had been victim of seven different forms of violence during the civil war, including kidnapping, extortion, and murder, as well as the identity of the perpetrator(s). We code standardized additive indices for exposure to violence perpetrated by government, rebel, and paramilitary forces.

In general, we find little to no evidence of treatment effect heterogeneity along any of these dimensions. Previous studies have found that politically connected individuals tend to benefit disproportionately from programs designed to improve the quality of dispute resolution at the local level (Hartman, Blair and Blattman 2019). In the context of the *ComunPaz* program, we might

expect politically connected individuals to report especially large reductions in unresolved and violent disputes. In Table A.16, we find no evidence of treatment effect heterogeneity of this sort. We might also expect politically connected individuals to be especially likely to substitute towards both governmental and communal institutions to resolve disputes, given that these individuals are likely to enjoy privileged status and easier access to these institutions. In Table A.19, we find no evidence of treatment effect heterogeneity of this sort either.

In rural Colombia, rebel groups (especially FARC) often coopted communal institutions as mechanisms of governance at the local level. As a result, JACs tend to be especially strong in communities previously controlled by rebel groups (Vargas Castillo 2019). Paramilitaries did not coopt communal institutions in this way, at least not in rural communities. In the context of *ComunPaz*, we might expect communities that were controlled by rebels in the past (and that therefore have stronger JACs in the present) to report especially large reductions in unresolved and violent disputes, given how central JACs were to the success of the program. In Table A.17, we find that residents of communities that were previously controlled by rebel groups report especially large reductions in unresolved (but not violent) disputes. Leaders of these communities also report especially large reductions in unresolved disputes, though this effect is not statistically significant. We find no evidence of treatment effect heterogeneity by paramilitary governance.

Communities that were previously controlled by armed groups may be less receptive to renewed government presence, and thus less willing to substitute towards governmental institutions to resolve disputes. In Table A.20, we find no evidence of treatment effect heterogeneity of this sort. Individuals who were especially severely victimized by state security forces in the past may be similarly less willing to substitute towards governmental institutions in the present, while those who were especially severely victimized by rebels (and perhaps paramilitaries as well) may be more so. In Table A.21 we find no evidence of treatment effect heterogeneity of this sort; indeed, if anything, the program appears to have induced more rather than less reliance on armed groups among victims of rebel violence.

F.9 ALTERNATE CODING RULES FOR DEPENDENT VARIABLES

Tables A.22, A.23, A.24, and A.25 explore the robustness of our results to alternate coding rules for our dependent variables. Table 1 reports the ITT of the *ComunPaz* program on the number of unresolved and violent disputes, rather than dummies for any unresolved or violent disputes. (This analysis was not prespecified.) Our ITT estimates remain negative across specifications but are no longer statistically significant at conventional levels, suggesting that the program reduced the prevalence of unresolved and violent disputes along the extensive margin, but not the intensive margin.

Table A.23 reports the ITT on indices for reliance on armed groups, JACs, and police officers and Police Inspectors to resolve disputes, rather than dummies. Because very few residents or leaders reported relying on armed groups, our indices of reliance on armed groups are highly skewed, and our results in columns 1 and 2 are likely driven by outliers. (This analysis also was not prespecified.) With this caveat in mind, the negative ITT on reliance on armed groups among residents is no longer statistically significant at conventional levels when we index in this way.

Table A.24 reports the ITT on indices for perceptions of armed groups, JACs, and police officers and Police Inspectors, rather than dummies. Again, because very few residents or leaders reported favorable perceptions of armed groups, our indices of perceptions of armed groups are highly skewed. In addition, our indices for perceptions of JACs among police commanders and Police Inspectors suffer from missingness (and thus from post-treatment bias), since police commanders and Police Inspectors reported not knowing the JACs in some communities in our sample. In the body of the paper we avoid this problem by coding these communities as 0s. (This analysis also was not prespecified.) With these caveats in mind, the negative ITT on perceptions of armed groups among residents is no longer statistically significant, while the negative ITT among leaders is now statistically significant at conventional levels, albeit only weakly so. The positive ITT on perceptions of JACs among Police Inspectors remains substantively large but is imprecisely estimated. The positive ITT on perceptions of police officers and Police Inspectors among residents

remains weakly statistically significant.

Following our PAP, Table A.25 reports the ITT on reliance on JACs and police officers and Police Inspectors, disaggregating by jurisdiction. We test whether the program increased reliance on JACs in cases that JACs are legally authorized to resolve (columns 1 and 2) and in cases they are not (columns 3 and 4). We also test whether the program increased reliance on police officers and Police Inspectors in cases that JACs are legally authorized to resolve (columns 5 and 6) and in cases they are not (columns 7 and 8). Our ITT estimates remain statistically indistinguishable from 0 regardless of specification.

Finally, and again following our PAP, Table A.26 reports the ITT on the degree of consensus around how particular disputes should be resolved, using alternate constructions in the dependent variable. For each of four hypothetical scenarios of conflict and crime, respondents were asked which authority they would report to first, and which they believed should provide a "final and definitive" resolution. We take the modal response to each of these questions among residents in each community, then test whether the program increased the likelihood that each resident's response (column 1) and each leader's response (column 2) matches the modal resident response. We also take the modal response among leaders and test whether the program increased the likelihood that each resident's response (column 3) and each leader's response (column 4) matches the modal lead response. Our ITT estimates remain statistically indistinguishable from 0 regardless of specification.

F.10 Missingness in behavioral dependent variables

Table A.27 reports the correlates of missingess in our costly behavioral measures of demand for (column 1) and actual (column 2) coordination between governmental and communal institutions. We find no evidence that missingness is correlated with treatment, or with any of our community-level control variables.

G QUALITATIVE DATA CODING RULES AND ANALYSIS

Qualitative data consisted of detailed field reports compiled by facilitators for each module of the program. To contextualize the quantitative results of the study and explore the plausibility of particular interpretations of these results, we systematically extracted and classified information on the following topics: actors involved in dispute resolution; types of disputes and dispute resolution practices; armed group presence; response routes *(*"rutas de atención"); assumptions of program design; relations between JACs and state authorities during the program; relations between residents; and resolution of disputes in connection with program activities. We classified the information in facilitator reports in spreadsheets, where each row corresponded to a treatment community, and columns corresponded to variables that operationalized each of the above themes. In some cases, the variables simply served as categories to synthesize or cite relevant passages from the field reports, while others were coded as dichotomous or categorical variables. Below we offer a brief description of the specific variables coded for each thematic area of interest, and wherever applicable the specific source within the field reports used to code those variables.

Local actors involved in dispute resolution: Our quantitative results showed a decrease in unresolved and violent disputes, but no increased reliance on communal or state authorities. To attempt to explain this puzzling combination of results, we used our qualitative data to extract additional information on the actors involved in dispute resolution, beyond those included in our the survey. To identify these actors, we reviewed all facilitator reports, with particular emphasis on two activities. The first was activity 4 in module 2 (M2.A4)[4], where community members and JAC leaders were prompted to reconstruct their current conflict resolution practices. The second was M3.A4, where state and communal authorities jointly created response routes for conflict resolution. We identified 31 actors or institutions that are involved in dispute resolution in these communities. Besides coding a categorical variable for each of these actors, we also coded segments of text describing these actors' involvement in dispute resolution.

[4]We will refer to modules and activities using the shorthand M#.A#, where the "M" stands for module, and the "A" for activity.

Disputes and dispute resolution practices: To supplement our quantitative data, we coded the most important sources of disputes that were prioritized for discussion by community members in M2.A2. We also read through facilitator reports for all modules and indexed all information where existing practices of dispute resolution were described. Finally, we coded which actors were considered by the community to be the primary authority in M4.A3.

Armed group presence: We coded a treatment community as having armed group presence if it was directly reported by community members or facilitators, or if events or comments allowed us to infer armed group presence from facilitator reports. Examples of the latter include violent events like the kidnapping of municipal officials, or comments on security concerns related to visits to surrounding areas. We also read all facilitator reports to index mentions of the names of armed groups or descriptions of their activities (past or present).

Response routes: We used facilitator reports to code the complexity of response routes. We constructed five different proxies for complexity: number of actors included in the response route, number of actors included in the first step, number of steps, existence of paths conditional on the type of conflict (yes/no), and the number of "forks" in the response route. The first three proxies were based on information for M3.A5; the latter two were based on visual depictions of the response routes used to disseminate them to communities during module 4. We were also interested in characterizing communal and state authority involvement in response routes, and subjective perceptions about them. To explore these issues we coded the "first responder" for each response route, based on M3.A5 as well as the visual depiction of the response route, and we also indexed all subjective perceptions of the response routes mentioned by community members in module 4.

Relations between JACs and state authorities: To investigate trust between communal and state authorities during the program, we indexed segments of facilitator reports from modules 3 and 4 in which facilitators commented on state/community authority relations, or where they reported specific interactions that spoke to the quality and characteristics of such relations.

Relations between residents: We indexed segments of the facilitator reports that contained

information on the characteristics of social interactions between residents of treatment communities, paying special attention to the perceptions of community members regarding their neighbors with community leadership roles.

Resolution of disputes during the program: We indexed segments of the facilitator reports in which facilitators described any actions taken in regards to resolve disputes that were raised in the context of the program itself. This information was particularly relevant to evaluate whether the program might have reduced the "stock" of disputes in treatment communities. We indeed found several instances where authorities made commitments to intervene in particular disputes, and at least one instance where an agreement among disputants was reached.

Program design assumptions: Finally, we used the qualitative data to generate anecdotal evidence on the validity of the assumptions underlying the program. We reviewed all facilitator reports to code any descriptions of mistrust or miscommunication between state and communal authorities, and coded a dichotomous variable indicating the presence of such descriptions. We found descriptions of some kind of mistrust or miscommunication between state and communal authorities in one-third (23) of all treatment communities. We also found that in 19 treatment communities, distance to the municipal capital was cited as an obstacle to the delivery of state dispute resolution services, or to coordination between state and communal authorities. These findings are consistent with the assumptions underlying the program.

Figure A.1: **An anonymized response route (“*ruta de atención*”)**

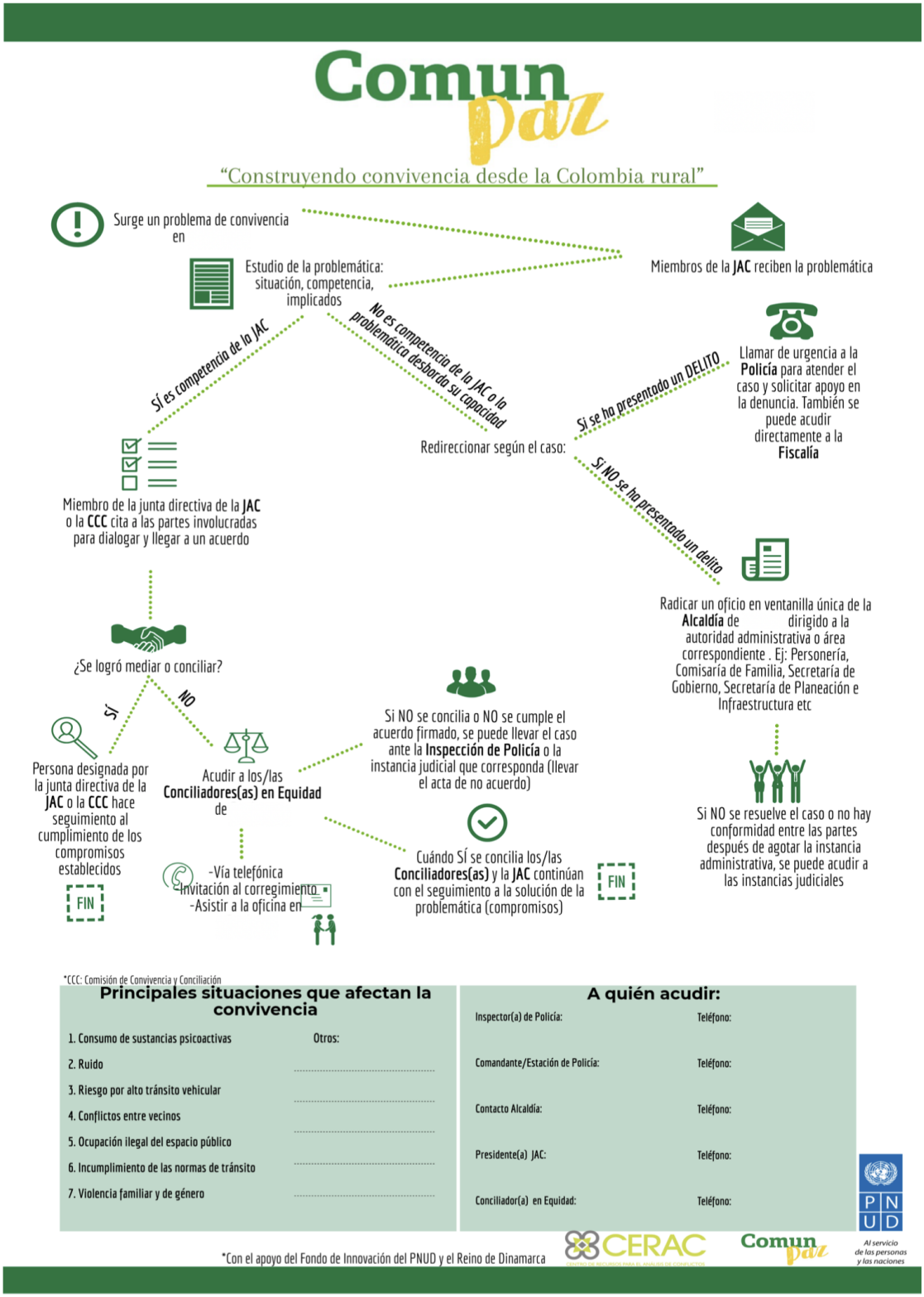
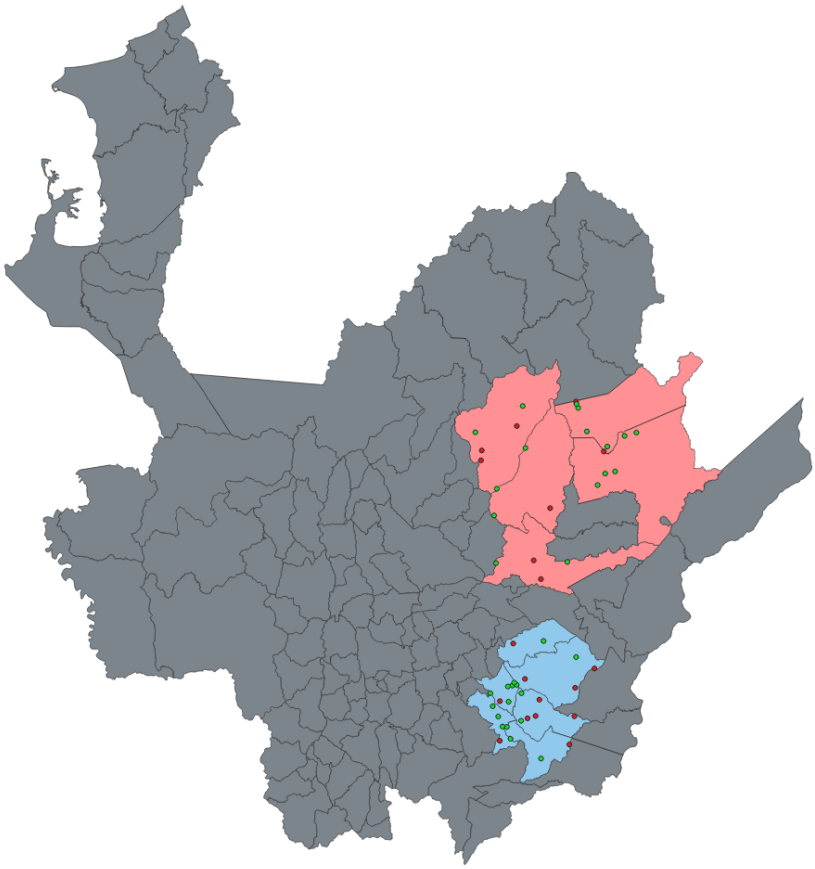
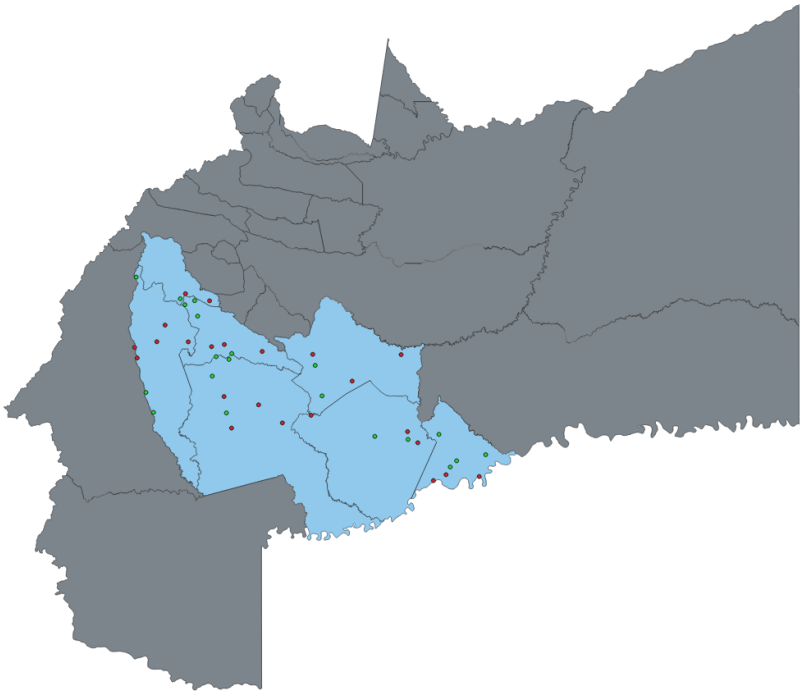


Figure A.2: **Site selection in Antioquia**



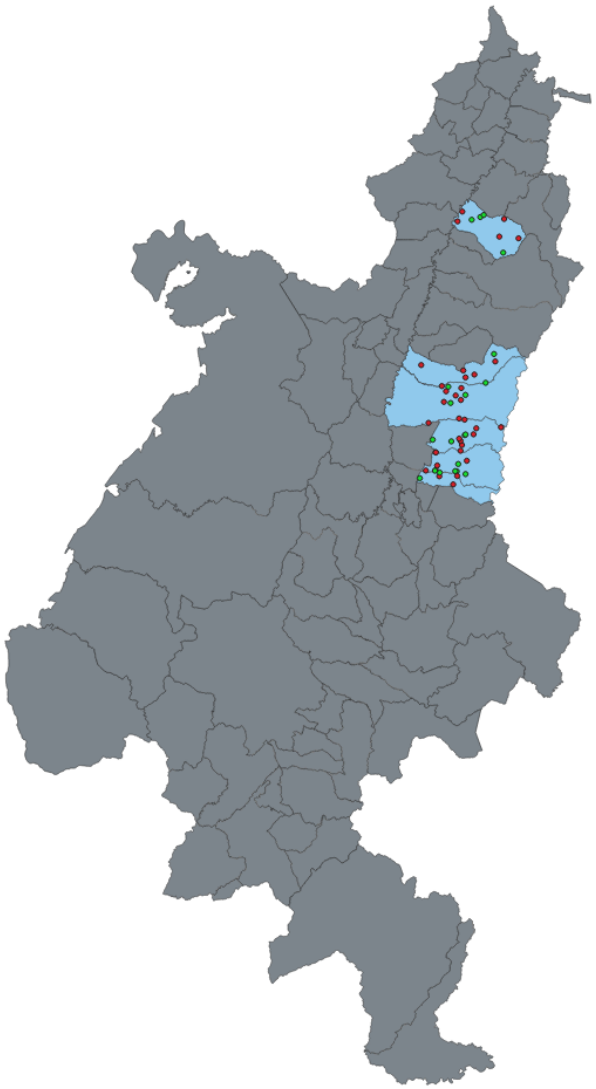
Notes: We sample from two separate sub-regions in Antioquia: Nordeste (salmon shading) and Oriente (blue shading). Red dots denote control communities, green dots denote treatment communities.

Figure A.3: **Site selection in Ariari-Guayabero**



Notes: Red dots denote control communities, green dots denote treatment communities.

Figure A.4: **Site selection in Centro del Valle del Cauca**



Notes: Red dots denote control communities, green dots denote treatment communities.

Table A.1: **Balance**

	Assigned to treatment
Population	0.000
	[0.000]
Distance to nearest arterial road (km)	0.003
	[0.004]
Distance to municipal capital (km)	-0.003
	[0.005]
Distance to municipal capital (min.)	-0.001
	[0.002]
Coca cultivation within 15km	-0.000
	[0.001]
Coca substitution program	-0.012
	[0.130]
Observations	149
Individual controls	No
Community controls	Yes
Block FE	Yes
Weights	No
Estimator	OLS
F	.208
p(F)	.974

Notes: Balance test for the *ComunPaz* program including block fixed effects. Standard errors are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.2: **Resident and leader characteristics**

	Residents			Leaders		
	Mean	S.D.	N	Mean	S.D.	N
Age	46.08	16.13	2673	47.36	13.04	1182
Male	0.35	0.48	2673	0.51	0.50	1182
Quality of walls	0.93	0.25	2673	0.94	0.23	1182
Quality of floors	0.91	0.29	2673	0.92	0.28	1182
Household size	3.29	1.68	2673	3.52	1.59	1182
Preschool	0.04	0.19	2673	0.01	0.12	1182
Primary school	0.70	0.46	2673	0.58	0.49	1182
Middle school	0.20	0.40	2673	0.28	0.45	1182
Employed	0.55	0.50	2673	0.75	0.44	1182

Notes: Individual-level descriptive statistics from resident and leader surveys.

Table A.3: **Community characteristics**

	Mean	S.D.	N
Population	764.33	757.19	149
Distance to nearest arterial road (km)	21.16	21.17	149
Distance to municipal capital (km)	20.22	14.66	149
Distance to municipal capital (min.)	58.01	47.23	149
Coca cultivation within 15km	49.92	134.33	149
Coca substitution program	0.38	0.49	149

Notes: Community-level descriptive statistics from municipal planning offices, the Instituto Agustín Codazzi, and the United Nations Office on Drugs and Crime (UNODC).

Table A.4: **Manipulation check**

	Heard of dispute resolution program		**Heard of *ComunPaz* program**	
	(1) Residents	(2) Leaders	(3) Residents	(4) Leaders
Assigned to treatment	0.041^{***} [0.014]	0.095^{***} [0.028]	0.029^{**} [0.013]	0.119^{***} [0.027]
Observations	2673	1182	2673	1182
Individual controls	Yes	Yes	Yes	Yes
Community controls	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes
Estimator	OLS	OLS	OLS	OLS

Notes: All specifications include individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors, clustered by community, are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.5: **Prevalence of unresolved and violent disputes with multiple comparisons corrections**

	Any unresolved disputes		**Any violent disputes**	
	(1) Residents	(2) Leaders	(3) Residents	(4) Leaders
Assigned to treatment	-0.027	-0.093**	0.001	-0.051*
	[0.033]	[0.041]	[0.010]	[0.026]
Observations	2673	1182	2673	1182
Individual controls	Yes	Yes	Yes	Yes
Community controls	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes
Estimator	OLS	OLS	OLS	OLS
p-value	0.418	0.024	0.945	0.053
B-H q-value	0.557	0.097	0.945	0.106
H-B threshold	0.050	0.025	0.100	0.033

Notes: All specifications include individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors, clustered by community, are in brackets. The B-H q-value is the smallest false discovery rate at which the null hypothesis will be rejected, following the Benjamini-Hochberg procedure. The H-B threshold is the adjusted p-value threshold below which the null hypothesis will be rejected at significance level $\alpha = 0.1$. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.6: **Reliance on armed groups and state and communal authorities to resolve disputes with multiple comparisons corrections**

	Reliance on armed groups		**Reliance on JACs**		**Reliance on police and PIs**	
	(1) Residents	(2) Leaders	(3) Residents	(4) Leaders	(5) Residents	(6) Leaders
Assigned to treatment	-0.056**	-0.001	-0.028	0.039	-0.028	-0.022
	[0.027]	[0.037]	[0.051]	[0.055]	[0.055]	[0.057]
Observations	2673	1182	2673	1182	2673	1182
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Community controls	Yes	Yes	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes	Yes
Estimator	AES	AES	AES	AES	AES	AES
p-value	0.038	0.813	0.584	0.477	0.606	0.697
B-H q-value	0.230	0.813	0.813	0.813	0.813	0.813
H-B threshold	0.017	0.100	0.025	0.020	0.033	0.050

Notes: All specifications include individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors, clustered by community, are in brackets. The B-H q-value is the smallest false discovery rate at which the null hypothesis will be rejected, following the Benjamini-Hochberg procedure. The H-B threshold is the adjusted p-value threshold below which the null hypothesis will be rejected at significance level $\alpha = 0.1$. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.7: **Information about communities and Colombian law with multiple comparisons corrections**

	Understanding of JACs' authority		**Understanding of most important disputes**		
	(1) Residents	(2) Leaders	(3) Leaders	(4) Police	(5) PIs
Assigned to treatment	0.015	0.014	-0.026	0.007	0.040
	[0.034]	[0.031]	[0.039]	[0.041]	[0.041]
Observations	2620	1171	1182	149	149
Individual controls	Yes	Yes	Yes	No	No
Community controls	Yes	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes
Estimator	AES	AES	OLS	OLS	OLS
p-value	0.647	0.654	0.508	0.863	0.335
B-H q-value	0.818	0.818	0.818	0.864	0.818
H-B threshold	0.033	0.050	0.025	0.100	0.020

Notes: Specifications in columns 1-3 include individual- and community-level controls, block fixed effects, and inverse probability weights. Columns 4 and 5 exclude individual-level controls. Standard errors are in brackets, and are clustered by community in columns 1-3. The B-H q-value is the smallest false discovery rate at which the null hypothesis will be rejected, following the Benjamini-Hochberg procedure. The H-B threshold is the adjusted p-value threshold below which the null hypothesis will be rejected at significance level $\alpha = 0.1$. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.8: **Perceptions of armed groups and state and communal authorities with multiple comparisons corrections**

	Perceptions of armed groups		**Perceptions of JACs**			**Perceptions of police and PIs**	
	(1) Residents	(2) Leaders	(3) Residents	(4) Police	(5) PIs	(6) Residents	(7) Leaders
Assigned to treatment	-0.083**	-0.074	-0.023	0.012	0.187**	0.105*	0.068
	[0.037]	[0.046]	[0.062]	[0.074]	[0.085]	[0.064]	[0.066]
Observations	2673	1182	2673	149	149	2673	1182
Individual controls	Yes	Yes	Yes	No	No	Yes	Yes
Community controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Estimator	AES	AES	AES	OLS	OLS	AES	AES
p-value	0.024	0.106	0.715	0.875	0.029	0.098	0.301
B-H q-value	0.102	0.186	0.834	0.875	0.102	0.186	0.422
H-B threshold	0.014	0.025	0.050	0.100	0.017	0.020	0.033

Notes: Specifications in columns 1-3, 6, and 7 include individual- and community-level controls, block fixed effects, and inverse probability weights. Columns 4 and 5 exclude individual-level controls. The B-H q-value is the smallest false discovery rate at which the null hypothesis will be rejected, following the Benjamini-Hochberg procedure. The H-B threshold is the adjusted p-value threshold below which the null hypothesis will be rejected at significance level $\alpha = 0.1$. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.9: **Coordination between and among governmental and communal authorities with multiple comparisons corrections**

	Consensus around dispute resolution		**Coordination between JACs, police, and PIs**			**Coordination within JACs**
	(1) Residents	(2) Leaders	(3) Leaders	(4) Police	(5) PIs	(6) Leaders
Assigned to treatment	-0.049	-0.040	0.093*	0.028	0.249**	0.153**
	[0.035]	[0.030]	[0.056]	[0.129]	[0.114]	[0.062]
Observations	2673	1182	1168	149	149	1135
Individual controls	Yes	Yes	Yes	No	No	Yes
Community controls	Yes	Yes	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes	Yes
Estimator	AES	AES	AES	AES	AES	AES
p-value	0.163	0.177	0.099	0.827	0.030	0.099
B-H q-value	0.213	0.213	0.198	0.827	0.090	0.198
H-B threshold	0.033	0.050	0.025	0.100	0.020	0.025

Notes: Specifications in columns 1-3 and 6 include individual- and community-level controls, block fixed effects, and inverse probability weights. Columns 4 and 5 exclude individual-level controls. The B-H q-value is the smallest false discovery rate at which the null hypothesis will be rejected, following the Benjamini-Hochberg procedure. The H-B threshold is the adjusted p-value threshold below which the null hypothesis will be rejected at significance level $\alpha = 0.1$. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.10: **Item counts in list experiment**

	Control		**Treatment**	
	Frequency	Percentage	Frequency	Percentage
0	24	1.83	19	1.40
1	573	43.61	604	44.44
2	577	43.91	580	42.68
3	131	9.97	141	10.38
4	9	0.68	13	0.96
5			2	0.15

Notes: Item counts from the list experiment.

Table A.11: **Reliance on armed groups among residents using list experiment**

	Reliance on armed groups
Assigned to treatment	0.046
	[0.043]
Observations	2673
Individual controls	Yes
Community controls	Yes
Block FE	Yes
Weights	Yes
Estimator	OLS

Notes: Specification includes individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors, clustered by community, are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.12: **Respect for government authority**

	Respect for government authority	
	(1) Residents	(2) Leaders
Assigned to treatment	-0.026	0.004
	[0.056]	[0.052]
Observations	2673	1182
Individual controls	Yes	Yes
Community controls	Yes	Yes
Block FE	Yes	Yes
Weights	Yes	Yes
Estimator	AES	AES

Notes: All specifications include individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors, clustered by community, are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.13: **Reliance on direct mediation to resolve disputes**

	Direct mediation for dispute resolution	
	(1) Residents	(2) Leaders
Assigned to treatment	-0.022	-0.069*
	[0.031]	[0.037]
Observations	2673	1182
Individual controls	Yes	Yes
Community controls	Yes	Yes
Block FE	Yes	Yes
Weights	Yes	Yes
Estimator	OLS	OLS

Notes: All specifications include individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors, clustered by community, are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.14: **Reliance on multiple authorities to resolve disputes**

	Reliance on multiple authorities	
	(1) Residents	(2) Leaders
Assigned to treatment	0.013	0.042
	[0.017]	[0.033]
Observations	2673	1182
Individual controls	Yes	Yes
Community controls	Yes	Yes
Block FE	Yes	Yes
Weights	Yes	Yes
Estimator	OLS	OLS

Notes: All specifications include individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors, clustered by community, are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.15: **Prevalence of disputes**

	Any disputes	
	(1) Residents	(2) Leaders
Assigned to treatment	-0.038	-0.099***
	[0.033]	[0.037]
Observations	2673	1182
Individual controls	Yes	Yes
Community controls	Yes	Yes
Block FE	Yes	Yes
Weights	Yes	Yes
Estimator	OLS	OLS

Notes: All specifications include individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors, clustered by community, are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.16: **Prevalence of unresolved and violent disputes, heterogeneity by connectedness**

	Any unresolved disputes		**Any violent disputes**	
	(1) Residents	(2) Leaders	(3) Residents	(4) Leaders
Assigned to treatment	-0.079	-0.131**	-0.004	-0.054*
	[0.056]	[0.053]	[0.015]	[0.030]
Connected	-0.035	0.017	0.032**	0.013
	[0.039]	[0.047]	[0.014]	[0.027]
Assigned to treatment × connected	0.079	0.077	0.005	0.009
	[0.056]	[0.062]	[0.022]	[0.039]
Observations	2673	1182	2673	1182
Individual controls	Yes	Yes	Yes	Yes
Community controls	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes
Estimator	OLS	OLS	OLS	OLS

Notes: Heterogeneous treatment effects (HTEs) of the *ComunPaz* program by connectedness to local and municipal power. All specifications include individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors, clustered by community, are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.17: **Prevalence of unresolved and violent disputes, heterogeneity by armed group governance**

	Any unresolved disputes		**Any violent disputes**	
	(1) Residents	(2) Leaders	(3) Residents	(4) Leaders
Assigned to treatment	0.074	-0.014	0.006	-0.017
	[0.080]	[0.103]	[0.020]	[0.040]
Rebel governance	0.062	0.101	-0.035	0.017
	[0.099]	[0.121]	[0.032]	[0.080]
Paramilitary governance	-0.028	0.118	0.024	0.175**
	[0.112]	[0.118]	[0.030]	[0.071]
Assigned to treatment × rebel governance	-0.261**	-0.124	0.004	0.018
	[0.120]	[0.147]	[0.038]	[0.091]
Assigned to treatment × paramilitary governance	0.119	0.003	-0.016	-0.082
	[0.122]	[0.153]	[0.038]	[0.099]
Observations	2673	1182	2673	1182
Individual controls	Yes	Yes	Yes	Yes
Community controls	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes
Estimator	OLS	OLS	OLS	OLS

Notes: Heterogeneous treatment effects (HTEs) of the *ComunPaz* program by armed group governance. All specifications include individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors, clustered by community, are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.18: **Prevalence of unresolved and violent disputes, heterogeneity by exposure to violence**

	Any unresolved disputes		**Any violent disputes**	
	(1) Residents	(2) Leaders	(3) Residents	(4) Leaders
Assigned to treatment	-0.026	-0.077*	0.001	-0.045*
	[0.034]	[0.042]	[0.010]	[0.027]
Rebel violence	0.039*	0.033*	0.018*	0.020
	[0.021]	[0.020]	[0.010]	[0.015]
Paramilitary violence	0.037**	0.025	0.014	0.046***
	[0.017]	[0.019]	[0.011]	[0.015]
Government violence	-0.020*	-0.072*	0.022	-0.007
	[0.011]	[0.039]	[0.013]	[0.040]
Assigned to treatment × rebel violence	-0.025	0.026	0.009	0.025
	[0.030]	[0.031]	[0.014]	[0.022]
Assigned to treatment × paramilitary violence	-0.041*	-0.009	-0.008	-0.023
	[0.022]	[0.029]	[0.013]	[0.019]
Assigned to treatment × government violence	0.030**	0.040	-0.017	-0.000
	[0.014]	[0.040]	[0.015]	[0.041]
Observations	2631	1160	2631	1160
Individual controls	Yes	Yes	Yes	Yes
Community controls	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes
Estimator	OLS	OLS	OLS	OLS

Notes: Heterogeneous treatment effects (HTEs) of the *ComunPaz* program by exposure to violence. All specifications include individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors, clustered by community, are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.19: **Reliance on armed groups and governmental and communal institutions to resolve disputes, heterogeneity by connectedness**

	Reliance on armed groups		**Reliance on JACs**		**Reliance on police and PIs**	
	(1) Residents	(2) Leaders	(3) Residents	(4) Leaders	(5) Residents	(6) Leaders
Assigned to treatment	-0.027	0.009	-0.027	0.015	-0.055	-0.054
	[0.032]	[0.035]	[0.078]	[0.067]	[0.070]	[0.057]
Connected	0.046	0.072	0.029	-0.001	0.073	0.101*
	[0.054]	[0.050]	[0.055]	[0.050]	[0.054]	[0.054]
Assigned to treatment × connected	-0.046	-0.018	-0.008	0.044	0.046	0.082
	[0.061]	[0.064]	[0.080]	[0.071]	[0.069]	[0.075]
Observations	2673	1182	2673	1182	2673	1182
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Community controls	Yes	Yes	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes	Yes
Estimator	AES	AES	AES	AES	AES	AES

Notes: Heterogeneous treatment effects (HTEs) of the *ComunPaz* program by connectedness to local and municipal power. All specifications include individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors, clustered by community, are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.20: **Reliance on armed groups and governmental and communal institutions to resolve disputes, heterogeneity by armed group governance**

	Reliance on armed groups		**Reliance on JACs**		**Reliance on police and PIs**	
	(1) Residents	(2) Leaders	(3) Residents	(4) Leaders	(5) Residents	(6) Leaders
Assigned to treatment	-0.054	-0.095	0.014	0.093	-0.072	0.006
	[0.045]	[0.064]	[0.125]	[0.145]	[0.146]	[0.131]
Rebel governance	0.127	0.057	0.452***	0.403***	-0.424***	-0.413**
	[0.086]	[0.110]	[0.148]	[0.153]	[0.148]	[0.173]
Paramilitary governance	-0.161*	-0.185	-0.129	-0.091	0.318**	0.332**
	[0.087]	[0.127]	[0.141]	[0.129]	[0.148]	[0.161]
Assigned to treatment × rebel governance	-0.119	0.016	-0.120	-0.103	0.171	0.121
	[0.081]	[0.111]	[0.163]	[0.176]	[0.189]	[0.193]
Assigned to treatment × paramilitary governance	0.132	0.158	0.042	0.007	-0.085	-0.175
	[0.094]	[0.138]	[0.191]	[0.183]	[0.186]	[0.207]
Observations	2673	1182	2673	1182	2673	1182
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Community controls	Yes	Yes	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes	Yes
Estimator	AES	AES	AES	AES	AES	AES

Notes: Heterogeneous treatment effects (HTEs) of the *ComunPaz* program by armed group governance. All specifications include individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors, clustered by community, are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.21: **Reliance on armed groups and governmental and communal institutions to resolve disputes, heterogeneity by exposure to violence**

	Reliance on armed groups		**Reliance on JACs**		**Reliance on police and PIs**	
	(1) Residents	(2) Leaders	(3) Residents	(4) Leaders	(5) Residents	(6) Leaders
Assigned to treatment	-0.051*	-0.011	-0.033	0.050	-0.018	-0.000
	[0.029]	[0.041]	[0.050]	[0.057]	[0.054]	[0.060]
Rebel violence	-0.006	0.003	0.066***	0.040*	0.067***	0.052**
	[0.019]	[0.015]	[0.022]	[0.021]	[0.026]	[0.025]
Paramilitary violence	-0.003	0.005	-0.020	-0.023	0.017	-0.005
	[0.019]	[0.020]	[0.034]	[0.030]	[0.026]	[0.030]
Government violence	0.011	0.056	-0.021	0.093**	-0.007	-0.046
	[0.020]	[0.073]	[0.027]	[0.040]	[0.019]	[0.058]
Assigned to treatment × rebel violence	0.068**	0.022	-0.006	-0.040	0.014	-0.007
	[0.032]	[0.033]	[0.036]	[0.036]	[0.038]	[0.035]
Assigned to treatment × paramilitary violence	0.005	0.019	-0.020	0.003	-0.008	-0.023
	[0.030]	[0.037]	[0.045]	[0.041]	[0.034]	[0.041]
Assigned to treatment × government violence	-0.009	-0.063	0.016	-0.056	0.024	0.040
	[0.021]	[0.071]	[0.029]	[0.042]	[0.022]	[0.059]
Observations	2631	1160	2631	1160	2631	1160
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Community controls	Yes	Yes	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes	Yes
Estimator	AES	AES	AES	AES	AES	AES

Notes: Heterogeneous treatment effects (HTEs) of the *ComunPaz* program by exposure to violence. All specifications include individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors, clustered by community, are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.22: **Prevalence of unresolved and violent disputes, indexed DVs**

	# of unresolved disputes		**# of violent disputes**	
	(1) Residents	(2) Leaders	(3) Residents	(4) Leaders
Assigned to treatment	-0.021 [0.081]	-0.137 [0.160]	-0.015 [0.020]	-0.130 [0.120]
Observations	2673	1182	2673	1182
Individual controls	Yes	Yes	Yes	Yes
Community controls	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes
Estimator	OLS	OLS	OLS	OLS

Notes: All specifications include individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors, clustered by community, are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.23: **Reliance on armed groups and state and communal authorities to resolve disputes, indexed DVs**

	Reliance on armed groups		**Reliance on JACs**		**Reliance on police and PIs**	
	(1) Residents	(2) Leaders	(3) Residents	(4) Leaders	(5) Residents	(6) Leaders
Assigned to treatment	-0.040	0.037	0.000	0.063	-0.032	-0.006
	[0.038]	[0.059]	[0.050]	[0.061]	[0.054]	[0.054]
Observations	2249	1050	2673	1182	2673	1182
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Community controls	Yes	Yes	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes	Yes
Estimator	AES	AES	AES	AES	AES	AES

Notes: All specifications include individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors, clustered by community, are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.24: **Perceptions of armed groups and state and communal authorities, indexed DVs**

	Perceptions of armed groups		**Perceptions of JACs**			**Perceptions of police and PIs**	
	(1) Residents	(2) Leaders	(3) Residents	(4) Police	(5) PIs	(6) Residents	(7) Leaders
Assigned to treatment	-0.064	-0.080*	-0.026	-0.183	0.174	0.115*	0.056
	[0.045]	[0.049]	[0.064]	[0.212]	[0.213]	[0.067]	[0.066]
Observations	2298	1062	2673	81	89	2673	1182
Individual controls	Yes	Yes	Yes	No	No	Yes	Yes
Community controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Estimator	AES	AES	AES	OLS	OLS	AES	AES

Notes: Specifications in columns 1-3, 6, and 7 include individual- and community-level controls, block fixed effects, and inverse probability weights. Columns 4 and 5 exclude individual-level controls. Standard errors are in brackets, and are clustered by community in columns 1-3, 6, and 7. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.25: **Reliance on armed groups and state and communal authorities to resolve disputes, disaggregated by jurisdiction**

	Reliance on JACs (communal jurisdiction)		**Reliance on JACs (state jurisdiction)**		**Reliance on police and PIs (communal jurisdiction)**		**Reliance on police and PIs (state jurisdiction)**	
	(1) Residents	(2) Leaders	(3) Residents	(4) Leaders	(5) Residents	(6) Leaders	(7) Residents	(8) Leaders
Assigned to treatment	-0.031	0.040	0.017	0.042	0.003	0.033	0.009	0.033
	[0.049]	[0.055]	[0.046]	[0.053]	[0.036]	[0.041]	[0.041]	[0.041]
Observations	2673	1182	2673	1182	2673	1182	2673	1182
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Community controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Estimator	AES	AES	AES	AES	AES	AES	AES	AES

Notes: All specifications include individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors, clustered by community, are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.26: **Consensus around dispute resolution, alternate constructions of the DV**

	Consensus around dispute resolution (modal resident)		**Consensus around dispute resolution (modal leader)**	
	(1) Residents	(2) Leaders	(3) Residents	(4) Leaders
Assigned to treatment	-0.049	-0.024	-0.025	-0.040
	[0.035]	[0.038]	[0.040]	[0.030]
Observations	2673	1182	2673	1182
Individual controls	Yes	Yes	Yes	Yes
Community controls	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes
Estimator	AES	AES	AES	AES

Notes: All specifications include individual- and community-level controls, block fixed effects, and inverse probability weights. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.27: **Correlates of missingness in behavioral dependent variables**

	Missing petitions data	**Missing WhatsApp group data**
	(1)	(2)
Assigned to treatment	-0.018	-0.017
	[0.068]	[0.066]
Population	0.000	0.000
	[0.000]	[0.000]
Distance to nearest arterial road (km)	0.001	0.001
	[0.003]	[0.003]
Distance to municipal capital (km)	0.004	0.004
	[0.003]	[0.003]
Observations	149	149
Individual controls	No	No
Community controls	Yes	Yes
Block FE	Yes	Yes
Weights	Yes	Yes
Estimator	OLS	OLS

Notes: Correlates of missingness in costly behavioral measures. All specifications include block fixed effects and inverse probability weights. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$