

Preventing Rebel Resurgence after Civil War: 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 Colombia's *ComunPaz* program, a scalable, inexpensive intervention that sought to replace rebel governance by harnessing complementarities between state and communal authorities and by improving security and justice provision in areas once dominated by FARC, the country's largest rebel group. We find that *ComunPaz* enhanced the quality of local dispute resolution, increased citizens' trust in (some) state institutions, and strengthened coordination between state and communal authorities. It also appears to have 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 from 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 years 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. Governance gaps may also create opportunities for new or existing armed groups to consolidate territorial control. 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. In states transitioning from civil war, “the quality of dispute institutions embodies the quality of local governance more generally,” and providing institutions to resolve disputes is “essential” to “decrease the opportunities for criminal groups to become new de facto rulers in areas formerly ruled by rebels or paramilitaries” (Arjona 2016, 69, 311). Because these states are weak, however, we argue that their most viable strategy for resolving disputes fairly and efficiently is to partner with communal institutions (Baldwin 2015)—locally embedded mechanisms for sustaining order independently of the state, typically through the use of social sanctions. (We provide a more detailed definition of communal institutions below.) Communal institutions often have local legitimacy and access to inside information that states lack; states (even weak ones) often have material resources and coercive capacity that communal institutions lack. We argue that by exploiting these complementarities, states can reduce the risk of local conflict escalation and prevent the reemergence of rebel rule.

We test this argument in Colombia, where the demobilization of the *Fuerzas Armadas Revolucionarias de Colombia* (FARC), the country's largest rebel group, has provided 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 created or coopted a variety of mechanisms for maintaining order in the

territories they controlled, including, crucially, mechanisms of dispute resolution (Arjona 2016; Vargas Castillo 2019). Now that FARC has demobilized, the state must fill the resulting governance gaps 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 these governance gaps by exploiting complementarities between the state and communal institutions. 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 these authorities to build trust with each other, and with citizens; and develop strategies to increase communication and coordination between communal institutions and the state. 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 (Kaplan 2017; Vargas Castillo 2019). FARC and other armed groups tried and in many cases succeeded in coopting JACs to facilitate governance of the communities under their control; most previously coopted JACs continue to function even in communities abandoned by FARC, but without the coercive capacity that FARC once provided. We argue that states can project power more successfully by mimicking this strategy, leveraging relationships with communal institutions to penetrate territories that lie within their *de jure* borders but beyond their *de facto* control. *ComunPaz* sought to implement precisely this approach to statebuilding.

Our sample consists of 149 communities across four rural regions of Colombia where FARC was historically dominant; roughly 81% of residents in our sample report that armed groups controlled their communities at some point in the past. 72 communities were randomly assigned to participate in *ComunPaz*, which was administered in four modules over the course of three months in each treatment community. We evaluate the impact of the program using surveys of residents, JAC leaders, police officers, and Police Inspectors. Unusually for an evaluation of this sort, our survey sample includes both the “supply” and “demand” sides of security and justice provision.

The residents survey also includes endorsement and list experiments aimed at measuring 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 these petitions. We corroborate and contextualize our quantitative results using detailed qualitative field reports from *ComunPaz* facilitators.

Consistent with our theoretical framework and pre-analysis plan (PAP),¹ we find that *ComunPaz* reduced the prevalence of unresolved and violent disputes at the community level as reported by survey respondents. We also find more suggestive evidence that the program diminished both perceptions of and reliance on armed groups among residents. Importantly, we find that reliance on armed groups was already rare in these communities, and perceptions of them already unfavorable. While FARC had largely demobilized by the time of our study, multiple additional armed groups were already competing to supplant FARC’s rule. *ComunPaz* appears to have driven perceptions of and reliance on these other armed groups nearly to zero, potential floor effects notwithstanding. The program also improved perceptions of some state authorities, especially those that have 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. This is especially striking given that our endline was administered approximately seven months after the end of the intervention. 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 existing demand for coordination, as reflected in the large number of petitions filed in all communities, regardless of treatment status. If *ComunPaz* helped satisfy existing demand for state-communal coordination, then it is perhaps unsurprising

¹Our PAP was pre-registered with the Evidence and Governance and Politics network and the Open Science Framework prior to endline data collection. An anonymized version of the PAP is available at https://www.dropbox.com/s/z6lftwsjwdaljm/PAP_anon.pdf?dl=0.

that we find lower demand in treatment communities after the program concluded.

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 their communities, or that it fostered consensus around how disputes should be resolved. We also find no evidence that the program increased reliance on either state or communal authorities to resolve disputes. This is puzzling given the reduced prevalence of unresolved and violent disputes in treatment communities. In the discussion we combine our quantitative and qualitative data to explore several potential explanations for this surprising combination of results.

Taken together, our results suggest that states can prevent a resurgence of local violence, improve their own relations with civilians, and impede the restoration of armed group control by exploiting complementarities with communal institutions—a strategy that armed groups themselves often use to reinforce their local governance capabilities in the territories they control. Patterns of contestation and control during civil war often reshape local institutional configurations in profound and lasting ways, which may in turn affect the prospects for peace. To date, however, few (if any) studies have tested whether or how governments can engage these altered institutional arrangements to promote stability and state consolidation. Our results demonstrate that states can exploit complementarities with communal institutions to extend their authority into areas where they were previously absent. More speculatively, given the ties between JACs and FARC in many Colombian communities, our results suggest that states recovering from conflict may be able to leverage the legacies of rebel governance in the past to prevent renewed rebel control in the future.

Our study contributes to multiple bodies of research. First, we contribute to the literature on rebel governance by exploring mechanisms for preventing armed groups from filling local governance gaps during transitions to national peace (Arjona 2016; Arjona, Kasfir and Mampilly 2015; Cunningham and Loyle 2021; Huang 2017; Mampilly 2011; Revkin and Ahram 2020; Stewart 2018; Steele and Weintraub 2021). Second, we contribute to research on the relationship between

state and communal institutions by exploring ways to leverage their underutilized complementarities (Baldwin 2015; Blair 2019; Bodea and LeBas 2016; Van der Windt et al. 2019). Third, we contribute to studies of security and justice provision in post-conflict countries—most of which focus on strengthening the state while excluding communal institutions, or vice versa (Blair, Karim and Morse 2019; Blattman, Hartman and Blair 2014; Hartman, Blair and Blattman 2021)—by testing mechanisms to empower the state and its communal counterparts simultaneously. Finally, we contribute to the broader literature on peacebuilding and statebuilding by showing how states can benefit from cooperation with communal institutions that are often marginalized during statebuilding processes (Call and Wyeth 2008; Isser 2011; Paris and Sisk 2009).

THEORETICAL FRAMEWORK

COMPLEMENTARITIES BETWEEN STATES AND COMMUNAL INSTITUTIONS

One of the most basic functions of any government is to establish institutions for adjudicating crimes and resolving disputes. These institutions preserve order (Levi 1989), protect property rights (Blattman, Hartman and Blair 2014), reduce transaction costs associated with economic exchange (North 1990), and more generally allow individuals to “live together peacefully and engage in mutually beneficial cooperation” (Arjona 2016, 70). Dispute resolution institutions are especially important in countries suffering or recovering from civil war. Where these institutions are effective and legitimate, they increase citizens’ loyalty to the state, strengthen their incentives to resist rebel encroachment, and improve their capacity to mobilize against rebel rule (Kaplan 2017). As Arjona (2016, 11,72) explains, resistance to rebel incursions is a function of the “quality of the local institutions in place prior to the arrival of the group, in particular, dispute institutions,” since “high-quality dispute institutions deprive armed actors of one of the most effective means to consolidate their power: creating new dispute institutions and becoming the *de facto* administrators of justice for local populations.”

But weak and war-torn states often fail to provide effective, legitimate institutions for re-

solving disputes, and armed groups have succeeded in becoming local “administrators of justice” in a wide variety of settings, including Afghanistan, Sri Lanka, Ethiopia, Nepal, Cuba, Peru, and Sudan, among others. When these groups disarm, they leave behind governance gaps that states must fill in order to facilitate the implementation of peace agreements, prevent local conflicts from escalating into regional or national crises (Autesserre 2010; Blair, Blattman and Hartman 2017), and impede other armed actors from seizing newly abandoned territories (García-Villegas and Espinosa 2015). But filling these gaps is not easy. The same factors that facilitate insurgency also tend to impede the projection of state power after insurgents demobilize. State infrastructure may be dilapidated and state bureaucracies may be dysfunctional, especially in rural areas. 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 and authorities. Communal institutions provide public goods and resolve disputes at the local level while retaining some degree of independence from the state. They tend to be deeply embedded in the communities they govern, and typically rely on social sanctions rather than physical coercion to enforce their decisions (Bowles and Gintis 2002). Crucially, communal authorities are not mere extensions of the state. They are distinct from local police officers, judges, or magistrates, who may also provide public goods and resolve disputes, but who are employed by the state and answerable to higher level officials within the state apparatus. Communal institutions are also distinct from the multitude of civil society groups and organizations that may exist at the local level, but that do not attempt to govern the communities in which they are based (for example, youth groups or sports leagues).

Some communal authorities operate independently of the state but are nonetheless recognized under state law, with constitutionally delineated roles and jurisdictions—for example, lineage chiefs in Mozambique (Lubkemann, Kyed and Garvey 2011) or sheikhs practicing tribal customary law in Iraq (Asfura-Heim 2011). Others—secret societies in Liberia, for example (Blair, Karim and Morse 2019)—are not recognized by the state. Some derive power from their association with specific historical customs, rituals, or lineages, as is the case with elders and *liurai* in

East Timor (Brown and Gusmao 2009); others were elevated by state or colonial rulers but subsequently became powerful in their own right, as is the case with Paramount Chiefs in Sierra Leone (Acemoglu, Reed and Robinson 2014). (We consider the generalizability of our results to different types of communal institutions in the conclusion.) Theoretically, the relationship between state and communal authorities is not inherently antagonistic, though confusion and contestation over jurisdictional boundaries is very common in practice (Isser 2011).

Communal institutions exist in most (if not all) societies. They tend to be locally legitimate and informed about the most important sources of disputes among citizens. During periods of civil war, when the state is weak and threats to security are imminent, communal institutions may help preserve a semblance of local stability in the midst of national upheaval (Isser 2011). During transitions to peace, they can facilitate access to rural communities and relieve the burden on the state by adjudicating non-violent crimes and petty domestic conflicts, which 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).

Of course, communal institutions often suffer from pathologies of their own. 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). While communal institutions may not need third parties to function, their reliance on social sanctions rather than physical coercion makes them more susceptible to shirking, free riding, and forum shopping (Blattman, Hartman and Blair 2014). 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, which may render their decisions unenforceable. Communal authorities may also be corrupt, biased, or ineffective, or may resolve

disputes in ways that contravene state laws and due process protections (Blair 2019; 2020).

We argue that peace processes create windows of opportunity for states to resolve these pathologies and foster symbiotic relationships with communal authorities. During these transitional moments, states have strong incentives to project their authority into previously rebellious territories, especially as the barriers to state penetration erected by formerly dominant rebel groups begin to fall. Transitions from civil war are also conducive to institutional innovations that may be more difficult to achieve once peace is consolidated and path dependence sets in (Call 2007). Communal institutions can in many cases provide the local legitimacy and inside information that states lack, helping to identify conflicts before they escalate and bringing disputants to the bargaining table. States can provide the coercive capacity that communal institutions often lack, ensuring that their decisions are enforceable and consistent with legal rules and procedures. By exploiting these complementarities, we argue that states can project power and increase their legitimacy, 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 demobilized rebels.

THREE OBSTACLES TO LEVERAGING THE COMPLEMENTARITIES

Exploiting the complementarities between states and communal institutions requires overcoming at least three obstacles that often arise in conflict and post-conflict settings: a lack of (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; 2020). 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 and communal authorities may distrust state institutions and may (rightly) fear that wartime patterns of state predation or neglect will

persist. Conversely, in areas previously controlled by armed groups, state authorities may (accurately) perceive their communal counterparts as accessories to rebel rule. Third and related, state and communal systems of dispute resolution may develop in isolation, with few mechanisms to coordinate their activities. Coordination problems may 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. 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 a given grievance. Moreover, the inclusiveness and consensus inherent to much local-level decision-making may leave communal institutions especially vulnerable to shirking, free riding, and dissension 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 in weak and war-torn states. 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 security and justice, 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 aims 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 multiple armed groups vying to fill governance

gaps left in FARC's wake. 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 communities previously controlled by FARC.

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. Police Inspectors are trained in conflict resolution by the central government's Ministry of Justice and Law and are paid by their respective municipal governments, but they are not members of the police force per se. Their roles and responsibilities were defined in Law 23 of 1991, which sought to reduce congestion in the civil and criminal justice systems by assigning tasks that might otherwise be undertaken by the police to Police Inspectors.

Colombian law defines JACs as “non-profit civic associations, made up of neighbors of a given place, who bring together their efforts and resources to address the most important needs of the community” (Decree 1930 of 1978, art.1). The Colombian government authorized the formation of JACs in 1958 following a brutal period of civil conflict known as *La Violencia*. The goal was to foster reconciliation and create a “formal interlocutor between citizens and the state” (Arjona 2016, 231). While the government actively promoted JACs in some areas, in most communities they organized on their own, and in many cases communal institutions with similar functions operated for years before formally constituting themselves as JACs (Vargas Castillo 2019). While JACs are sometimes conceptualized as a local level of government, they “clearly fall in the realm of local civil society,” and historically were “largely left to their own devices” by the state (Kaplan 2017, 14, 80).

Today JACs provide local public goods and resolve local disputes without guidance or interference by the government, typically through a Coexistence and Conciliation Commission (*Comisión de Convivencia y Conciliación*), the unit within JACs that is responsible for (most) conflict resolution. In most cases they are inclusive, with one representative per household in each

community (Vargas Castillo 2019, 83). They tend to enjoy high local legitimacy, are accessible and affordable, and have a presence in almost all rural areas, constituting the “most common form of rural organization” in the country (Kaplan 2017, 14). But they receive little if any training, have limited material resources, and are often unfamiliar with the duties, powers, and constraints imposed on them by Colombian law. They also lack the coercive capacity to enforce potentially unpopular decisions.

During the civil war, JACs developed varied and complex relationships with armed groups (Cubides 2006). In some communities they helped civilians protect themselves from armed group violence (Kaplan 2017); in others they were dismantled by encroaching insurgents (Arjona 2016). Armed groups sought to coopt JACs during the conflict (Jaramillo, Mora and Cubides 1986), and in some cases JACs developed symbiotic relationships with armed groups (Vargas Castillo 2019). This was especially true in communities controlled by FARC—the focus of our study. FARC generally operated as a “co-facilitator of organizational processes” at the community level, using its coercive capacity to support existing communal institutions (Jaramillo, Mora and Cubides 1986, 175). FARC often demanded that all households enroll in JACs, enforced attendance at JAC meetings, and ensured participation in collective labor days convened by JAC leaders. FARC benefited from the presence of locally legitimate institutions capable of resolving disputes and preserving order independently of the state, while JACs benefited from FARC’s coercive capacity, which made their decisions easier to enforce.

Relations between police officers, Police Inspectors, and JACs are beset by the three obstacles of information, trust, and coordination discussed above. As part of our endline survey, we asked JAC leaders seven 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; perceptions were more positive among JAC leaders, but were still unfavorable. Police Inspectors and JACs have 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. These information, trust, and coordination deficits limit the state’s ability to exploit complementarities with JACs in order to project power in areas susceptible to future armed group control.

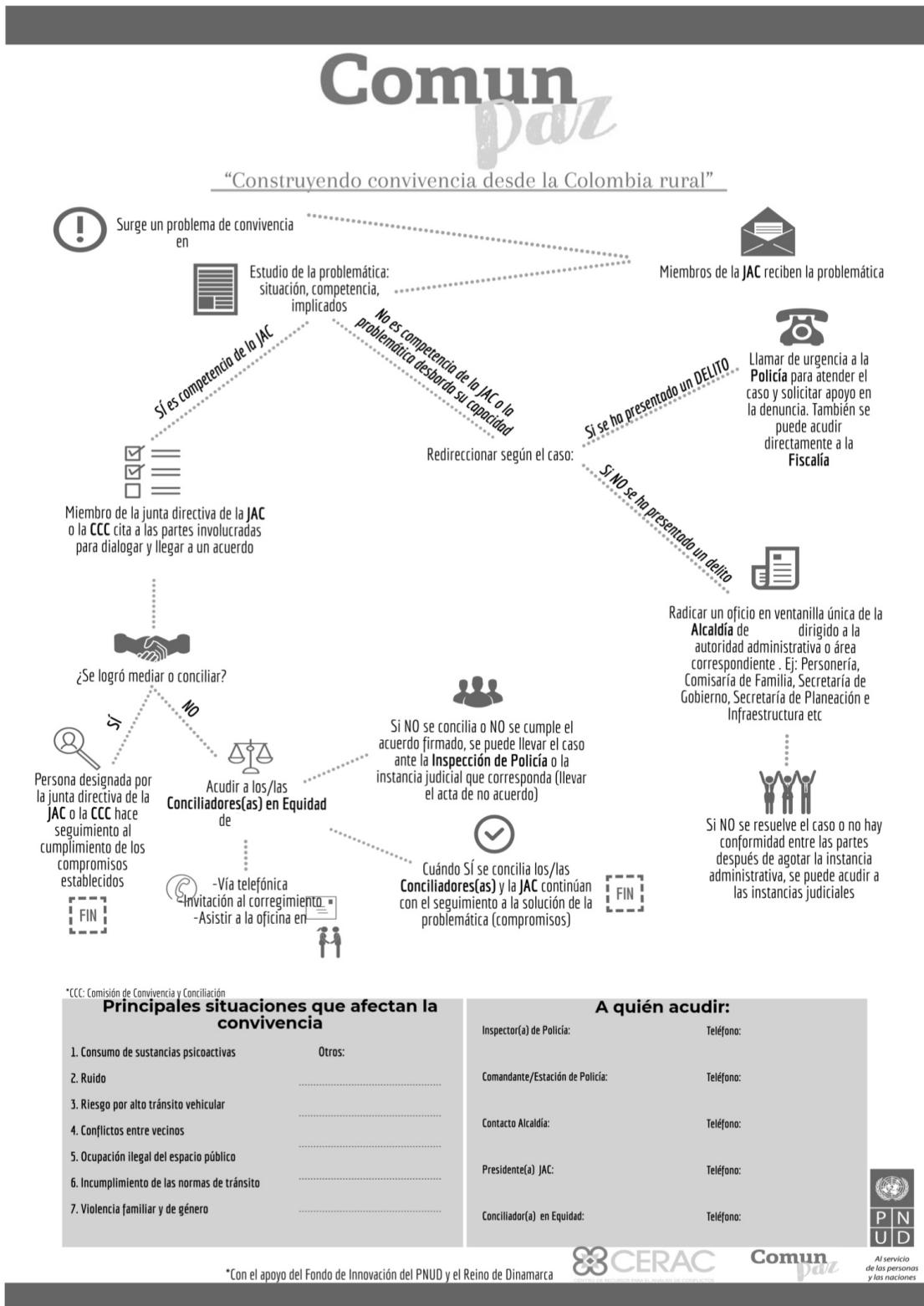
THE *ComunPaz* PROGRAM

We evaluate an inexpensive, scalable intervention designed to induce closer coordination between police officers, Police Inspectors, and JACs, while avoiding the pitfalls of forcibly imposing state laws and institutions on communities long accustomed to autonomy or rebel rule. The program and our evaluation of it 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 program was specifically designed to facilitate state penetration and impede the reemergence of rebel rule in communities abandoned by FARC—though, as we discuss in the conclusion, the goals and structure of the intervention are potentially generalizable to settings without histories of rebel governance.

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 per community, with gaps between sessions to allow residents to adopt new institutional arrangements and put new skills into practice. Module 1 targets police officers and Police Inspectors; Module 2 targets JACs. These modules 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; and locate the most important sources of conflict in the communities under their jurisdictions.

Module 3 targets police officers, Police Inspectors, and JACs, and aims to build trust while helping participants 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 while disseminating information about the proposals developed in Module 3. The program culminates with the promulgation of a *ruta de atención*

Figure 1: An anonymized response route



(“response route”) for resolving disputes, as in Figure 1. Response routes are tailored to each community and then displayed prominently in a central location in each village.

ComunPaz specifically targets the three obstacles to state-communal cooperation discussed above. The program aims to provide information about the roles and responsibilities of state and communal authorities; 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.” The structure of the program was informed by a survey that we administered in 2015 to 100 communities 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 May 2019 by the UN Development Programme (UNDP), the Colombian government’s National Planning Department (DNP), 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.

ComunPaz represents a return to the Colombian government’s strategy when it first authorized the formation of JACs in the 1950s. As originally conceived, JACs embodied an “alternative form of statebuilding:” while the Colombian government could not “quickly and easily increase its capacity,” it could “quickly encourage capacity from the bottom up and then establish links of communication and coordination” with JACs (Kaplan 2017, 80). In reality, these links remained tenuous in most rural Colombian communities, and often ruptured altogether when armed groups seized control. Ironically, some armed groups were more aggressive (and more successful) than the government in coopting JACs during the conflict. In a sense, *ComunPaz* was designed to mimic these armed groups’ strategies; in so doing, however, it aimed to establish precisely the mechanisms of communication and coordination that the government initially believed would facilitate statebuilding in rural communities, especially those that were most severely affected by civil strife. In this way, the program aimed to address the government’s concern that armed groups might

leverage the legacies of rebel rule to undermine statebuilding efforts.

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.² Our primary site selection criterion was historical FARC presence. Within each region we purposively selected five to six municipalities where we were most confident FARC had established uncontested territorial control at some point during the civil war; other armed groups (including ELN and paramilitaries) were historically present as well, and controlled territory within our sample at various points during the conflict. We relied on primary and secondary sources to identify these municipalities, including NGO reports, academic studies, and government analyses, following the procedure described in Appendix A.1.

We excluded municipalities where extremely poor road conditions made it prohibitively expensive to implement the intervention. We also excluded municipalities where ongoing violence or active armed group control would pose a threat to facilitators' or participants' safety. Within selected municipalities we sampled all *centros poblados* ("populated centers") with fewer than 5,000 residents. 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.

Our endline survey suggests that we were successful in identifying former armed group strongholds: as we discuss in further detail in Appendix A.1, 81% of respondents in our sample reported that armed groups had established control over their communities at some point during

²This latter region was somewhat artificially created for our purposes, consisting of municipalities in Centro del Valle within the area of operation of FARC's 46th Front.

the conflict, either by maintaining a continuous physical presence, regulating the entrances and exits to the community, resolving disputes, and/or serving as the “primary authority” in the community.³ The four regions vary along other dimensions, including state presence, political history, and historical settlement patterns, as we discuss in Appendix A.1.

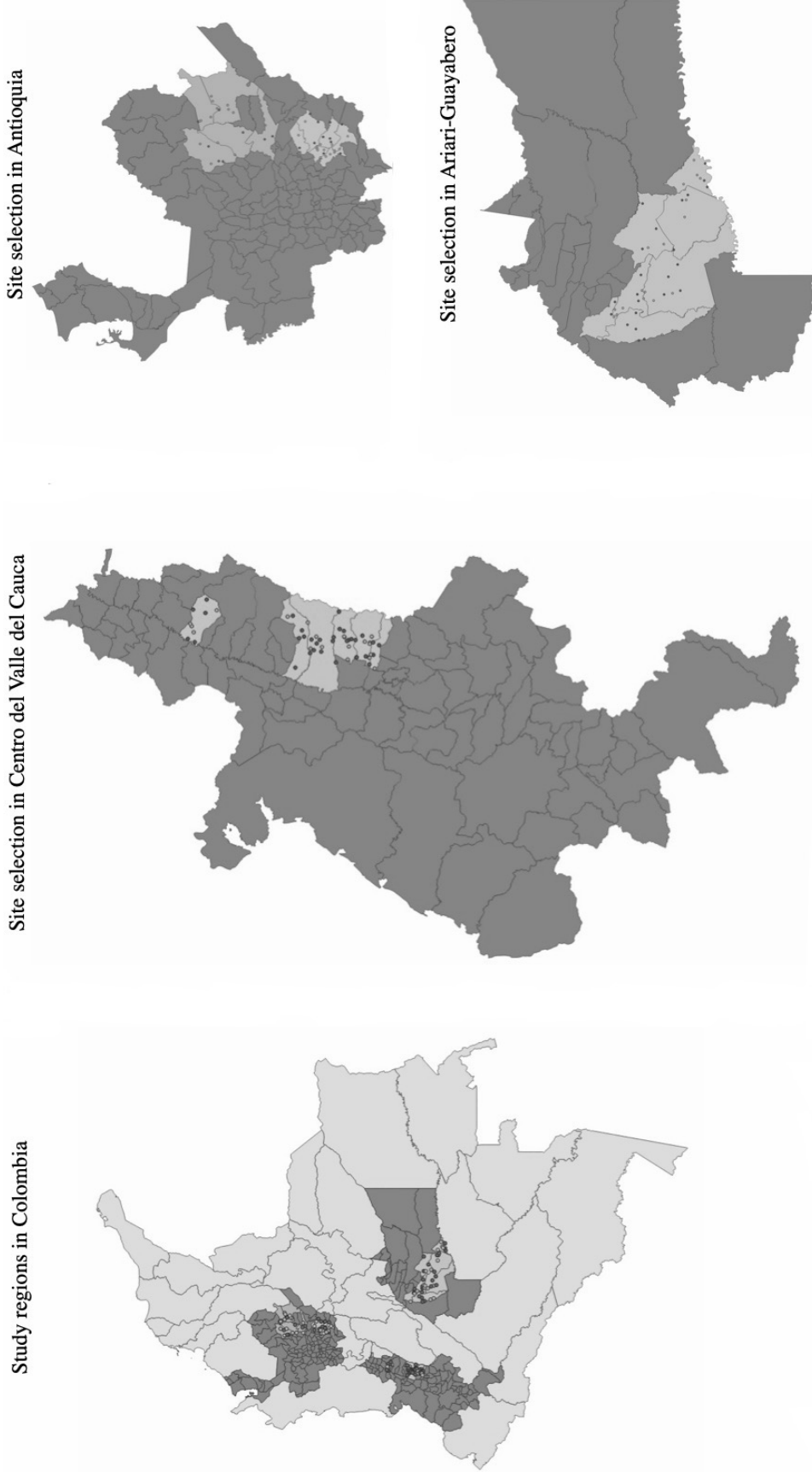
FARC had largely withdrawn or been eliminated from these regions by the time of our study. In some regions, such as Oriente Antioqueño, the government destroyed FARC militarily; in others, such as Ariari-Guayabero, FARC drew down following the signing of the 2016 peace agreement. But other armed groups were present at the time of our study, and while none had established control over the communities in our sample, they had already begun to compete to seize the territories FARC had abandoned. 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 some point during our study, including from ELN, large armed groups linked to drug cartels with (ostensibly) political agendas (e.g., the *Autodefensas Gaitanistas* and *Los Rastrojos*), and smaller groups that are more clearly criminal (e.g., *Los Pachelí*).

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 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. We also randomized the order in which municipalities would be treated. We map the distribution of treatment and control communities in Figure 2. A detailed description of our blocking procedure is in Appendix C.1, balance tests are in Appendix C.3, and manipulation checks are in Appendix E.1.

³For the safety of our respondents we did not ask them to disclose the name(s) of the armed group(s) that controlled their communities. Given that these regions are former FARC strongholds, it is likely that many respondents were referring to FARC.

Figure 2: Site selection



Notes: Dark dots denote control communities, light dots denote treatment communities.

LIMITATIONS

Our study is not without limitations. One is our inability to test for treatment effect heterogeneity along several potentially important dimensions. For example, while previous studies suggest that many JACs were coopted by FARC and became stronger under FARC rule (Vargas Castillo 2019), this was not the case everywhere (Arjona 2016). It is possible that the impact of *ComunPaz* might vary with the strength of JACs prior to implementation, or with the nature of the relationship between JAC leaders and local FARC commanders. It is also possible that the impact might vary with the intensity of citizens' distrust of the state. Unfortunately, we do not have baseline data on these moderators, and so cannot test for heterogeneous treatment effects along these dimensions. We can, however, test for heterogeneity along other potentially salient moderating variables, such as prior exposure to violence and historical strength of rebel and paramilitary rule. We motivate these analyses and report results in Appendix E.9, and we encourage future researchers to explore heterogeneous treatment effects of programs like *ComunPaz* along dimensions that we cannot measure here.

A second limitation is the “bundled” nature of the treatment. *ComunPaz* comprises four modules, and we cannot disentangle the effects of each module in isolation from the others. It is possible that a shorter, simpler program might have similar effects—though, given the initial wariness with which some 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 prone 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

Our theory posits that states can reduce the risk of local conflict escalation and prevent a resurgence of rebel governance by exploiting complementarities with communal institutions, thereby filling local governance gaps and encouraging citizens to seek redress for grievances through the mechanisms that state and communal authorities provide, rather than through armed actors. Following our theoretical framework, in our PAP we hypothesized that *ComunPaz* would reduce the prevalence of (H1) unresolved and (H2) violent disputes and decrease (H3) reliance on armed groups while increasing reliance on (H4) JACs and (H5) police officers and Police Inspectors.⁴

But our theory also identifies three obstacles to exploiting these complementarities: information, trust, and coordination. Information problems arise when citizens do not understand the legally circumscribed division of labor between state and communal institutions, or when state and communal authorities do not understand the most important conflicts affecting citizens' lives. Problems of trust arise when state authorities distrust their communal counterparts, or when citizens and communal authorities trust armed groups more than they trust the state. Coordination problems arise when citizens disagree about how particular conflicts should be resolved, when state and communal authorities prove incapable of harmonizing their activities, or when communal institutions are paralyzed by shirking, dissension, and free riding among their own members.

ComunPaz was designed to overcome precisely these obstacles. Again following our theoretical framework, we hypothesized that the program would 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. The program would mitigate problems of trust by improving perceptions of (M3) JACs and (M4)

⁴We present these hypotheses in a different order here than in our PAP, and 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 E.4, as it proved difficult to test using our survey.

police officers and Police Inspectors while diminishing perceptions of (M5) armed groups. Finally, the program would mitigate coordination problems by increasing (M6) consensus around how disputes should be resolved, (M7) the quality of coordination between JACs, police officers, and Police Inspectors, and (M8) the cohesiveness and functionality of JACs themselves. We expected the program to help resolve disputes and reduce reliance on armed groups through one or a combination of these mechanisms. Figure 3 summarizes how *ComunPaz* activities connect to the three obstacles described above, and to the outcomes we measured in our endline survey.

By supplanting armed groups and providing effective, legitimate alternatives to rebel rule, we expected that, over time, the state could begin (re)establishing territorial control over areas once lost to insurgents (Arjona 2016). Reduced reliance on armed groups, increased reliance on the state, and improved perceptions of state authorities are also important indicators of state consolidation in and of themselves (Blair 2019). Following our PAP, we also 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 on the outcomes most central to the program: the prevalence of unresolved and violent disputes, and reliance on JACs, police officers and Police Inspectors, and armed groups. These results are in Appendix E.9, and are almost uniformly null.

QUANTITATIVE DATA

We test our hypotheses using multiple sources of data. First, we conducted an endline survey roughly seven months after the end of the intervention. We surveyed 18 randomly selected residents and eight 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.

Figure 3: Theoretical framework, program activities, and outcomes

Obstacle	Program activities to overcome obstacle	Outcomes
Information	<p>Modules 1 and 2: state and communal authorities participate in interactive exercises to understand one another's roles and responsibilities and identify common local disputes</p>	<p>Understanding of JACs' authority Understanding of most important disputes</p>
Trust	<p>Module 3: state and communal authorities interact with one another in a secure, structured environment with clear rules of engagement</p> <p>Module 4: citizens interact with communal and state authorities</p>	<p>Perceptions of armed groups Perceptions of JACs Perceptions of police and PIs Reliance on armed groups Reliance on JACs Reliance on police and PIs Approval of policies endorsed by JACs, police, or armed groups</p>
Coordination	<p>Modules 1 and 2: state and communal authorities identify common obstacles to coordination</p> <p>Module 3: state and communal authorities jointly develop response routes for dispute resolution and strategies for overcoming obstacles to coordination</p> <p>Module 4: communities adopt response routes and disseminate information about them</p>	<p>Any unresolved disputes Any violent disputes Consensus around dispute resolution Coordination between JACs, police, and PIs Coordination within JACs Any petitions requesting greater coordination Number of petitions requesting greater coordination WhatsApp group to facilitate greater coordination</p>

Because police commanders and Police Inspectors have jurisdiction over multiple communities, for most questions we provided a list of communities in their jurisdiction and asked about each one individually. The survey was conducted between November and December of 2019 by Proyectamos, a Colombian survey firm. Descriptive statistics and further details on our sampling frame are in Appendix D.

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. The second behavioral measure sought 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, and were also given the name and phone number of the police commander and Police Inspector in their jurisdiction. 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 detailed qualitative field reports compiled by *ComunPaz* facilitators. Facilitators documented any discussions held, questions asked, and outputs produced during each activity, and recorded their own 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 suggestive) evidence regarding mechanisms. Importantly, while qualitative data were collected on the specific dates of the workshops, participants regularly discussed more general processes of conflict and conflict resolution in their communities, yielding insights into mechanisms that emerged throughout implementation.

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 (Clingsmith, Khwaja and Kremer 2009).⁵ 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 Instituto Agustín Codazzi and the planning office of each municipality. We estimate the intention-to-treat effect (ITT) on all outcomes.

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. We report results with multiple comparisons corrections in Appendix E.2.

⁵The AES across J related dependent variables is $\tau = \frac{1}{J} \sum_{j=1}^J \frac{\pi_j}{\sigma_j}$, 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. 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.

POTENTIAL THREATS TO INFERENCE

SPILLOVER

Our analysis is potentially susceptible to three types of spillover. First, dispute resolution strategies and information conveyed to participants during the 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. In any event, spillover of this kind strikes us as unlikely, given that the average treatment community is located more than 8 kilometers from the nearest control community—a long way in rural Colombia, where roads are rough and often impassable, and where few residents own cars.

Second, since the jurisdictions of police officers and Police Inspectors encompass both treatment and control communities, it is possible that the benefits of improved performance among these state authorities will spill over from the treatment group to the control group. Again, this would bias our ITT estimates towards the null. Spillover of this kind strikes us as unlikely as well: 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 remote rural areas, and the amount of effort that could plausibly be redirected is small. Floor effects likely would mitigate the risk of negative spillover of this kind.

NON-COMPLIANCE

Participation in the *ComunPaz* program was voluntary, and was therefore susceptible to one-sided non-compliance. We took several precautions to minimize this problem, including sending individually addressed letters about the program from the Colombian government’s National Planning Department (DNP); visiting each municipal capital to coordinate timing and logistics with municipal authorities; and visiting each treatment community to coordinate timing and logistics with JACs as well. (We were especially concerned with maximizing participation among JACs, who have agency of their own, and who were empowered to decline to participate.) We also sent a letter from the DNP to the Planning Director of the Colombia National Police, who then sent individually addressed letters authorizing the participation of the police commanders in our sample.

Even with these precautions, some non-compliance did occur, as we discuss in detail in Appendix C.2. Fortunately, there are only three treatment communities that we can classify as “full” non-compliers, in that 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 include both full and partial non-compliers in the treatment group for purposes of our ITT analyses. This should 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 approach yields CACE estimates that are substantively different from the ITT. For compactness, we report the ITT alone.

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 [0.033]	-0.093** [0.041]	0.001 [0.010]	-0.051* [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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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 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. Residents were also asked if they or a family member had been the victim of an assault. We code dummies for any unresolved or violent disputes at the household (columns 1 and 3) and community (columns 2 and 4) level.⁶

⁶In 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 view the separate results as more informative.

We find that the program reduced the prevalence of both unresolved and violent disputes at the community (columns 2 and 4) but not the household level (columns 1 and 3), as reported in our survey. 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 9.3 percentage points less likely to report an unresolved dispute in their community, a reduction of 16.1% relative to the control group mean (0.579). Residents were 2.7 percentage points less likely to report an unresolved dispute involving a household member—a statistically insignificant reduction of 8.0% relative to the control group mean (0.338). Treatment group leaders were also 5.1 percentage points less likely to report a violent dispute in their community. While this effect is not quite statistically significant at the 95% level ($p = 0.053$), it represents a substantively large 25.1% reduction relative to the control group mean (0.203). Residents in the treatment group were no less likely to report a violent dispute, 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.

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

Table 2 reports the ITT on residents' and leaders' 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 then asked to which authority (if any) they would report the incident first, and which authority should be responsible for providing a “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 in Table 1, respondents were asked if they or a household member had approached an armed group for help. Residents were also asked if they or a household member had been victim of any of seven crimes in the past six months—

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** [0.027]	-0.006 [0.036]	-0.028 [0.051]	-0.043 [0.058]	-0.028 [0.055]	-0.049 [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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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.⁷

Third, respondents were read another four hypothetical scenarios of crime and conflict and asked if they believed other community members would seek assistance from an armed group. We code a dummy for respondents who believed they would. Finally, respondents were asked if they believed other community members were in the habit of reporting actual disputes to armed groups. We code a dummy for respondents who agreed or strongly agree that they were. By eliciting beliefs about other community members, 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 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 27.3% reduction in the belief that *other* community members would seek the help of armed groups

⁷To 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.

in hypothetical scenarios, and a 48.8% reduction in the belief that *other* community members reported actual disputes to armed groups. These measures are less susceptible to social desirability concerns. In Appendix E.3 we test whether the program also reduced residents' reliance on armed groups in a list experiment. We find no evidence that it did, though this analysis is underpowered. We also find no evidence of social desirability bias in residents' reports of reliance on armed groups when comparing the survey to the list experiment, lending additional credence to the results in Table 2.

Importantly, reliance on armed groups was rare in the control group, ranging from less than 1% (the proportion of control group residents who had solicited help from an armed group to resolve an actual dispute) to 6.2% (the proportion who believed other community members would solicit armed group assistance in hypothetical disputes). Reliance on armed groups was no more common in the list experiment. This is informative in itself, suggesting that rebel governance in these communities was already weak at endline. It is also unsurprising, as *ComunPaz* targeted communities where FARC had demobilized but where rival armed groups had not yet established territorial control. Given that reliance on armed groups was so rare in our sample, we interpret the ITT in column 1 of Table 2 somewhat cautiously. Nonetheless, our results suggest that *ComunPaz* reduced already 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 (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 disputes in the communities under their jurisdiction. To measure understanding of JACs' authority, residents and leaders were asked seven factual questions about the types of cases JACs

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	0.014 [0.034]	0.010 [0.031]	-0.026 [0.039]	0.007 [0.041]	0.040 [0.041]
Observations	2673	1182	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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

are and are not authorized to adjudicate, ranging in severity from noise complaints to domestic violence. We code dummies for correct answers to each of these questions.

To measure understanding of the most serious disputes, residents were first asked which of 12 potential sources of disputes they viewed as most important in their community. We take the modal response across the 12 randomly selected residents of each community. Leaders, police commanders, and Police Inspectors were asked the same question about each community under their jurisdiction. We code dummies indicating whether leaders', police commanders', and Police Inspectors' responses match the modal response among residents. This is a hard test, since leaders, police commanders, and Police Inspectors may be aware of residents' assessments of the most serious disputes in their communities, but may disagree with those assessments. In this case we will underestimate the ITT on understanding of the most serious disputes.

We find no evidence that the program increased understanding of JACs' authority under Colombian law, or that it increased understanding of the most serious disputes. Importantly, there is much agreement among residents and leaders about the most important sources of disputes in their communities, though the extent of this agreement is no greater in the treatment group

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

	Perceptions of armed groups		Perceptions of JACs			Perceptions of police and PIs	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Residents	Leaders	Residents	Police	PIs	Residents	Leaders
Assigned to treatment	-0.083** [0.037]	-0.074 [0.046]	-0.023 [0.062]	0.012 [0.074]	0.187** [0.085]	0.105* [0.064]	0.068 [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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

than in the control group.⁸ In other words, we find no evidence to suggest that either state or communal authorities gained a better understanding of the communities under their jurisdiction due to *ComunPaz*.

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, respondents were asked whether other members of the community view armed groups as fair and effective in resolving disputes. We code a dummy for respondents who agreed or strongly agreed that they do. Respondents were also asked whether they themselves believe armed groups resolve disputes fairly and effectively, are trustworthy, and understand the problems afflicting the community. We code dummies for respondents who reported holding these beliefs somewhat, quite a bit, or a lot. Respondents were asked these latter three questions about JACs, police officers, and

⁸Across communities, the first and second most serious disputes according to both residents and leaders were improper garbage disposal and public drug consumption. Police commanders identified noise complaints, public drug consumption, and rumors as the most serious; Police Inspectors identified contested land boundaries and noise complaints.

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 those that said they do. While this latter analysis was not pre-specified, it follows immediately from our theoretical framework. 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. In this sense, we implicitly assume that police commanders and Police Inspectors cannot trust JACs with whom they have never interacted.⁹

We find that *ComunPaz* diminished residents' perceptions of armed groups. The negative and statistically significant AES among residents is driven by a 1.5 percentage point (45.8%) reduction in trust in armed groups, a 1.7 percentage point (42.0%) reduction in the perception that armed groups resolve disputes fairly and effectively, and a 2.6 percentage point (37.2%) reduction in the belief that other community members perceived armed groups as fair and effective. The program diminished leaders' perceptions of armed groups as well, though this effect falls just short of (weak) statistical significance at conventional levels ($p = 0.106$). It is, however, similar in magnitude to the effect among residents.

Again, we interpret these results somewhat cautiously, since favorable perceptions of armed groups were rare in our sample. Only 3.3% of residents and 3.8% of leaders in the control group reported trusting armed groups; only 3.9% of residents and 4.2% of leaders said they believe armed groups understand the problems afflicting their communities; only 3.7% of residents and 3.6% of leaders said they believe armed groups resolve disputes fairly and effectively; and only 6.9% of residents and 4.1% of leaders said they believe other community members would agree with this assessment. While taking care not to over-interpret treatment effects on outcomes this rare, our results in Table 4 suggest that rebel groups were already unwelcome in our sample, and that *ComunPaz* made them even more so, potential floor effects notwithstanding.

⁹Restricting this analysis to JACs that had interacted with police commanders and Police Inspectors would induce bias, since these interactions are partly a function of treatment.

We also find that the program (weakly) improved perceptions of police officers and Police Inspectors in the residents survey—a result driven by an improvement in perceptions of Police Inspectors in particular. Treatment group residents were 5.7 percentage points (15.7%) more likely to express trust in Police Inspectors, and 6.4 percentage points (17.9%) 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, but is smaller than the AES among residents, and is imprecisely estimated.

We find no evidence that the program improved perceptions of JACs among either residents or police commanders. The program did, however, improve perceptions of JACs among Police Inspectors, who were 18.7 percentage points more likely to express trust in JACs in the treatment group—a substantively large and statistically significant 63.8% increase over the control group mean (0.293). A possible explanation for this discrepancy between police commanders and Police Inspectors is that Police Inspectors generally have more contact with rural Colombian communities than police commanders do. Police Inspectors are also more likely to cooperate with JACs to resolve disputes. This may also help explain why we observe a more pronounced change in perceptions of Police Inspectors among leaders and (especially) residents.

Table 5 reports the ITT on perceptions of armed groups, JACs, and the police using an endorsement experiment. Residents were asked to consider three potential policies: a ban on public alcohol consumption; a mandate that motorbike riders wear helmets; and restrictions on the passage of pack animals through the communities. The policies were endorsed by one of three authorities: armed groups, the police,¹⁰ or JACs. Endorsement was randomly assigned at the respondent level, such that the same authority endorsed all three policies. A control group received the same three policies without an endorsement. Respondents were then asked how much they would support each policy. We aggregate responses into a standardized additive index and estimate the effect of *ComunPaz* by interacting assignment to the program with assignment to each of the

¹⁰For reasons of statistical power we did not distinguish between endorsement by the police and endorsement by Police Inspectors in the survey.

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

	Index of approval
Assigned to treatment	-0.134 [0.084]
JAC endorsement	-0.087 [0.061]
Police endorsement	-0.092 [0.061]
Armed group endorsement	-0.961*** [0.133]
Assigned to treatment × JAC endorsement	0.154* [0.087]
Assigned to treatment × police endorsement	0.204** [0.080]
Assigned to treatment × armed group endorsement	0.051 [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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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

	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	-0.031 [0.036]	0.004 [0.036]	0.092 [0.056]	0.028 [0.129]	0.249** [0.114]	0.153** [0.062]
Observations	2673	1182	1182	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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

three endorsements. We report marginal effects from the endorsement experiment graphically in Appendix E.8.

Consistent with our results in Table 4, we find that *ComunPaz* increased support for policies endorsed by the police. It also (weakly) increased support for policies endorsed by JACs. We find no evidence that the program decreased support for policies endorsed by armed groups, though this may be an artifact of floor effects. A plurality of control group respondents expressed the highest possible level of support for all three policies when they were endorsed by either the JAC or the police. In contrast, a plurality expressed the *lowest* possible level of support when policies were endorsed by an armed group. This is consistent with Table 4, and helps explain the substantively large and highly statistically significant negative effect of the armed group endorsement in the control group. This also helps explain the null when we interact assignment to *ComunPaz* with the armed group endorsement.

MORE COORDINATION BETWEEN AND WITHIN GOVERNMENTAL AND COMMUNAL INSTITUTIONS

Table 6 reports the ITT on consensus around dispute resolution (columns 1 and 2), coordination between JACs, police officers, and Police Inspectors (columns 3–5), and the cohesiveness and functionality of JACs (column 6). To measure consensus around dispute resolution, we use the same four hypothetical scenarios of conflict and crime that we used to measure reliance on armed groups. For each hypothetical scenario, respondents were asked which authority they would report to first, and which they believed should provide a “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 number for a police commander or Police Inspector stored on their phone; whether they believed coordination between JACs, police officers, and Police Inspectors improves dispute resolution; whether they believed police officers and Police Inspectors “actively” support the JAC; and whether the JAC 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 number for a JAC member from each treatment and control community stored on their phones; whether each JAC had contacted them for help with dispute resolution in the past six months; and whether they had visited each community in the past six months. (The endline was conducted more than seven months after the end of the intervention.) We code dummies for affirmative answers to each of these questions. Finally, to measure the cohesiveness and functionality of JACs, leaders were asked whether 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

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** [0.089]	-1.139 [0.725]	-0.038 [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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

the third.

We find no evidence that the program increased consensus around dispute resolution. We find suggestive evidence that the program increased coordination between state and communal authorities according to leaders, though this result is only weakly statistically significant and is sensitive to specification.¹¹ While the program did not increase coordination according to police officers, it did increase coordination according to Police Inspectors. This is consistent with Table 4, and again suggests that the program was more effective in improving relations and encouraging coordination with Police Inspectors, perhaps because they have more contact with communities than police officers do. We find that the program improved coordination within JACs as well, in particular by creating an atmosphere 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. Because the distribution of this variable is highly skewed,¹² we also code a dummy for *any* petitions signed.

¹¹In our PAP we pre-specified that leaders would also be asked whether they know the name of a police officer or Police Inspector. Because *ComunPaz* participants exchanged names during the workshops, we believe this is more appropriately interpreted as a manipulation check. Interestingly, 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 more than 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.

¹²The number of petitions signed ranges from a low of 0 (36.8% of communities) to a high of 18 (1.7% of communi-

We measure actual coordination with a dummy for leaders who reported forming a WhatsApp group with police officers or Police Inspectors in their jurisdiction.

These measures suffer from missing data. When we attempted to contact JAC leaders a week after the survey was complete, many were unreachable. As a result, we are missing data on petitions and WhatsApp groups for 32 of the 149 communities in our sample. Fortunately, missingness is not statistically significantly correlated with treatment,¹³ or with any of our community-level controls, mitigating bias concerns. Interestingly, we find that, if anything, residents of treatment communities were *less* likely to sign petitions requesting greater coordination between state and communal authorities. 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 26.7% and 28.8%, respectively, relative to the control group means (0.72 and 4.27).

One possible explanation for this finding is that demand for coordination between state and communal authorities was already high in our sample, and that *ComunPaz* helped meet this demand in the treatment group. Consistent with this interpretation, 63.2% of all communities received at least one petition, with a mean of 3.64 petitions per community. If the intervention helped satisfy demand by improving coordination between state and communal authorities—as Table 6 suggests—then it is unsurprising that demand was lower in the treatment group after the intervention was complete. Fewer communities (20.6%) established WhatsApp groups, and we find no evidence that treatment communities established them at a higher (or lower) rate than control communities.

DISCUSSION

We find that *ComunPaz* reduced the prevalence of unresolved and violent disputes at the community level and diminished residents' already limited reliance on armed groups. These results are

¹³We 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.

consistent with our theoretical framework, and our PAP. Contrary to our expectations, however, we find no evidence that the program induced reliance on either state or communal authorities. This is especially surprising given our finding that the program reduced the prevalence of unresolved and violent disputes. If more disputes are being resolved in treatment communities, but neither state authorities nor communal institutions nor armed groups are resolving them, then who is?

While we cannot answer this question definitively, we can combine our quantitative and qualitative data to explore three potential explanations. (These analyses were not pre-specified, and should be interpreted as exploratory. The three potential explanations are neither exhaustive nor mutually exclusive.) First, *ComunPaz* may have increased participants' ability to resolve conflicts through direct dialogue, without recourse to a third party. Our qualitative data indicates that participants identified direct dialogue as an important first step in dispute resolution in nearly one-third (20) of treatment communities. Our quantitative data similarly suggests that roughly one-third of respondents viewed direct dialogue as the most appropriate response in hypothetical scenarios of crime and conflict. Our quantitative data also suggests, however, that much larger proportions of respondents preferred reliance on either state or communal authorities. Moreover, as we show in Appendix E.5, direct dialogue does not appear to be more common in treatment communities than in control communities, and if anything may be less so. Taken together, these results suggest that an increase in direct dialogue is unlikely to explain the reduction in unresolved and violent disputes.

Second, *ComunPaz* may have created response routes so complex that they did not lend themselves to reliance on any particular authority. From our qualitative data, the modal response route involved three different authorities and seven different mechanisms of dispute resolution. (Crucially, for all their complexity, *ComunPaz* response routes never included rebel groups.) This complexity is likely attributable to the structure of authority already in place in treatment communities, which facilitators did not seek to alter, rather than to changes induced by the program. One potential observable implication of this explanation is that treatment group respondents should be more likely to rely on multiple authorities to resolve any given dispute (i.e. "forum shopping").

From our quantitative data, however, a minority of residents (8%) reported relying on more than one authority; the proportion of leaders who reported relying on multiple authorities was larger (28%), but still relatively small. Moreover, as we show in Appendix E.6, reliance on multiple authorities does not appear to be more common in the treatment group, suggesting that response route complexity is unlikely to explain the null effects on reliance on police officers, Police Inspectors, and JACs.

Third, *ComunPaz* may have enabled better coexistence and cohabitation (“*convivencia*”) among residents, thus mitigating the risk that conflict would occur in the first place. As our qualitative data illustrates, facilitators explicitly encouraged participants to identify the most important sources of disputes in their communities, and to avoid behaviors that might provoke conflict with neighbors. In at least three treatment communities, facilitators helped participants resolve disputes during the intervention itself. A potential observable implication of this explanation is that treatment group respondents should be more likely to report the absence of significant sources of disputes in their communities. Appendix E.7 confirms that treatment group residents and leaders were, respectively, 2.9 and 2.6 percentage points more likely to report an absence of disputes. While these ITT estimates are not quite statistically significant at conventional levels ($p = 0.165$ and $p = 0.133$, respectively), they constitute substantively large increases of roughly 50% and 99% relative to their respective control group means (0.058 and 0.029). This may help explain why the reduction in unresolved and violent disputes was not accompanied by increased reliance on either state or communal authorities.

CONCLUSION

Fair, efficient dispute resolution is key to lowering the risk of conflict escalation and impeding the reemergence of rebel governance in countries recovering from civil war. We argue that dispute resolution can be made more effective by leveraging complementarities between state and communal authorities. During civil war, armed groups often coopt communal institutions to help govern the

territories they control (what Arjona 2016 describes as “aliocracy”). We argue that states can use a similar strategy to prevent rebel resurgence once civil war ends. We test our theory through an experimental evaluation of the *ComunPaz* program in Colombia.

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 state and communal institutions. We also find that the program reduced citizens’ trust in, and reliance on, armed groups (albeit without increasing reliance on either state or communal authorities). These results are striking given that the intervention was short (approximately 10 hours per community) and inexpensive (approximately \$1,000 USD per community). They are also striking given that in many Colombian communities, JACs were incorporated into rebel governance arrangements at the local level (Vargas Castillo 2019). While speculative, this suggests that governments may in some cases be able to leverage the legacies of rebel governance in the past to prevent renewed rebel governance in the future.

While we cannot say if these effects will persist over time, the endline was administered roughly seven months after the program was complete, suggesting the possibility of lasting change. And while we cannot know for certain how our results will generalize to other settings, we do not believe the scope of our findings is specific to the particular dynamics of violence and rebel governance in our sample. These dynamics are not unique to these particular regions of Colombia, or to Colombia as a whole (Arjona 2016). Nor do we believe the scope is limited to strong states (the Colombian state is stronger than many conflict and post-conflict states, but remains weak in rural areas), or to countries in which peace is already consolidated (despite the 2016 peace agreement, the security situation in Colombia remains volatile). Moreover, while JACs are recognized under Colombian law, we see no reason to expect formal legal recognition to be a necessary condition for the success of the program.

Two important scope conditions are, however, implicit in our theory. First, our theory (and the program itself) hinges on the existence of potential complementarities between state and communal institutions. States benefit from the local legitimacy and inside information that communal

institutions can provide; communal institutions benefit from the resources and coercive capacity that states can provide. If communal institutions are so ineffective and illegitimate that they have no comparative advantages relative to the state—or, conversely, if the state is so weak and resource constrained that it has no comparative advantages relative to communal institutions—then interventions like *ComunPaz* are unlikely to succeed. Second, our theory depends on windows of opportunity for the state to expand into territories from which it was previously absent. Programs like *ComunPaz* are unlikely to succeed in places wracked by violence at the time of implementation, or that are already controlled by rebel groups. In these settings, the window of opportunity for consolidating state authority likely has already closed.

But these scope conditions are not overly restrictive. Indeed, we speculate that the benefits of interventions like *ComunPaz* may even extend to contexts without histories of civil war or rebel rule. We focus on areas that were previously governed by rebel groups, since these are likely to be of especially urgent concern to statebuilders seeking to consolidate power and prevent rebel resurgence. But communal institutions also exist in areas where rebel groups never sought or achieved territorial control in the past; indeed, they exist in many countries that never experienced civil war at all, especially those with relatively weak and resource constrained states (Baldwin 2015). Programs like *ComunPaz* could, in theory, improve perceptions of the state and reduce the prevalence of unresolved and violent disputes in these settings as well. We leave this question for future research to explore.

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A INTERVENTION

A.1 STUDY SITES

The *ComunPaz* program targeted rural municipalities with strong historic FARC presence, but where ongoing violence or armed group control would not pose a threat to facilitators' or participants' safety. To select our study sites, we began by dividing the country into 77 regions, following the “*subregionalización*” framework proposed by the National Center for Historic Memory. We then identified regions with strong historic FARC presence using a combination of primary and secondary data sources, including quantitative data from our baseline survey, qualitative data collected by the authors during fieldwork for unrelated studies, the existing academic literature on the geography of the Colombian civil war, and our own knowledge of the case gleaned from many years of living and working in the country.

In consultation with our implementing partners in the UN Development Programme (UNDP), the Colombian government's National Planning Department (DNP), and the Conflict Analysis Resource Center (CERAC), we selected four regions that we were confident had strong historic FARC presence, but that otherwise varied along potentially important dimensions, including state presence, political history, and prior settlement patterns. Oriente Antioqueño and Valle del Cauca exhibit relatively high levels of state presence as measured through the Myers score index (Lee 2016), while Ariari-Guayabero and Nordeste Antioqueño exhibit medium to low levels. Nordeste Antioqueño and Ariari-Guayabero were Colombian Communist Party strongholds, while Oriente Antioqueño and Valle del Cauca have long been dominated by the Liberal and Conservative parties. Communist Party strongholds tend to have more robust communal institutions, as do areas that were more recently settled—such as Ariari-Guayabero—especially relative to those like Oriente Antioqueño, which were colonized during the 19th century. These regions were also of interest to our implementing partners, and fell within reach of UNDP's regional offices.

Within each region we then purposively selected five to six municipalities. We focused

on municipalities where we were most confident that FARC had established uncontested territorial control at some point during the civil war, again relying on a combination of primary and secondary sources. We excluded municipalities that were fully controlled by an armed group at the time of our study; that were sites of ongoing violence; or that had such poor road conditions that they were impossible for us and our implementing partners to access. This procedure yielded a list of 24 purposefully selected municipalities comprising a total of 240 “populated centers” (*centros poblados*). We cross-checked this list against information from municipal administrators, and later with Google Maps. This process resulted in a sampling frame of 162 populated centers whose existence and location we were able to confirm. In order to reduce heterogeneity in cluster size, and because the intervention was designed to target rural communities, we discarded urban centers with more than 5,000 residents. This left us with a list of 149 eligible populated centers.

Importantly, while all of the municipalities in our sample had strong historic FARC presence, the degree of prior rebel control varies across communities, and other armed groups were present as well. 80% of respondents in our endline survey reported that there were times during the conflict when an armed group (either a guerilla or paramilitary group) established a continuous physical presence in and around their community; 68% reported that there were times when an armed group controlled the entrances and exits to the community; 50% reported that there were times when residents relied on armed groups to resolve disputes; and 71% reported that there were times when an armed group constituted the “primary authority” in the community. Aggregating responses to these four questions, 81% of respondents reported that there were times when an armed group established some form of control over their community.¹

Our qualitative data suggests that 23 of our 72 treatment communities also experienced active armed group presence at some point during our study. Armed group presence was most pervasive in the region of Nordeste Antioqueño, where ELN had already begun to make incursions into communities previously controlled by FARC’s 36th front.² In other regions as well, communities

¹Recall that we sampled 26 respondents per community: 18 residents and eight JAC leaders. In 58 communities, *all* 26 respondents reported that armed groups established some form of control at some point during the conflict. There were only four communities in which *no* respondent reported armed group control.

²Community #36, Module 2 (November 27, 2018); Community #29, Module 2 (December 15, 2018).

reported the presence of large armed groups linked to drug cartels with (ostensibly) political agendas (e.g., the *Autodefensas Gaitanistas* and *Los Rastrojos*),³ smaller groups that are more clearly criminal (e.g., *Los Pacheli*),⁴ and FARC *disidencias*.⁵ (Unfortunately we do not have qualitative data on active armed group presence in control communities.)

A.2 STRUCTURE AND TIMELINE OF THE *ComunPaz* PROGRAM

ComunPaz was implemented over eight months between October 2018 and May 2019. The program consisted of four modules, each lasting a day and staggered over the course of three months in each treatment community, with one week between Modules 1 and 2, seven weeks between Modules 2 and 3, and four weeks between Modules 3 and 4. (Communities were treated at different times, which is why the intervention as a whole lasted eight months.)

Module 1 consisted of a one-day workshop targeting police commanders and Police Inspectors, with one workshop per region. Participants were first asked to describe their own personal experiences in the field and their own understanding of their professional roles and responsibilities. They were then divided into groups and provided with flashcards listing the legal functions associated with state and communal authorities. They affixed these flashcards to posters on the wall, each with the name of one authority at the top. Facilitators helped participants correct mistakes and misperceptions. Next, participants listed the most common conflicts in their municipalities, then mapped the steps they typically take to adjudicate them. Facilitators helped participants identify procedures that deviate from Colombian law and encouraged them to consider ways to integrate JACs into previously established “response routes.”

Module 2 targeted JACs, with one workshop per treatment community. Participants were asked to identify the most common conflicts in their communities, the methods they use to resolve those conflicts, and the challenges they face in reaching and enforcing resolutions. Participants were then given flashcards listing a variety of potential mechanisms for dispute resolution and

³Community #39, Module 2 (December 7, 2018).

⁴Community #44, Module 2 (November 16, 2018).

⁵Ariari-Guayabero, Municipality #5, Module 3 (February 26, 2019).

asked to construct a complete response route for specific hypothetical conflicts. Facilitators helped participants identify any discrepancies between their own strategies for resolving disputes and the strategies available to them under Colombian law. Facilitators also encouraged participants to consider ways to improve the services they provide to residents, including through collaboration with state authorities.

Drawing on the challenges and opportunities identified in the previous two modules, Module 3 aimed to increase the degree of coordination between state and communal authorities. 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 commanders and Police Inspectors. Facilitators brought the information 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 proposals for collaborating more directly with one another, within the bounds of Colombian law. Importantly, facilitators did not assume the existence of a single “correct” way to induce cooperation between the state and communal institutions, and allowed these proposals to vary across municipalities and communities.

Module 4 sought to inform the population of each treatment community about the agreements reached in Module 3 and further cement nascent mechanisms of communication and cooperation between JACs, police officers, and Police Inspectors. The module included both formal and pedagogical components, including a discussion with residents about the contents of the new agreements, as well as a “signing ceremony” at which the agreements were officially validated by both state and communal authorities. Following the conclusion of Module 4, facilitators followed up with Police Inspectors and JAC leaders multiple times in person and by phone to reinforce the messages of the workshops and encourage participants to adhere to any commitments made during the intervention (e.g. pledges of greater communication and coordination, often in relation to specific issues or disputes).

B ETHICS

We were interested in evaluating the *ComunPaz* program because we believed it might improve the welfare of rural Colombian citizens by creating fairer, more efficient mechanisms for resolving disputes. We also believed the program might help the government meet its urgent policy goal of extending state authority into rural areas without alienating civilians or marginalizing communal institutions, which are often sidelined in peacebuilding and statebuilding processes (Isser 2011). But we were also aware of possible risks and adverse unintended consequences, which we attempted to mitigate from the outset.

We sought to mitigate four risks in particular: first, that armed groups would target communal authorities with threats or violence if they were perceived to be “informants” due to their increased contact and coordination with state authorities (especially the police); second, that armed groups would target state authorities as they traveled to and from communities to participate in the program; third, that armed groups would perceive the program itself as a threat to their operations, and would retaliate against facilitators or participants; and fourth, that armed groups would misidentify survey enumerators as government officials, or survey respondents as government collaborators, and retaliate against them.

In consultation with our partners, CERAC and UNDP, we took precautions to mitigate or eliminate these risks. Most important, we excluded from our sampling frame municipalities and communities where an armed group had established firm territorial control, or where multiple armed groups were actively engaged in violence with one another. To assess the extent and nature of armed group activity, we consulted the DNP, the Colombian National Police, and other experts, including *ComunPaz* facilitators themselves, many of whom were familiar with the regions in our sample. We also sought the advice of Proyectamos, a local survey firm that assisted with endline data collection and that had other ongoing data collection efforts in our study regions. Finally, we monitored regional and national media for reports of armed group activity. Whenever possible, *ComunPaz* facilitators travelled to municipal capitals to collect more detailed, localized informa-

tion from municipal authorities about the security situation at the community level. Communities experiencing active armed group control or active armed group violence were removed from the sample.

Since communities themselves are the most reliable source of information on local security conditions, 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. (Importantly, *either* police officers *or* Police Inspectors participated in these modules in all communities.) State 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 state or communal authorities to participate in the program.

We also took multiple precautions to protect survey enumerators and respondents. These precautions were developed in collaboration with Proyectamos, and were published in a detailed manual that supervisors followed during enumerator training and survey administration. Supervisors were required to consult local leaders to gauge the security situation in each community prior to data collection. Enumerators were required to make contact with their supervisors at the beginning and end of each work day. Enumerators also carried ID cards, hats, and vests that clearly indicated their affiliation with a survey firm dedicated to academic research. Enumerators were instructed to avoid taking photographs (which might arouse suspicion), to administer the survey between the hours of 6:00am and 6:00pm, and to spend as little time as possible in each community. To protect respondents, all surveys were conducted in private, and no personally identifiable information was collected. Respondents were informed of the potential risks and benefits of participating in the survey, and were repeatedly reminded of their right to skip any question or discontinue their participation altogether without suffering any adverse consequences. No threats to the security of facilitators, participants, supervisors, enumerators, or respondents were reported

during program implementation or survey administration.

A secondary ethical concern was to avoid creating or legitimizing response routes at odds with Colombian law. 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 multiple activities designed to clearly communicate legal limits on the authority of JACs and other communal institutions. *ComunPaz* facilitators also documented the response routes devised in each community, which allowed us to confirm that they were consistent with Colombian law.

C RANDOMIZATION

C.1 BLOCKING

In Oriente Antioqueño, we created three blocks of seven communities and one block of six (27 communities total). In Nordeste Antioqueño, we created four blocks of six communities (24 communities total). In Ariari-Guayabero, we created seven blocks of four communities and three blocks of five (43 communities total). In Centro del Valle del Cauca, we created five blocks of five communities and five blocks of six (55 communities total). We used 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 remainder community itself.

C.2 COMPLIANCE

In five municipalities, encompassing 17 treatment communities, Police Inspectors did not participate in Module 1. 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. In

six of the 19 municipalities in which Police Inspectors participated in Module 1, their participation was limited to half the session. In two of the 22 municipalities in which police officers participated, their participation was limited to half the session as well. In most cases we unfortunately do not know the reasons that particular Police Inspectors or police officers did not participate, though the explanations we were able to document were idiosyncratic. In one case, for example, the Police Inspector was involved in a traffic accident en route to the session and sent a deputy in his place. In another two cases, police officers were called to respond to crimes committed in their jurisdictions.

As discussed in Appendix B, JACs were empowered to decide whether, when, and how the program would be implemented in their communities. 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 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 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 (except the three in which the JAC had disbanded or refused to participate altogether).

C.3 BALANCE

Table A.1 reports a balance test for the *ComunPaz* program. We regress a dummy for treatment assignment on six community-level covariates: 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 a voluntary coca substitution program established by the 2016 peace agreement. We test the individual and joint significance of these variables and find no evidence of imbalance.

D ENDLINE SURVEY

D.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 eight JAC leaders per 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 Coexistence and Conciliation Commission (*Comisión de Convivencia y Conciliación*) within the JAC, then sampled leaders of other committees until we fulfilled the sampling quota. 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.

D.2 DESCRIPTIVE STATISTICS

Tables A.2 and A.3 report individual- and community-level descriptive statistics, respectively.

E ANCILLARY ANALYSES AND ROBUSTNESS CHECKS

E.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 and leaders. Not surprisingly, we find that treatment group residents and leaders were more aware of these programs.

E.2 MULTIPLE HYPOTHESIS TESTING

The conditions under which multiple comparisons corrections are necessary remain a matter of debate in political science, especially when outcomes are pre-specified (Samii 2016). With this caveat, 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, following the recommendations in Lakens (2016) and Vanhove (2016).⁶ Thus, for example, Table A.8 amounts to a test of the hypothesis family that *ComunPaz* affected perceptions of any of the authorities 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

⁶In 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 misleading and unhelpful, and we abandon it in both our theoretical framework and our empirical strategy.

structure of these hypothesis tests differs dramatically from the structure of the others.

E.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 direct survey questions with a list experiment. Respondents were read a list of mechanisms that communities might use to resolve “basic problems of coexistence (*convivencia*),” then asked to count how many of those mechanisms are in use in 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 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. 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. 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 lower) estimates for reliance on armed groups than our direct questions, suggesting that reliance on armed groups is indeed rare, but also that it is not especially susceptible to social desirability bias.

Table A.11 reports the ITT on residents’ reliance on armed groups in the list experiment using a linear estimator 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. 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.

Social desirability may affect other results, too. Individuals assigned to the treatment group might have reported fewer unresolved or violent disputes, for example, if they understood that this was a goal of the intervention. While this is indeed a concern, as we discuss in the paper, our results are generally not consistent with socially desirability bias. While we observe beneficial effects on some outcomes that are potentially susceptible to social desirability concerns (violent disputes, for example), we observe null or even adverse effects on others (perceptions of JACs, for example, or reliance on armed groups among leaders). For reliance on armed groups, our list experiment (described above) helps us determine whether residents' responses to direct questions were contaminated by social desirability bias. We find no evidence that they were. Finally, for those outcomes we expect would be particularly affected by social desirability bias (e.g. willingness to report actual disputes to armed groups), we ask respondents about their beliefs regarding the behavior of *other* community members, which may prompt more truthful responses and mitigate social desirability concerns.

E.4 RESPECT FOR GOVERNMENT AUTHORITY

Table A.12 reports the ITT on respect for government authority in cases that fall unambiguously under state jurisdiction. To measure respect 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 state institutions, but we focus exclusively on cases over which the government claims both original and ultimate jurisdiction (e.g. domestic violence and robbery). Unfortunately, interpretation of this outcome is ambiguous. Rural Colombians often report to communal authorities first, regardless of the incident's severity, but this is only sometimes indicative of disrespect for government authority. (For example, in some cases residents report to communal institutions because they do not know how to access the police and want communal authorities to serve as liaisons.) With this caveat in mind, we find no evidence that the program increased respect for government authority.

E.5 RELIANCE ON DIRECT DIALOGUE TO RESOLVE DISPUTES

Table A.13 reports the ITT on reliance on direct dialogue to resolve disputes among residents (column 1) and leaders (column 2). For each hypothetical scenario of crime and conflict in the survey, respondents were asked which authority they would report to first (if any), and which they believed should provide a “definitive” resolution. We code a dummy for respondents who preferred direct dialogue (“*que arregle directamente*”) for any of these hypothetical scenarios. We find no evidence that the program increased reliance on direct dialogue. If anything, we find the opposite.

E.6 RELIANCE ON MULTIPLE AUTHORITIES TO RESOLVE DISPUTES

Table A.14 tests the ITT on reliance on multiple authorities to resolve disputes among residents (column 1) and leaders (column 2). For each hypothetical scenario of crime and violence, respondents were asked which authority they would report to first, and which authority they believed should provide a “definitive” resolution. We code a dummy for respondents who selected different authorities for these two questions. For each actual dispute in the survey, respondents were also asked if they or a household member had approached the JAC, a police officer, or the Police Inspector for assistance. We code a dummy for respondents who approached more than one authority. We find no evidence that the program increased reliance on multiple authorities.

E.7 ABSENCE OF DISPUTES

Table A.15 reports the ITT on the absence of disputes according to residents (column 1) and leaders (column 2). Respondents were asked to identify the most important sources of disputes in their communities. We code dummies for respondents who reported that there were no important sources of disputes, as long as they were not themselves involved in a dispute. We find that treatment group residents and leaders were, respectively, 2.9 and 2.6 percentage points more likely to report the absence of disputes. These ITTs are not quite statistically significant at conventional levels ($p = 0.165$ and $p = 0.133$, respectively), but they constitute substantively large increases of

roughly 50% and 99% relative to the control group means (0.058 and 0.029, respectively).

E.8 MARGINAL EFFECTS FROM ENDORSEMENT EXPERIMENT

Figure A.1 plots marginal effects from the endorsement experiment designed to measure perceptions of armed groups and governmental and communal institutions.

E.9 HETEROGENEOUS TREATMENT EFFECTS

In our PAP we pre-specified three potential sources of treatment effect heterogeneity: (1) the degree of residents' and JAC leaders' connectedness with local and municipal political power; (2) the strength of prior rebel and paramilitary governance at the community level;⁷ and (3) the extent to which residents and JAC leaders were victimized by violence during the Colombian civil war. We did not pre-specify a direction for these heterogeneous treatment effects, which we viewed as theoretically ambiguous. Nonetheless, each potential source of treatment effect heterogeneity is grounded in elements of our theoretical framework.

First, *ComunPaz* aims to encourage communication and collaboration between state and communal institutions. The program may be more effective in communities where residents and JAC leaders are already politically connected, as JAC leaders in these communities may feel more comfortable communicating and collaborating with state institutions, and residents may feel better equipped to demand respect and responsiveness from state authorities. On the other hand, if politically connected JAC leaders were already communicating and collaborating with their state counterparts at baseline, then ceiling effects may diminish the program's impact. Moreover, if JAC leaders serve as brokers between residents and local or municipal power networks, then they may perceive the police and Police Inspectors as interlopers, and may refuse to cooperate in order to protect their own role as intermediaries.

Second, *ComunPaz* depends on the existence of legitimate, locally embedded JACs. Re-

⁷In our PAP we disaggregated this second potential source of treatment effect heterogeneity into two components: prior rebel governance and prior paramilitary governance. For compactness we combine them here.

search on the legacies of civil war in Colombia has found that rebel groups (especially FARC) were more likely than paramilitaries to strengthen communal institutions as part of their efforts to establish territorial control (Vargas 2019). If stronger JACs are also more legitimate and more embedded in their communities, then the program may be more effective in communities that were previously controlled by rebel groups, especially relative to those that were previously controlled by paramilitaries. On the other hand, police officers and Police Inspectors may assume that JACs in these communities were previously allied with the rebels, and may be reluctant to cooperate with them. By the same token, police officers and Police Inspectors may assume that JACs in communities previously ruled by paramilitaries are partial to the state, and may be more willing to engage with them. Ceiling effects may also limit the impact of the program in communities with strong JACs.

Third, *ComunPaz* (implicitly) assumes that there is at least some demand for renewed state security provision in communities previously governed by rebel groups. The program may be more effective among residents who were victims of violence during the Colombian civil war, as recent studies have shown that citizens who were victimized in the past are more likely to demand state security provision even years after the fighting stops (Blair 2021; Blair and Morse 2021). On the other hand, existing evidence also suggests that exposure to wartime violence induces altruism (Voors et al. 2012), empathy (Hartman and Morse 2020), civic culture (Bellows and Miguel 2006), and political participation among victims (Blattman 2009). Residents who were victims of violence may be less likely to instigate conflicts with one another, more capable of resolving any conflicts that arise, and more willing to use social sanctions to enforce resolutions, thus obviating the need for a program like *ComunPaz*. Moreover, citizens who were brutalized by state security forces in the past may be less willing to seek protection from them in the present (Blair and Morse 2021).

Tables A.16 through A.21 report HTEs on the prevalence of unresolved and violent disputes (Tables A.16 through A.18) and reliance on armed groups and state and communal authorities 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 rebel or

paramilitary governance (Tables A.17 and A.20); and exposure to violence perpetrated by government, rebel, and paramilitary forces (Tables A.18 and A.21). Figure A.2 plots marginal effects on the prevalence of unresolved and violent disputes by connectedness; Figures A.3 and A.4 plot marginal effects on the prevalence of unresolved and violent disputes, respectively, by rebel and paramilitary governance; and Figures A.5 and A.6 plot marginal effects on the prevalence of unresolved and violent disputes, respectively, by exposure to violence perpetrated by government, rebel, and paramilitary forces. (Because we use an AES estimator to test for heterogeneous treatment effects on reliance on armed groups and state and communal authorities to resolve disputes, we do not plot marginal effects for these tables.)

To measure connectedness to political power, residents were asked if they are members of the JAC; members of the JAC's board of directors; related to a JAC member; related to the mayor; or 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 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. To measure exposure to violence, residents and leaders were asked if they or a family member had been a 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. We find that the negative ITT on unresolved disputes is stronger in communities with histories of rebel governance, stronger among victims of paramilitary violence, and weaker among victims of government violence, but only in the residents survey. We also find that the negative ITT on reliance on armed groups is weaker among victims of rebel violence, again in the residents

survey only. But these are exceptions, and we take care not to over-interpret them. In general, the effects of the *ComunPaz* program do not appear to vary along the dimensions we pre-specified.

E.10 ALTERNATE CODING RULES FOR DEPENDENT VARIABLES

Following our PAP, Table A.22 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 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 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.

Also following our PAP, Table A.23 reports the ITT on consensus around dispute resolution, using alternate constructions of 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 “definitive” resolution. We take the modal response to each of these questions among residents, then test for effects on 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 for effects on the likelihood that each resident’s response (column 3) and each leader’s response (column 4) matches the modal leader response. Our ITT estimates remain statistically indistinguishable from 0 regardless of specification.

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
Household size	3.29	1.68	2673	3.52	1.59	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
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 <i>ComunPaz</i> program	
	(1)	(2)	(3)	(4)
	Residents	Leaders	Residents	Leaders
Assigned to treatment	0.042*** [0.014]	0.094*** [0.028]	0.030** [0.013]	0.117*** [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 [0.033]	-0.093** [0.041]	0.001 [0.010]	-0.051* [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	.418	.024	.945	.053
B-H q-value	.557	.097	.945	.106
H-B threshold	.05	.025	.1	.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)	(2)	(3)	(4)	(5)	(6)
	Residents	Leaders	Residents	Leaders	Residents	Leaders
Assigned to treatment	-0.056** [0.027]	-0.006 [0.036]	-0.028 [0.051]	-0.043 [0.058]	-0.028 [0.055]	-0.049 [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	.038	.956	.584	.458	.606	.388
B-H q-value	.23	.957	.728	.728	.728	.728
H-B threshold	.017	.1	.033	.025	.05	.02

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	0.014 [0.034]	0.010 [0.031]	-0.026 [0.039]	0.007 [0.041]	0.040 [0.041]
Observations	2673	1182	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	.668	.75	.508	.863	.335
B-H q-value	.864	.864	.864	.864	.864
H-B threshold	.033	.05	.025	.1	.02

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)	(2)	(3)	(4)	(5)	(6)	(7)
Residents		Leaders	Residents	Police	PIs	Residents	Leaders
Assigned to treatment	-0.083** [0.037]	-0.074 [0.046]	-0.023 [0.062]	0.012 [0.074]	0.187** [0.085]	0.105* [0.064]	0.068 [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	.024	.106	.715	.875	.029	.098	.301
B-H q-value	.102	.186	.834	.875	.102	.186	.422
H-B threshold	.014	.025	.05	.1	.017	.02	.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	-0.031 [0.036]	0.004 [0.036]	0.092 [0.056]	0.028 [0.129]	0.249** [0.114]	0.153** [0.062]
Observations	2673	1182	1182	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	.379	.902	.102	.827	.03	.102
B-H q-value	.570	.903	.205	.903	.09	.205
H-B threshold	.033	.1	.025	.05	.02	.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	-0.031 [0.055]	-0.014 [0.050]
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 [0.031]	-0.069* [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)	(2)
	Residents	Leaders
Assigned to treatment	-0.017 [0.049]	-0.003 [0.056]
Observations	2673	1182
Individual controls	Yes	Yes
Community controls	Yes	Yes
Block FE	Yes	Yes
Weights	Yes	Yes
Estimator	AES	AES

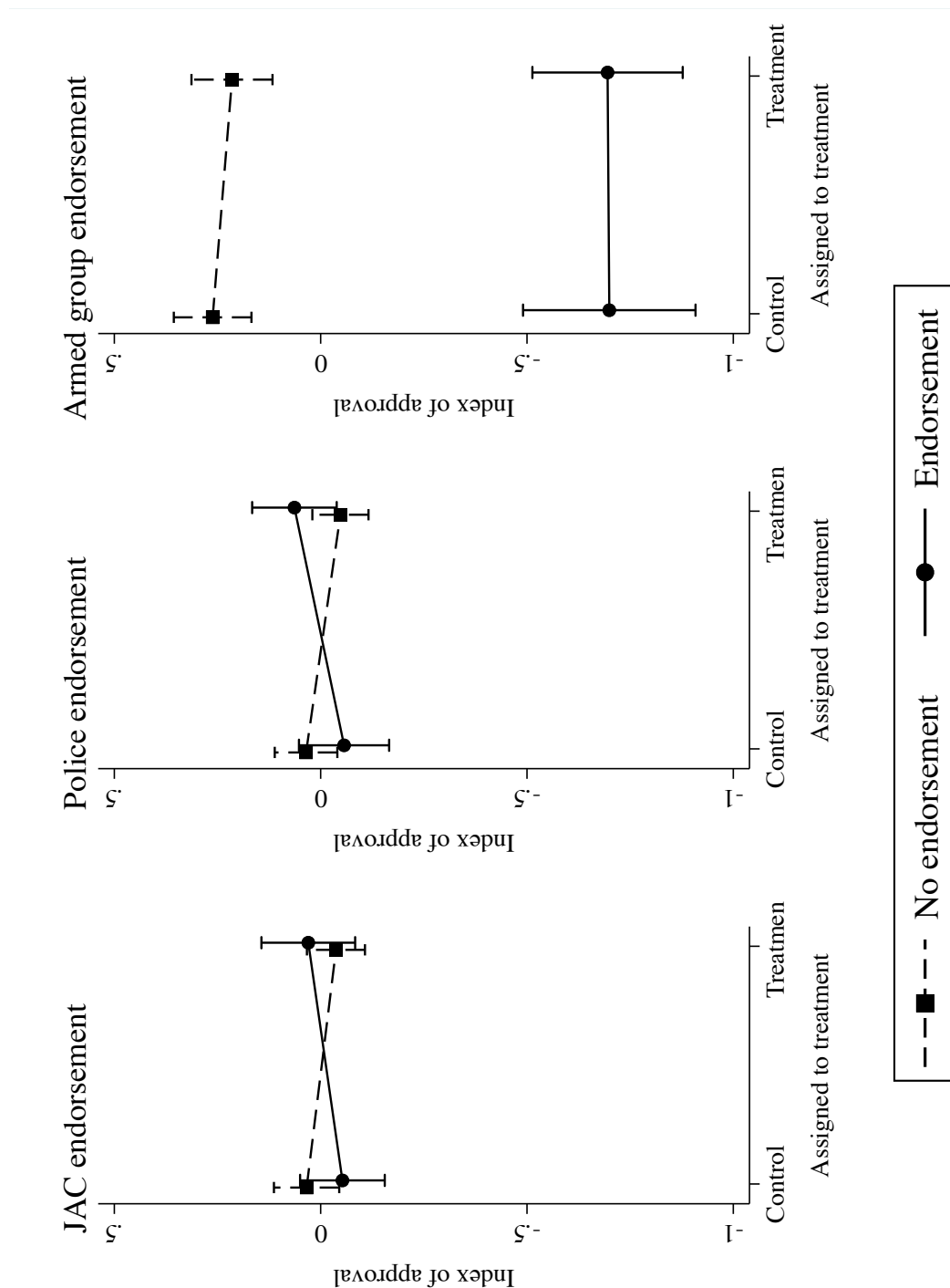
Notes: All specifications include individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors, clustered by community, are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.15: Absence of disputes

	No disputes	
	(1) Residents	(2) Leaders
Assigned to treatment	0.029 [0.021]	0.026 [0.019]
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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure A.1: Perceptions of armed groups and governmental and communal institutions among residents using endorsement experiment



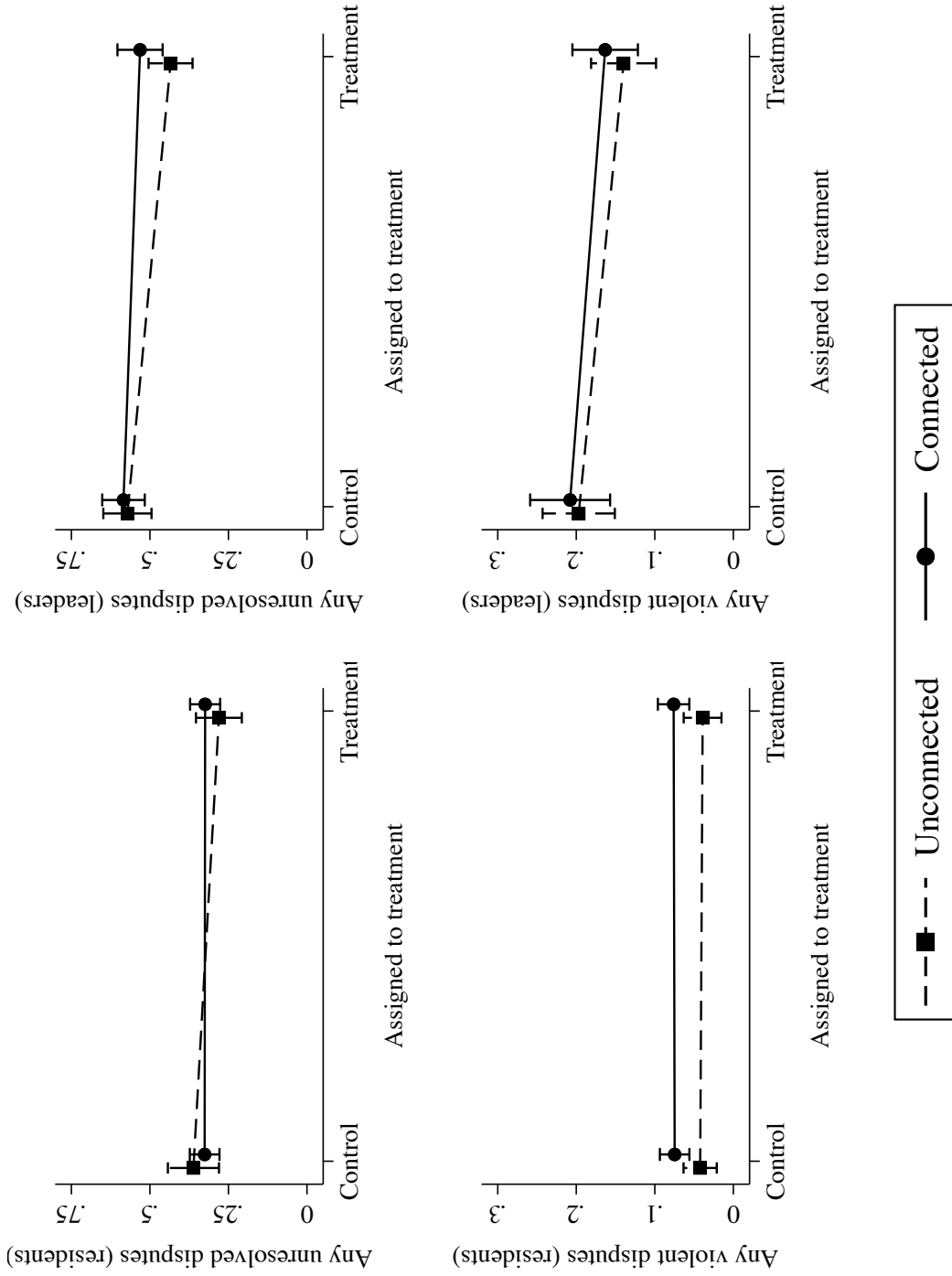
Notes: Marginal effects of the *ComunPaz* program on perceptions of armed groups and governmental and communal institutions among residents in the endorsement experiment. Specification includes individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors are clustered by community. Lines denote 95% confidence intervals.

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	-0.081 [0.056]	-0.137*** [0.052]	-0.003 [0.015]	-0.057* [0.029]
Connected	-0.036 [0.039]	0.013 [0.047]	0.032** [0.014]	0.011 [0.027]
Assigned to treatment \times connected	0.080 [0.057]	0.085 [0.062]	0.004 [0.022]	0.013 [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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure A.2: Prevalence of unresolved and violent disputes, heterogeneity by connectedness



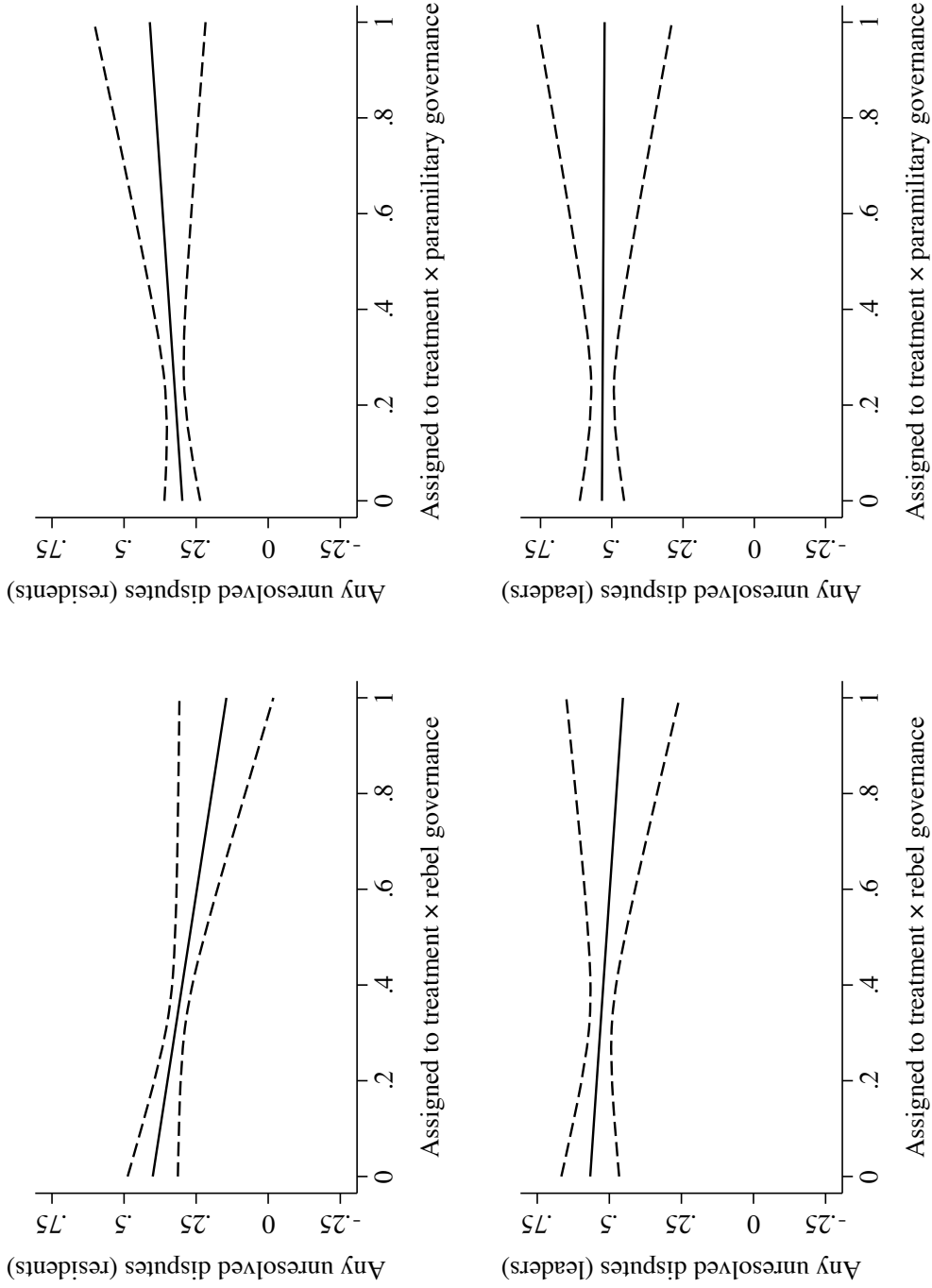
Notes: Heterogeneous treatment effects (HTEs) of the *ComunPaz* program by connectedness to local and municipal power. All specifications include individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors are clustered by community. Lines denote 95% confidence intervals.

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

	Any unresolved disputes		Any violent disputes	
	(1)	(2)	(3)	(4)
	Residents	Leaders	Residents	Leaders
Assigned to treatment	0.072 [0.079]	-0.018 [0.102]	0.008 [0.020]	-0.018 [0.040]
Rebel governance	0.074 [0.095]	0.127 [0.122]	-0.045 [0.032]	0.026 [0.081]
Paramilitary governance	-0.042 [0.107]	0.088 [0.117]	0.035 [0.031]	0.165** [0.070]
Assigned to treatment × rebel governance	-0.255** [0.123]	-0.113 [0.145]	-0.001 [0.038]	0.022 [0.092]
Assigned to treatment × paramilitary governance	0.113 [0.124]	-0.009 [0.153]	-0.011 [0.037]	-0.086 [0.100]
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

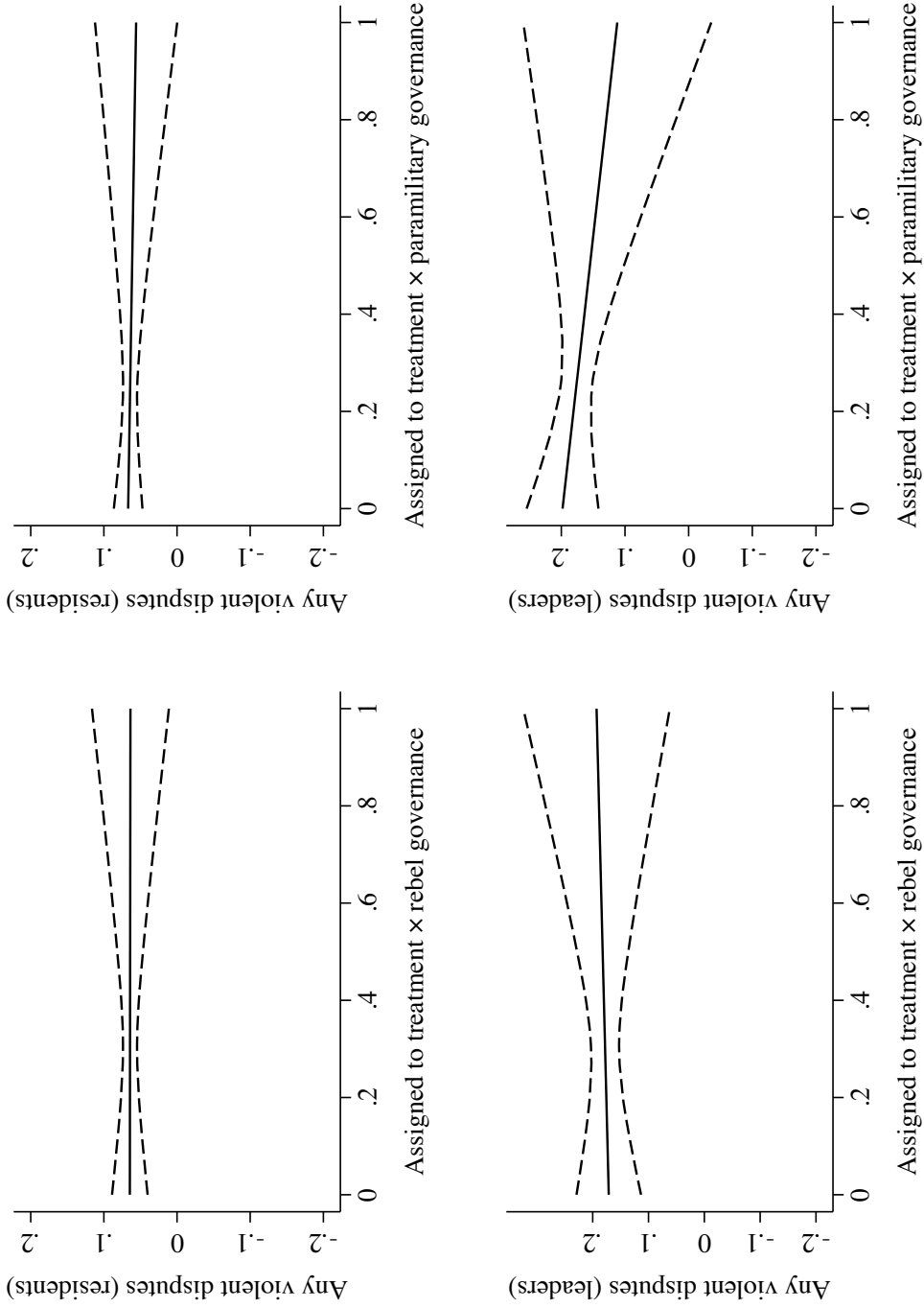
Notes: Heterogeneous treatment effects (HTEs) of the *ComunPaz* program by armed group governance. All specifications include individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors, clustered by community, are in brackets. *** $p < 0.01$, ** $p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

Figure A.3: Prevalence of unresolved disputes, heterogeneity by armed groups governance



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 are clustered by community. Lines denote 95% confidence intervals.

Figure A.4: Prevalence of violent disputes, heterogeneity by armed groups governance



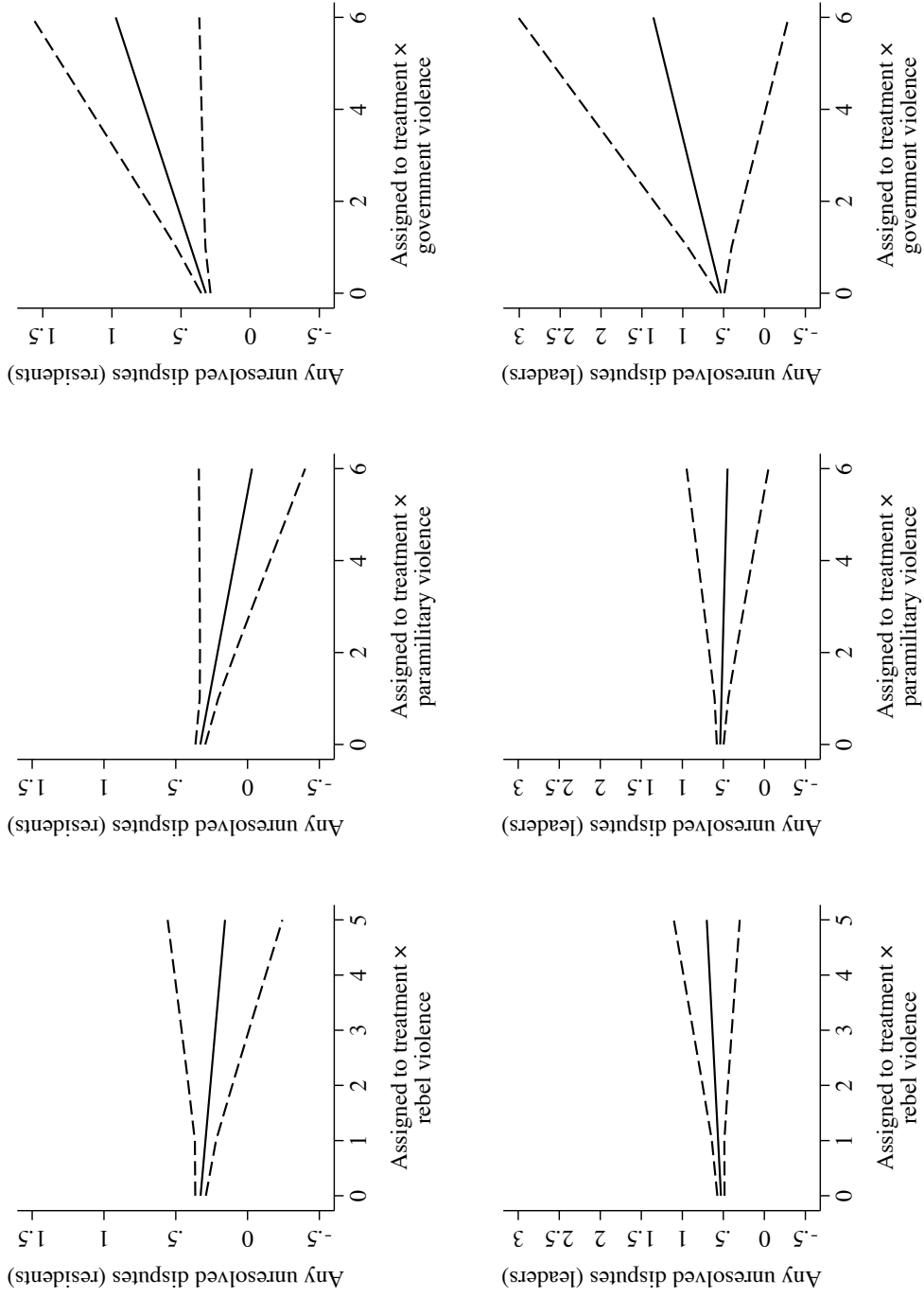
Notes: Heterogeneous treatment effects (HTEs) of the *ComunPaz* program by armed group governance. All specifications include individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors are clustered by community. Lines denote 95% confidence intervals.

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	-0.027 [0.033]	-0.079* [0.042]	0.001 [0.010]	-0.046* [0.027]
Rebel violence	0.041** [0.021]	0.034* [0.020]	0.018* [0.010]	0.021 [0.015]
Paramilitary violence	0.037** [0.017]	0.025 [0.019]	0.014 [0.011]	0.046*** [0.016]
Government violence	-0.020* [0.011]	-0.071* [0.038]	0.021 [0.013]	-0.006 [0.040]
Assigned to treatment × rebel violence	-0.024 [0.030]	0.025 [0.031]	0.009 [0.014]	0.024 [0.022]
Assigned to treatment × paramilitary violence	-0.041* [0.022]	-0.010 [0.030]	-0.008 [0.013]	-0.024 [0.020]
Assigned to treatment × government violence	0.030** [0.014]	0.038 [0.039]	-0.017 [0.015]	-0.001 [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

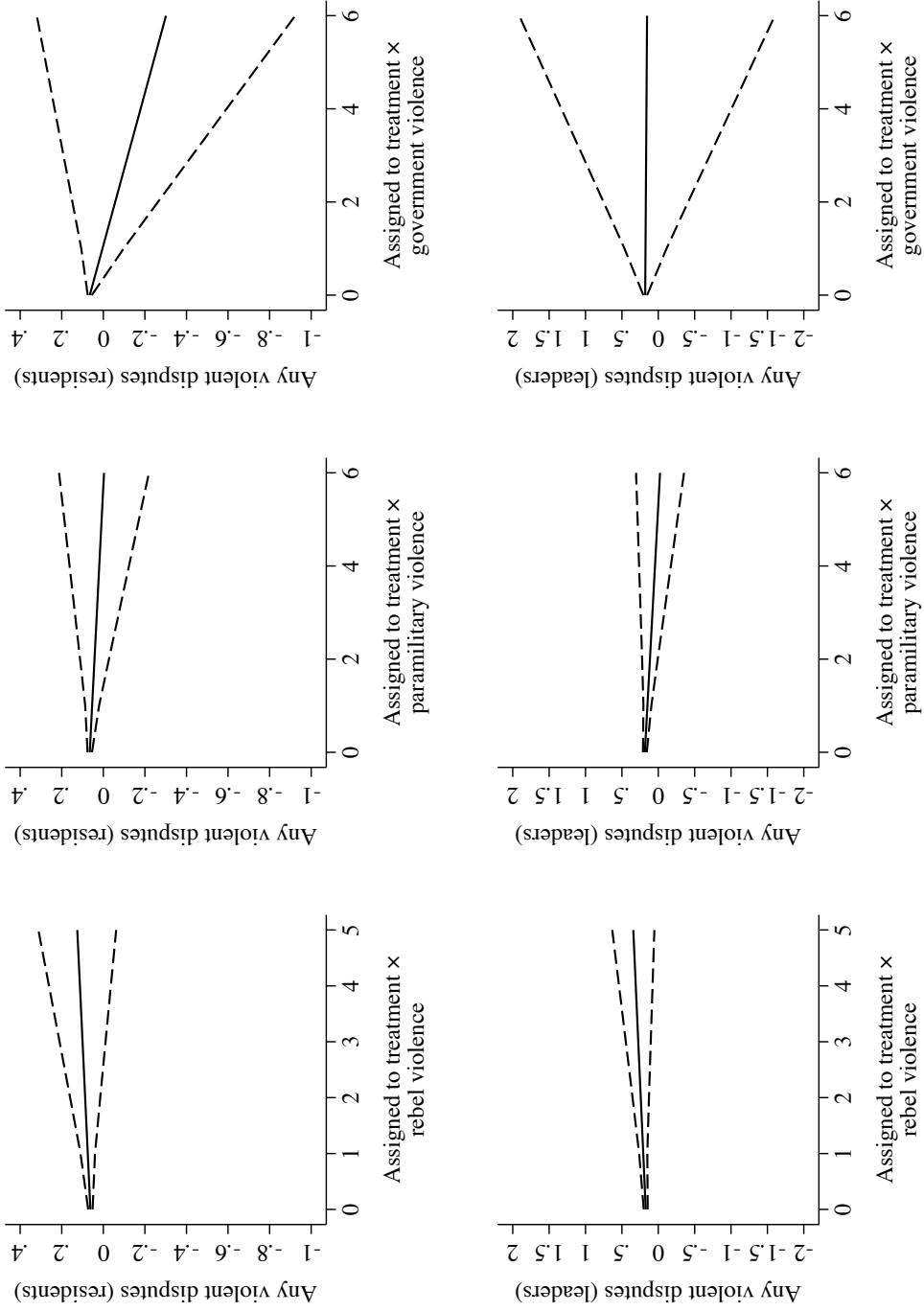
Notes: Heterogeneous treatment effects (HTEs) of the *ComunPaz* program by exposure to violence. All specifications include individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors, clustered by community, are in brackets. *** $p < 0.01$, ** $p 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

Figure A.5: Prevalence of unresolved disputes, heterogeneity by exposure to violence



Notes: Heterogeneous treatment effects (HTEs) of the *ComunPaz* program by exposure to violence. All specifications includes individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors are clustered by community. Lines denote 95% confidence intervals.

Figure A.6: Prevalence of violent disputes, heterogeneity by exposure to violence



Notes: Heterogeneous treatment effects (HTEs) of the *ComunPaz* program by exposure to violence. All specifications includes individual- and community-level controls, block fixed effects, and inverse probability weights. Standard errors are clustered by community. Lines denote 95% confidence intervals.

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)	(2)	(3)	(4)	(5)	(6)
	Residents	Leaders	Residents	Leaders	Residents	Leaders
Assigned to treatment	-0.025 [0.033]	0.018 [0.035]	-0.021 [0.078]	-0.088 [0.073]	-0.064 [0.070]	-0.106* [0.061]
Connected	0.047 [0.055]	0.085* [0.051]	0.031 [0.055]	-0.011 [0.054]	0.070 [0.054]	0.083 [0.055]
Assigned to treatment \times connected	-0.046 [0.062]	-0.039 [0.064]	-0.011 [0.079]	0.084 [0.073]	0.050 [0.069]	0.115 [0.074]
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)	(2)	(3)	(4)	(5)	(6)
	Residents	Leaders	Residents	Leaders	Residents	Leaders
Assigned to treatment	-0.053 [0.045]	-0.093 [0.062]	0.020 [0.126]	0.006 [0.146]	-0.081 [0.146]	-0.005 [0.131]
Rebel governance	0.122 [0.078]	0.032 [0.095]	0.421*** [0.148]	0.425*** [0.147]	-0.378*** [0.143]	-0.273* [0.166]
Paramilitary governance	-0.154** [0.076]	-0.151 [0.102]	-0.091 [0.139]	-0.117 [0.136]	0.263* [0.145]	0.230 [0.160]
Assigned to treatment \times rebel governance	-0.122 [0.084]	-0.017 [0.121]	-0.135 [0.166]	-0.120 [0.163]	0.194 [0.191]	0.045 [0.196]
Assigned to treatment \times paramilitary governance	0.135 [0.097]	0.193 [0.148]	0.058 [0.191]	0.035 [0.173]	-0.109 [0.186]	-0.132 [0.216]
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	-0.050* [0.028]	-0.012 [0.039]	-0.029 [0.050]	-0.027 [0.061]	-0.025 [0.053]	-0.037 [0.059]
Rebel violence	-0.007 [0.018]	0.004 [0.016]	0.061*** [0.020]	0.033 [0.021]	0.076*** [0.025]	0.056** [0.024]
Paramilitary violence	-0.003 [0.019]	0.006 [0.020]	-0.022 [0.034]	-0.019 [0.034]	0.019 [0.026]	-0.002 [0.029]
Government violence	0.011 [0.020]	0.052 [0.073]	-0.023 [0.027]	0.019 [0.034]	-0.005 [0.021]	-0.047 [0.053]
Assigned to treatment × rebel violence	0.068** [0.032]	0.022 [0.032]	-0.007 [0.034]	-0.040 [0.035]	0.015 [0.037]	-0.005 [0.034]
Assigned to treatment × paramilitary violence	0.006 [0.030]	0.020 [0.037]	-0.019 [0.045]	0.012 [0.042]	-0.011 [0.033]	-0.034 [0.040]
Assigned to treatment × government violence	-0.009 [0.021]	-0.057 [0.071]	0.017 [0.028]	0.037 [0.038]	0.022 [0.023]	0.049 [0.054]
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: 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 [0.049]	-0.033 [0.057]	0.017 [0.046]	0.024 [0.051]	0.003 [0.036]	0.040 [0.044]	0.009 [0.041]	0.040 [0.044]
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.23: Consensus around dispute resolution, alternate constructions of the DV

	Consensus around dispute resolution (modal resident)		Consensus around dispute resolution (modal leader)	
	(1)	(2)	(3)	(4)
	Residents	Leaders	Residents	Leaders
Assigned to treatment	-0.049 [0.035]	-0.024 [0.038]	-0.025 [0.040]	-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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.