SUPPLEMENTAL INFORMATION FOR "PREVENTING REBEL RESURGENCE AFTER CIVIL WAR: A FIELD EXPERIMENT IN SECURITY AND JUSTICE PROVISION IN RURAL COLOMBIA"

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A.1 INTERVENTION

A.1.1 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 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 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

[1] Recall 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.

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

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

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

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

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 traveled to municipal capitals to collect more detailed, localized information 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.

A.3 RANDOMIZATION

A.3.1 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 generated 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.

A.3.2 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 A.2, 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).

A.3.3 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

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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

satellite-detected coca cultivation within 15 kilometers 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.

A.4 Endline survey

A.4.1 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.

A.4.2 Descriptive statistics

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

A.5 Ancillary analyses and robustness checks

A.5.1 Respect for government authority

Table A.4 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

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 UN Office on Drugs and Crime (UNODC).

Table A.4: **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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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.

A.5.2 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 structure of these hypothesis tests differs dramatically from the structure of the others.

[6] In our PAP we pre-specified that we would apply multiple comparisons corrections across our "primary" and "secondary" hypotheses. This distinction between primary and secondary hypotheses turned out to be misleading and unhelpful, and we abandon it in both our theoretical framework and our empirical strategy.

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

	Any unresolved disputes		**Any violent disputes**	
	(1) Residents	(2) Leaders	(3) Residents	(4) Leaders
Assigned to treatment	-0.027	-0.093**	0.001	-0.051*
	[0.033]	[0.041]	[0.010]	[0.026]
Observations	2673	1182	2673	1182
Individual controls	Yes	Yes	Yes	Yes
Community controls	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes
Estimator	OLS	OLS	OLS	OLS
p-value	.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) Residents	(2) Leaders	(3) Residents	(4) Leaders	(5) Residents	(6) Leaders
Assigned to treatment	-0.056**	-0.006	-0.028	-0.043	-0.028	-0.049
	[0.027]	[0.036]	[0.051]	[0.058]	[0.055]	[0.057]
Observations	2673	1182	2673	1182	2673	1182
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Community controls	Yes	Yes	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes	Yes
Estimator	AES	AES	AES	AES	AES	AES
p-value	.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) Residents	(2) Leaders	(3) Residents	(4) Police	(5) PIs	(6) Residents	(7) Leaders
Assigned to treatment	-0.083**	-0.074	-0.023	0.012	0.187**	0.105*	0.068
	[0.037]	[0.046]	[0.062]	[0.074]	[0.085]	[0.064]	[0.066]
Observations	2673	1182	2673	149	149	2673	1182
Individual controls	Yes	Yes	Yes	No	No	Yes	Yes
Community controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Estimator	AES	AES	AES	OLS	OLS	AES	AES
p-value	.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	0.004	0.092	0.028	0.249**	0.153**
	[0.036]	[0.036]	[0.056]	[0.129]	[0.114]	[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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

A.5.3 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 were 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 in the article. 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 article, 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.

A.5.4 Reliance on direct dialogue to resolve disputes

Table A.12 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.

A.5.5 Reliance on multiple authorities to resolve disputes

Table A.13 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.

Table A.12: **Reliance on direct mediation to resolve disputes**

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

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

Table A.13: **Reliance on multiple authorities to resolve disputes**

	Reliance on multiple authorities	
	(1) Residents	(2) Leaders
Assigned to treatment	-0.017	-0.003
	[0.049]	[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.14: **Absence of disputes**

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

A.5.6 Absence of disputes

Table A.14 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 90% relative to the control group means (0.058 and 0.029, respectively).

A.5.7 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

[7] In 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.

Table A.15: **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	-0.137***	-0.003	-0.057*
	[0.056]	[0.052]	[0.015]	[0.029]
Connected	-0.036	0.013	0.032**	0.011
	[0.039]	[0.047]	[0.014]	[0.027]
Assigned to treatment × connected	0.080	0.085	0.004	0.013
	[0.057]	[0.062]	[0.022]	[0.039]
Observations	2673	1182	2673	1182
Individual controls	Yes	Yes	Yes	Yes
Community controls	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes
Estimator	OLS	OLS	OLS	OLS

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

Table A.16: **Prevalence of unresolved and violent disputes, heterogeneity by armed group governance**

	Any unresolved disputes		**Any violent disputes**	
	(1) Residents	(2) Leaders	(3) Residents	(4) Leaders
Assigned to treatment	0.072	-0.018	0.008	-0.018
	[0.079]	[0.102]	[0.020]	[0.040]
Rebel governance	0.074	0.127	-0.045	0.026
	[0.095]	[0.122]	[0.032]	[0.081]
Paramilitary governance	-0.042	0.088	0.035	0.165**
	[0.107]	[0.117]	[0.031]	[0.070]
Assigned to treatment × rebel governance	-0.255**	-0.113	-0.001	0.022
	[0.123]	[0.145]	[0.038]	[0.092]
Assigned to treatment × paramilitary governance	0.113	-0.009	-0.011	-0.086
	[0.124]	[0.153]	[0.037]	[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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.17: **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	-0.079*	0.001	-0.046*
	[0.033]	[0.042]	[0.010]	[0.027]
Rebel violence	0.041**	0.034*	0.018*	0.021
	[0.021]	[0.020]	[0.010]	[0.015]
Paramilitary violence	0.037**	0.025	0.014	0.046***
	[0.017]	[0.019]	[0.011]	[0.016]
Government violence	-0.020*	-0.071*	0.021	-0.006
	[0.011]	[0.038]	[0.013]	[0.040]
Assigned to treatment × rebel violence	-0.024	0.025	0.009	0.024
	[0.030]	[0.031]	[0.014]	[0.022]
Assigned to treatment × paramilitary violence	-0.041*	-0.010	-0.008	-0.024
	[0.022]	[0.030]	[0.013]	[0.020]
Assigned to treatment × government violence	0.030**	0.038	-0.017	-0.001
	[0.014]	[0.039]	[0.015]	[0.041]
Observations	2631	1160	2631	1160
Individual controls	Yes	Yes	Yes	Yes
Community controls	Yes	Yes	Yes	Yes
Block FE	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes
Estimator	OLS	OLS	OLS	OLS

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

Table A.18: **Reliance on armed groups and governmental and communal institutions to resolve disputes, heterogeneity by connectedness**

	Reliance on armed groups		**Reliance on JACs**		**Reliance on police and PIs**	
	(1) Residents	(2) Leaders	(3) Residents	(4) Leaders	(5) Residents	(6) Leaders
Assigned to treatment	-0.025	0.018	-0.021	-0.088	-0.064	-0.106*
	[0.033]	[0.035]	[0.078]	[0.073]	[0.070]	[0.061]
Connected	0.047	0.085*	0.031	-0.011	0.070	0.083
	[0.055]	[0.051]	[0.055]	[0.054]	[0.054]	[0.055]
Assigned to treatment × connected	-0.046	-0.039	-0.011	0.084	0.050	0.115
	[0.062]	[0.064]	[0.079]	[0.073]	[0.069]	[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.19: **Reliance on armed groups and governmental and communal institutions to resolve disputes, heterogeneity by armed group governance**

	Reliance on armed groups		**Reliance on JACs**		**Reliance on police and PIs**	
	(1) Residents	(2) Leaders	(3) Residents	(4) Leaders	(5) Residents	(6) Leaders
Assigned to treatment	-0.053	-0.093	0.020	0.006	-0.081	-0.005
	[0.045]	[0.062]	[0.126]	[0.146]	[0.146]	[0.131]
Rebel governance	0.122	0.032	0.421***	0.425***	-0.378***	-0.273*
	[0.078]	[0.095]	[0.148]	[0.147]	[0.143]	[0.166]
Paramilitary governance	-0.154**	-0.151	-0.091	-0.117	0.263*	0.230
	[0.076]	[0.102]	[0.139]	[0.136]	[0.145]	[0.160]
Assigned to treatment × rebel governance	-0.122	-0.017	-0.135	-0.120	0.194	0.045
	[0.084]	[0.121]	[0.166]	[0.163]	[0.191]	[0.196]
Assigned to treatment × paramilitary governance	0.135	0.193	0.058	0.035	-0.109	-0.132
	[0.097]	[0.148]	[0.191]	[0.173]	[0.186]	[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.20: **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*	-0.012	-0.029	-0.027	-0.025	-0.037
	[0.028]	[0.039]	[0.050]	[0.061]	[0.053]	[0.059]
Rebel violence	-0.007	0.004	0.061***	0.033	0.076***	0.056**
	[0.018]	[0.016]	[0.020]	[0.021]	[0.025]	[0.024]
Paramilitary violence	-0.003	0.006	-0.022	-0.019	0.019	-0.002
	[0.019]	[0.020]	[0.034]	[0.034]	[0.026]	[0.029]
Government violence	0.011	0.052	-0.023	0.019	-0.005	-0.047
	[0.020]	[0.073]	[0.027]	[0.034]	[0.021]	[0.053]
Assigned to treatment × rebel violence	0.068**	0.022	-0.007	-0.040	0.015	-0.005
	[0.032]	[0.032]	[0.034]	[0.035]	[0.037]	[0.034]
Assigned to treatment × paramilitary violence	0.006	0.020	-0.019	0.012	-0.011	-0.034
	[0.030]	[0.037]	[0.045]	[0.042]	[0.033]	[0.040]
Assigned to treatment × government violence	-0.009	-0.057	0.017	0.037	0.022	0.049
	[0.021]	[0.071]	[0.028]	[0.038]	[0.023]	[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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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. Research 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 Castillo 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.15 through A.20 report HTEs on the prevalence of unresolved and violent disputes (Tables A.15 through A.17) and reliance on armed groups and state and communal authorities to resolve disputes (Tables A.18 through A.20). We test for treatment effect heterogeneity by connectedness to local and municipal political power (Tables A.15 and A.18); histories of rebel or paramilitary governance (Tables A.16 and A.19); and exposure to violence perpetrated by government, rebel, and paramilitary forces (Tables A.17 and A.20).

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.

A.6 Additional supplemental information

Additional supplemental information and results from additional ancillary analyses and robustness checks are included with our replication files, available at https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/OXSQMU.