Engineering informal institutions: Long run impacts of alternative dispute resolution on violence and property rights in Liberia

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Abstract

Informal institutions govern property rights and disputes when formal systems are weak. Effective informal institutions should help people reach and maintain bargains, minimizing violence. Can outside organizations engineer persistent institutional change? Will this strengthen property rights and investment? We experimentally evaluate a UN and civil society mass education campaign to promote alternative dispute resolution (ADR) practices and norms in rural Liberia, where violent land disputes are common. Prior work showed a drop in violence and unresolved disputes within one year. We return after three years to test for sustained impacts and mechanisms. Treated communities report large, persistent drops in violent disputes and a slight shift towards nonviolent norms. Treated residents also report larger farms, though overall effects on property rights and investment are mixed. Politically-connected residents report more secure property rights while those with fewer connections feel less secure. Sustained institutional engineering is feasible but politics shapes distributional outcomes.[1]

In settings where formal justice systems are absent or overloaded, informal institutions resolve disputes and protect property rights.[2] Policymakers frequently try to improve informal institutions, but can more just, equal, or peaceful systems be engineered? This paper presents the short- and long-term effects of an alternative dispute resolution (ADR) campaign designed to strengthen informal institutions in rural Liberia. ADR is a set of informal mediation and negotiation practices intended to help parties improve communication, contain emotion, avoid the use of violence, and commit to negotiated or mediated solutions to disputes (Mnookin 1998; Lieberman and Henry 1986).

We evaluate the ADR campaign using a randomized controlled trial. The government of Liberia nominated 246 eligible communities, 85 of which were assigned to and received ADR. Implementers invited a sixth of all adults to participate in eight days of training spread over several months. In previous work we reported the short-run effects of the campaign roughly one year after the average community was treated. This paper assesses longer term impacts and mechanisms with data from three years after the average community was treated. Treatment increased the rate of dispute resolution in the first year, but after three years this effect appears to decay. Treatment communities did, however, witness a substantively large and statistically significant drop in threats and violence associated with disputes after three years. Relatedly, there is suggestive evidence that ADR worked in part by promoting norms and skills related to managing emotions and avoiding violence.

Previous studies suggest that more efficient informal dispute resolution systems can strengthen property rights and increase investment (Besley and Ghatak 2009; Goldstein and Udry 2008; Goldstein et al. 2015). We find little evidence of such an effect in our context. Still, while the program was expensive and appears not to have stimulated investment, the persistent decline in violence suggests that it is at least feasible to engineer more effective informal institutions. This is an important finding for both theory and practice, and is especially remarkable given that the intervention lasted only two months in each village, and that we measured outcomes three years after implementation.

1 Theoretical framework

In rural areas of many less developed countries, formal systems for resolving disputes are ineffective or non-existent. In their absence, disputants may negotiate with one another or seek informal third party mediators. But commitment problems, information asymmetries, and failures of "rational" bargaining—e.g. emotional reactions to perceived slights—can result in bargaining breakdown, heightening the risk of violence.

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[2] Informal institutions are shared unwritten rules of appropriate behavior enforced through social sanction and praise, and through neocustomary or "traditional" mechanisms of adjudication (Ellickson 1991; Knight 1992; North 1990).

ADR aims to reduce the length and cost of conflicts and strengthen resolutions through improved negotiation between disputants, or through mediation by a third party (Lieberman and Henry 1986; Mnookin 1998). It does so by imparting skills (how to resolve disputes) and norms (how people ought to resolve disputes). The goal is to foster trust, encourage communication, and increase empathy; discourage defection and forum shopping; and teach disputants to manage their emotions using techniques that have been shown to reduce violence in a variety of settings (Beck 2000; Blattman et al. 2017; Heller et al. 2017).

But there are reasons to be skeptical that campaigns of this sort can create sustained change. Norms are sticky, and the mechanisms of social sanction and praise that sustain them can prevent individuals from abandoning old norms or adopting new ones, even if they would like to. Moreover, the settings into which ADR is introduced are never institutional vacuums. Efforts to change skills and norms often fail, or have unintended consequences when thrust on strong, preexisting social relations and obligations (Moore 1973). In many developing countries, informal institutions favor men over women, elders over youths, ethnic majority over minority group members, and, more generally, those who hold connections to local leadership over those who do not. Because bargains reached through ADR are by nature unenforceable, they may only accentuate local power imbalances (Sternlight 2006).

These challenges make it all the more important to test the efficacy of ADR over both shorter and longer time horizons. Some of ADR's purported benefits—changes in norms, for example—may take years to accrue. Others may decay over time, for example if locally powerful individuals co-opt new mechanisms for resolving disputes. Some potential longer-run benefits, like enhanced security of property rights, depend on shorter-term benefits, like a reduction in the length and severity of conflicts. To our knowledge, ours is the first study to test ADR's effectiveness in both the short and medium term.

2   Setting

Liberia is a small West African nation still recovering from 14 years of civil war that ended in 2003. It is also one of the poorest countries in the world, where subsistence farming provides the livelihood for most rural residents, and where land is often a household's most important asset. Access to land in rural areas is governed by a "neocustomary" system of property rights, which tends to privilege the (relatively) wealthy and politically connected (Boone 2014). Disputes over land and other assets are endemic, especially in the wake of the civil war, which resulted in the displacement of much of the population, the destruction of written records securing individuals' rights to land, and the loss of oral records when local leaders and elders were displaced, emigrated, or died. In 2011, 16% of all households nationwide reported a land dispute since the end of the conflict, and 10% reported another dispute over money or inheritance (Vinck et al. 2011).

Liberians seldom use the formal justice system to resolve these disputes. Almost three-quarters of respon-

dents with land disputes in our sample sought resolution through an informal institution instead, typically overseen by local leaders. While these informal institutions can be cheap and effective, they have limitations. Nationwide, roughly 40% of land disputes and 16% of other disputes recorded in 2011 remained unresolved (Vinck et al. 2011, 61). Nearly half of all land disputes in our sample involved threats or violence, which occasionally escalated (Blair et al. 2017). The government of Liberia and UN peacekeepers have long worried that local disputes could spark a national crisis.

2.1 Intervention

In response to these challenges, in 2008 the United Nations High Commission for Refugees (UNHCR) and an NGO, the Justice and Peace Commission (JPC), proposed a mass education program that would promote ADR in Liberian communities. Each workshop involved eight days of training led by two facilitators who spoke the local language. The program built on existing informal institutions administered by local leaders. In a departure from the status quo, however, implementers trained residents to negotiate resolutions to their own disputes or mediate conflicts between their neighbors. In this way, implementers sought to reduce the favoritism that many Liberians believe is rife within existing informal institutions (Isser et al. 2009).

UNHCR and JPC sought to recruit about one-sixth of all adults in each village for training. Achieving spillover was central to the design of the intervention: the goal was to shift dispute resolution skills and norms throughout the community, not just among trainees. To that end, implementers encouraged participants to discuss lessons learned from the workshops with friends and family, and to apply them outside of the training. Facilitators also lived and slept in communities on weekdays, and used their interactions with both participants and non-participants after hours to demonstrate and reinforce messages from the training, and to disseminate them more broadly.

The curriculum encouraged participants to engage in their own and one another's disputes, taught problem solving and negotiation skills, discouraged forum shopping, and provided strategies for regulating emotions and avoiding threats and violence. Workshops combined lectures with discussion, group work, and role play, and focused in particular on interpersonal conflicts, especially conflicting claims over land, given their importance in most communities.

2.2 Sample selection

UNHCR and JPC worked in 3 of Liberia's 15 counties—Lofa, Nimba, and Grand Gedeh—where the risk of violence associated with disputes was believed to be especially high. County officials nominated the 246 communities they felt would benefit most from the intervention, ranging in size from 100 to 5000 residents. In reality, land conflicts were only moderately more common in these communities than in the country as a whole (Vinck et al. 2011). Facilitators and local leaders mobilized residents for the training in a variety of

ways in order to meet their target of one-sixth of the adult population.

3 Randomization, data, and estimation

We stratified by county and randomly assigned 116 of the 246 communities in our sample to treatment. Communities were widely dispersed, with little risk of spillover between them. Treatment was implemented in five randomly ordered phases over 21 months, from March 2009 to November 2010.[3] We conducted a baseline survey in March–April 2009. The first endline ran from November 2010 to January 2011, the second from February to April 2013. For simplicity we refer to these as the 1-year and 3-year endlines, respectively. In each round we surveyed roughly 20 residents per community, sampled using the "random route" method (see appendix B on page ii). We also surveyed four local leaders—typically a town chief and a female, youth, and minority leader.[4] As we show in appendix A on page ii, random assignment produced the expected degree of balance along covariates.

We estimate intent-to-treat (ITT) effects via the weighted least squares regression:

$$Y_{ij} = \theta T_j + \beta X_{ij} + \alpha_d + \epsilon_{ij}$$

where Y_{ij} denotes the outcome for respondent i in community j; T_j is an indicator for assignment to treatment; X_{ij} is a set of baseline covariates; α_d are district fixed effects; and ϵ_{ij} is an individual error term, clustered by community.[5]

3.1 Social desirability bias

If training induces social desirability bias, we will overestimate its impact. While this is a risk, our results are generally not consistent with social desirability bias: while some treatment effects resonate with the messages of the intervention (e.g. positive effects on norms discouraging the use of violence), others do not (e.g. null effects on norms encouraging empathy). These nulls provide reassurance that the significant effects we observe are not artifacts of social desirability bias alone. Moreover, given the length of time between the intervention and the 3-year endline, it seems very unlikely that social desirability bias persisted without any

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[3]UNHCR ran out of funds and stopped the intervention in Phase 4. To estimate treatment effects, we pool the Phase 5 communities into the control group. We also assigned 16 of the remaining 86 treatment communities (26 of the original 116) to an "intense" treatment, with 30 to 40% more workshops.

[4]For the 3-year endline we collected data in 204 of the 245 communities still in existence in 2012, dropping 40 control communities that added little to statistical power but much to survey costs. To do this, we stratified control communities into high and low similarity to treatment communities (based on baseline covariates), and high and low survey cost (based on distance). We randomly dropped 20 of the 40 lowest similarity communities, and 20 of the 40 highest cost communities. The other 20 in each strata are weighted by their inverse propensity of selection into the sample. Appendix C on page ii shows that dropping communities does not substantively change our treatment effect estimates.

[5]This specification differs from our previously published results in that we estimate an intent-to-treat effect (ITT) rather than a complier average causal effect (CACE). Our results are substantively unchanged regardless. In the 1-year analyses only, X also includes an indicator for a handful of Phase 4 treatment villages that were randomly assigned to be treated concurrently with the first endline survey.

change in actual underlying norms and skills.

3.2 Pre-registration and multiple comparisons

Both endlines were conducted before use of the social science registry became widespread, and our primary and secondary outcomes were not pre-registered. We use our previously published 1-year results to pre-specify primary outcomes for the 3-year analysis. The 3-year survey also added questions on a number of secondary outcomes, especially security of property rights and skills and norms of dispute resolution (see appendix L.1 on page xx). While these were not pre-specified, the previous paper identified them as outcomes of interest. Given the more exploratory nature of these secondary analyses, we present p-values with and without corrections for multiple outcomes within each class of outcomes (but not across all classes). We present results from two approaches: the Westfall et al. (1993) free step-down resampling method for the family-wise error rate (FWER), which captures the probability that at least one of the true null hypotheses will be falsely rejected, using randomization inference; and the more mechanical Sidak-Holmes method.

4 Results

4.1 Land disputes

Table 1 reports ITT effects on the incidence, severity, and resolution of land disputes. At each endline, we asked respondents whether they had any "serious dispute" over land in the past year. We then asked if the dispute had been resolved by the time of the survey. More than one-fifth of households reported a land dispute in the first endline; this fell to just under one-tenth by the three-year endline. While the training did not reduce the *incidence* of land disputes, it did increase the rate of resolution in the short term. After one year, the proportion of residents with an unresolved land dispute fell by 2 percentage points in treatment communities, a 28% reduction relative to control. These effects did not persist, however, and after three years we see no evidence of a continued increase in the rate of resolution, nor a decrease in the length of disputes. We previously speculated that the program was especially effective in resolving the most longstanding disputes, since the largest effect was in the subset of disputes that involved property taken during the civil war. That we fail to find an effect on dispute resolution after three years may reflect the fact that there were fewer of these longstanding disputes left to resolve.

We do, however, find evidence of a long-term reduction in threats and violence associated with land disputes. After one year, respondents in treatment communities were 1 percentage point less likely to report

Table 1: Effects on incidence, length, severity, and resolution of land disputes, 1- and 3-year endlines

Dependent Variable	1-year endline							3-year endline						
	Control mean (1)	N (2)	ITT (3)	ITT / control mean (%) (4)	Est. p-val (5)	WY Adj. p-val (6)	Holms Adj p-val (7)	Control mean (8)	N (9)	ITT (10)	ITT / control mean (%) (11)	Est. p-val (12)	WY Adj. p-val (13)	Holms Adj p-val (14)
Panel A: Ouctomes for all residents														
Any serious land dispute	0.221	5,435	0.003 [0.016]	1.2	0.867	0.887[a]	0.867	0.087	4,011	0.008 [0.011]	8.8	0.473	0.872[b]	0.854
Any unresolved land dispute	0.070	5,435	-0.020 [0.008]**	-28.0	0.015	0.080[a]	0.057	0.024	4,011	0.002 [0.005]	6.4	0.744	0.878[b]	0.854
Any threats, property damage, or violence	0.122	5,435	-0.010 [0.012]	-8.1	0.397	0.725[a]	0.781	0.041	4,011	-0.012 [0.006]**	-29.3	0.039	0.266[b]	0.182
Property damage or violence in land dispute	0.091	5,435	-0.012 [0.009]	-13.2	0.183			0.021	4,011	-0.007 [0.004]	-31.2	0.117		
Threats	0.114	5,435	-0.006 [0.011]	-5.3	0.596			0.035	4,011	-0.010 [0.006]*	-28.9	0.069		
Property damage	0.041	5,435	-0.013 [0.006]**	-32.4	0.029			0.010	4,011	-0.005 [0.003]*		0.072		
Violence	0.077	5,435	-0.007 [0.008]	-8.7	0.416			0.017	4,011	-0.004 [0.004]	-21.4	0.361		
Panel B: Conditional on a land dispute														
Length of maximum land conflict								13.247	353	3.642 [2.889]	27.5	0.209	0.680[b]	0.609
Resolved land dispute	0.684	1,212	0.072 [0.027]***	10.5	0.009	0.075[a]	0.045	0.668	353	-0.024 [0.046]	-3.6	0.604	0.878[b]	0.854
Any threats, property damage, or violence	0.554	1,212	-0.024 [0.035]	-4.3	0.496	0.773[a]	0.781	0.476	353	-0.192 [0.047]***	-40.4	0.000	0.008[b]	0.000
Property damage or violence	0.411	1,212	-0.037 [0.030]	-9.1	0.213			0.243	353	-0.090 [0.042]**	-37.2	0.035		
Threats	0.515	1,212	-0.013 [0.035]	-2.5	0.713			0.408	353	-0.157 [0.048]***	-38.6	0.001		
Property damage	0.186	1,212	-0.051 [0.025]**	-27.4	0.041			0.114	353	-0.067 [0.027]**	-58.5	0.016		
Violence	0.349	1,212	-0.022 [0.028]	-6.3	0.442			0.202	353	-0.056 [0.042]	-28.0	0.186		
Witchcraft								0.065	353	0.035 [0.026]	54.3	0.182		

Notes: Intent-to-treat (ITT) effects with baseline covariates and district fixed effects.. Estimates are weighted by the inverse probability of sampling. Standard errors are clustered by community. Columns (6) and (13) show Westfall-Young multiple hypothesis corrected p-values for the 1 and 3-year endlines, respectively,
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
[a] indicates multiple hypothesis corrections within the family of 1-year results.
[b] indicates multiple hypothesis corrections within the family of 3-year results.

a dispute involving threats or violence (an 8% decrease relative to the control group), but this decline was generally not statistically significant. After three years, these treatment effects are larger and more robust. Respondents in treatment communities were 1 percentage point (29%) less likely to report threats or violence. Conditional on having a land dispute, respondents were 19 percentage points (40%) less likely to report threats, property destruction, or violence, and 9 percentage points (37%) less likely to report property destruction or violence. While the unconditional effects are generally no longer significant after correcting for multiple comparisons, the (arguably less noisy) conditional effects remain strongly significant (p<0.001). In Appendix D.3 we show that training had similar effects on threats and violence related to other types of disputes as well.

4.2   Security of property rights

We measured four dimensions of property rights over particular plots of land in the 3-year endline: inheritance, alienation, collateralization, and demarcation. We also collected data on farm size, willingness to leave farmland fallow—an important indicator of property rights security in sub-Saharan Africa (Goldstein and Udry 2008)—capital improvements (such as fences, gutters, and grass), and money and time spent

Table 2: Effects on investment and security of property rights, 3-year endline

Dependent Variable	N (1)	Control mean (2)	ITT (3)	Est. p-val (4)	Wy adj. p-val (5)	Holms adj. p-val (6)
Security rights index, z-score	4,011	0.045	-0.085 [0.037]**	0.024	0.120	0.071
Improvement index, z-score	4,011	0.023	-0.066 [0.037]*	0.079	0.207	0.151
Index of fallow land for farm	3,666	0.003	-0.004 [0.044]	0.926	0.921	0.926
Size of farm	3,598	37.481	2.773 [1.051]***	0.009	0.064	0.036

Notes: 3-year *Notes:* Intent-to-treat (ITT) effects with baseline covariates and district fixed effects.. Estimates are weighted by the inverse probability of sampling. Standard errors are clustered by community. Unlike results in Tables 1 and D.3, results in this table are at the plot-level, where there are two types of plots - house or farm. Not all respondents tend to plots, which causes varying Ns across variables.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

on improvements. We use these measures to construct standardized indices of security of property rights, improvements, and fallowing, as well as a measure of farm size.

Table 2 reports effects on these outcomes, which are mixed. On the one hand, treatment community residents reported farm plots that were 2.75 acres larger than the control community average. On the other hand, farm and house plots in treatment communities scored .08 standard deviations *lower* on our index of security of property rights. Respondents in treatment communities also scored .065 standard deviations lower on our improvement index. The coefficient on our fallowing index is negative as well, though substantively small and not statistically significant. These results are inconsistent with expectations from our previous study, though they are sensitive to multiple comparisons corrections, and so should be interpreted with some caution.

In the appendix we explore potential sources of treatment effect heterogeneity (see appendix E). While our results are mixed, they provide suggestive evidence that some of the adverse effects on security of property rights are concentrated among residents who do not enjoy connections to local political power. ADR scholars worry that because bargains reached informally are often difficult to enforce, they may privilege wealthier, more connected disputants who can muster financial and political resources to ensure favorable outcomes (Sternlight 2006). Although the ADR training targeted community members broadly, the delivery may have inadvertently favored those with strong connections to local leaders (a challenge for many NGO programs that must work with local leaders for access). Our results lend some credence to these concerns.

Table 3: Effect on norms, attitudes and skills, 3-year endline

Dependent Variable	N (1)	Control mean (2)	ITT (3)	Est. p-val (4)	Wy adj. p-val (5)	Holms adj. p-val (6)
Index of all norms combined	4,011	-0.026	0.028 [0.038]	0.469	0.856	0.895
Bias index	4,011	-0.009	-0.002 [0.046]	0.962	0.997	0.997
Defection index	4,011	-0.043	0.045 [0.041]	0.274	0.796	0.853
Empathy index	4,010	0.002	0.030 [0.033]	0.363	0.856	0.895
Forum choice index	4,011	-0.028	0.031 [0.037]	0.400	0.856	0.895
Managing emotions index	4,011	-0.031	0.067 [0.031]**	0.032	0.267	0.228
Mediation index	4,011	0.003	-0.062 [0.037]*	0.094	0.489	0.499
Negotiation index	4,011	0.002	0.002 [0.027]	0.945	0.997	0.997

Notes: 3-year intent-to-treat (ITT) effects with baseline covariates and district fixed effects. Estimates for 3-year endline residents are weighted by the inverse probability of sampling. Standard errors are clustered by community.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

4.3 Norms and skills

To better understand how the ADR campaign worked, we asked both residents and local leaders about their experiences and perspectives on informal dispute resolution as it was taught during the intervention. We focused on descriptive social norms around dispute resolution, such as whether it is the norm for people to try to resolve disputes directly; respondents' personal beliefs about these norms, such as their opinion of resolving a dispute directly with the other party;[6] and respondents' dispute resolution skills, such as whether they tried to resolve a dispute directly with another party in the last year. When measuring social norms, we use the community as the reference group.

Table 3 reports effects on indices of these norms and skills, grouped by theme. More positive values indicate greater alignment with the messages of the training. We find some suggestive evidence that norms around mediation are actually *weaker* in treatment communities, though this effect is only marginally statistically significant. We do, however, find somewhat stronger evidence that treatment community residents internalized norms around managing emotions and avoiding violence. While these analyses are exploratory, and are sensitive to multiple comparisons corrections, they are consistent with our finding that the program

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[6]On the difference between descriptive social norms and personal beliefs, see Paluck and Ball (2010).

did not reduce the incidence, length, or resolution of disputes in the long term, but did mitigate violence.

5 Conclusion

As in most developing countries, property and other kinds of disputes are endemic in rural Liberia. We find that a relatively short but intensive ADR training program did not affect the incidence of disputes, but did reduce the likelihood of violence associated with them. These effects last at least three years, suggesting behavior is malleable over the long term. While the program was expensive and not especially cost effective (see Appendix G), lower-cost alternatives may be feasible (e.g. through radio or television), and are worth exploring. Moreover, the costs of even this relatively expensive program were likely lower than the costs of formal justice sector reform, though we do not have the data to say.

In the meantime, our results suggest that ADR can yield a lasting reduction in violence over time, perhaps through changes in norms and skills related to emotional regulation and the appropriateness of violence. Our study also demonstrates the benefits of testing the long-term effects of such interventions. These programs are cost- and labor-intensive, and aim to deliver lasting changes that may not be immediately detectable. Most evaluations assess only short-term effects, generating little knowledge about whether such investments lead to persistent shifts in behavior. Finding cost-effective ways to assess the long-term impact of programs, successful or not, will help to produce clearer guidance on how best to invest in durable change.

6 Acknowledgments

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Appendix for Online Publication

Contents

A Descriptive statistics and balance test

Table A.1 reports means for a selection of 41 baseline covariates by experimental assignment. For the most part, background attributes appear balanced across experimental conditions. There are some minor differences between treatment and control communities: 3 of the 41 mean differences (7%) have $p < .1$. Overall the imbalance is consistent with chance and is robust to alternative balance tests.

B Random route sampling method

We sampled survey respondents using the random route method. Typically there is no village census from which to sample in rural Liberia. Thus interviewers were assigned a starting location (the center of the village, or the center of a town quarter in larger towns) and provided with a set of random walking rules: to spin a pen to select a direction, to use a paper-based random number generator to choose house number n, to select the nth house in that direction for a survey, and to select an adult in that household to interview at random. This does not produce a representative sample of the community, but it does sample community members similarly across the treatment arms.

C Effects of randomly dropping 40 control communities from the sample at the 3-year endline

The experiment was imbalanced in terms of the number of treatment and control communities, with 159 control communities and 86 treated ones. These excess control communities contributed significantly to data collection costs but did little for statistical power. Thus we stratified the control communities by survey cost and similarity to treated communities and dropped 40 communities within two strata.

Table C.1 illustrates the largely negligible effects of this dropping using our 1-year endline survey data. First, as a reference, Column (2) shows the ITT estimate under the main specification for the 1-year follow-up. (These results are the same as in Table 1.) Column (3) replicates this analysis using a reduced sample of only the communities that were not dropped between the 1 and 3-year follow ups. The specification in Column (3) also uses the same weights as in the 3-year follow-up regressions. Significance levels hold and coefficients only change very slightly. For instance, the ITT effect of treatment assignment on *Any unresolved land dispute* is .020 in both the full sample and the reduced sample. Results are similar when the reduced sample is used without 3-year endline weights (not presented).

Column (4) in Table C.1 tests whether there is any correlation between being dropped and any of our outcomes. The independent variable in these regressions is a dummy indicating whether the community was dropped before the 3-year endline. Only one coefficient out of 17 is statistically significant, and only weakly so.

Finally, Table C.2 presents a balance test comparing communities that were dropped from the 3-year endline to those that were kept. Of the 43 covariates we test, the difference between dropped and kept communities is statistically significant at the 5% level or lower for just three. An additional four differences are significant at the 10% level, and are generally substantively small. This degree of balance provides further reassurance that dropping is unlikely to have affected our results.

Table A.1: 2008 baseline summary statistics and test of randomization balance

Panel A: Resident-level balance

Baseline covariate	Mean			Regression difference	
	All	Control	Treatment	Coeff.	p-value
	(1)	(2)	(3)	(4)	(5)
Resident demograhics					
Age	40.65	40.88	40.25	-0.50	0.44
Male	0.56	0.57	0.56	-0.00	0.91
Years of education	5.21	5.29	5.07	-0.04	0.87
Muslim	0.12	0.12	0.12	-0.01	0.76
Traditional religion	0.05	0.05	0.04	-0.01	0.27
christian	0.84	0.83	0.84	0.02	0.52
Wealth index	-0.01	-0.01	-0.01	0.05	0.29
Landless	0.13	0.13	0.15	0.01	0.50
No farm	0.07	0.06	0.08	0.01	0.54
Prior peace education	0.28	0.27	0.30	0.03	0.32
Participating in a peace group	0.40	0.41	0.37	-0.03	0.19
Resident war experience					
Refugee	0.58	0.58	0.58	-0.02	0.48
Displaced	0.43	0.40	0.48	0.08	0.01**
Violence experience index (0-13)	4.24	4.30	4.15	-0.19	0.33
Resident land and interpersonal conflict					
House spot or farmland taken during war	0.10	0.10	0.10	0.00	0.96
Any land conflict since end of war	0.24	0.26	0.22	-0.03	0.20
Had a money conflict in 2008	0.09	0.08	0.09	0.01	0.41
Any burglary or armed robbery in 2008	0.13	0.14	0.13	-0.01	0.59
Victim of witchcraft in 2008	0.09	0.10	0.07	-0.02	0.16
Had a dispute at a water source in past 6 mo.	0.08	0.08	0.08	0.00	0.70

Panel B: Community-level balance

Baseline covariate	Mean			Regression difference	
	All	Control	Treatment	Coeff.	p-value
	(1)	(2)	(3)	(4)	(5)
Town population	2,026.13	2,009.82	2,055.72	87.60	0.60
Town education level	5.16	5.22	5.05	-0.06	0.82
Number of tribes in town	2.62	2.46	2.93	0.48	0.03**
% of community members in largest tribe	0.82	0.83	0.80	-0.03	0.31
Town wealth index	-0.02	-0.03	-0.01	0.05	0.30
# of services available in town (0-14)	5.61	5.70	5.45	-0.23	0.52
# of resources within 2 hrs of comm. (0-5)	1.45	1.41	1.52	0.11	0.28
Distance to nearest road in hours, rainy season	1.07	1.08	1.07	-0.02	0.92
Town exposure to war violence	4.28	4.33	4.19	-0.16	0.43
% of town losing land during war	0.10	0.11	0.10	-0.00	0.73
Index of progressive political beliefs in town	3.85	3.85	3.84	-0.01	0.88
Index of progressive ethnic attitudes in town	5.78	5.81	5.73	-0.09	0.55
% of town with prior peace education	0.28	0.27	0.30	0.03	0.23
% of town participating in a peace group	0.40	0.41	0.38	-0.03	0.33
% of town accepting of inter-religious marriage	0.66	0.66	0.67	0.01	0.59
% of town reporting assault	0.19	0.19	0.18	-0.01	0.56
% of town reporting a land dispute	0.24	0.25	0.22	-0.03	0.33
% of town reporting witchcraft victimization	0.09	0.10	0.07	-0.03	0.09*
% of town reporting	0.05	0.05	0.06	0.01	0.61
% of town reporting violent dispute at water source	0.07	0.07	0.06	-0.01	0.74
Violent strike or ethnic dispute in town	0.10	0.12	0.07	-0.05	0.20
Witch killing or tral by ordeal in town	0.09	0.09	0.10	0.02	0.68
Rape or murder in town	0.15	0.15	0.16	0.02	0.74

Table C.1: Effect of decision to drop communities on Endline 1 main outcomes

	Mean full sample (1)	Effect of Treatment Full sample (2)	Effect of Treatment Reduced sample (3)	Effect of Dropping (4)	Effect as % of control mean Full sample (5)	Effect as % of control mean Reduced sample (6)	Effect as % of control mean Dropping (7)
Panel A: Land dispute outomes for all residents							
Any serious land dispute	0.221	0.003 [0.016]	0.002 [0.018]	0.001 [0.023]	1.24	1.00	0.27
Any unresolved land dispute	0.070	-0.020 [0.008]**	-0.020 [0.008]**	-0.011 [0.010]	-27.97	-28.63	-16.62
Any threats, property damage, or violence	0.122	-0.010 [0.012]	-0.011 [0.014]	0.001 [0.014]	-8.10	-8.95	0.54
Property damage or violence in land dispute	0.091	-0.012 [0.009]	-0.013 [0.010]	0.002 [0.013]	-13.16	-14.39	2.64
Threats	0.114	-0.006 [0.011]	-0.006 [0.013]	0.001 [0.014]	-5.27	-5.23	1.19
Property damage	0.041	-0.013 [0.006]**	-0.018 [0.007]***	-0.017 [0.010]*	-32.40	-40.65	-49.25
Violence	0.077	-0.007 [0.008]	-0.006 [0.009]	0.007 [0.011]	-8.73	-8.13	8.53
Panel B: Conditional on a land dispute							
Resolved land dispute	0.684	0.072 [0.027]***	0.076 [0.029]***	0.032 [0.040]	10.53	11.13	4.55
Any threats, property damage, or violence	0.554	-0.024 [0.035]	-0.012 [0.037]	0.016 [0.043]	-4.33	-2.22	2.72
Property damage or violence	0.411	-0.037 [0.030]	-0.027 [0.031]	0.028 [0.043]	-9.05	-6.54	6.57
Threats	0.515	-0.013 [0.035]	-0.002 [0.037]	0.013 [0.039]	-2.50	-0.31	2.39
Property damage	0.186	-0.051 [0.025]**	-0.056 [0.026]**	-0.058 [0.038]	-27.37	-28.46	-35.85
Violence	0.349	-0.022 [0.028]	-0.011 [0.030]	0.039 [0.036]	-6.26	-3.17	10.87
Panel C: General dispute outcomes for all residents							
Any serious dispute	0.299	0.022 [0.018]	0.021 [0.019]	-0.016 [0.023]	7.22	6.99	-5.57
Any unresolved dispute	0.118	-0.013 [0.011]	-0.011 [0.012]	-0.014 [0.014]	-11.01	-9.19	-12.54
Panel D: Conditional on a dispute							
Resolved dispute	0.676	0.051 [0.024]**	0.047 [0.024]*	0.013 [0.038]	7.55	6.88	1.84
Resolved via informal mechanism	0.247	0.018 [0.025]	0.018 [0.026]	0.025 [0.038]	7.42	7.34	9.10

Column (2) shows results with the 1-year specification like all tables above. Column (3) uses only non-dropped communities and includes the same weights as used in the 3-year endline specification.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.2: 2008 baseline summary statistics for dropped and non-dropped communities

Panel A: Resident-level balance

Baseline covariate	Mean			Regression difference	
	All	Kept	Dropped	Coeff.	p-value
	(1)	(2)	(3)	(4)	(5)
Resident demograhics					
Age	40.65	40.67	40.55	-0.44	0.65
Male	0.56	0.56	0.56	-0.01	0.67
Years of education	5.21	5.23	5.12	-0.24	0.43
Muslim	0.12	0.12	0.10	-0.02	0.62
Traditional religion	0.05	0.05	0.04	-0.02	0.11
christian	0.84	0.83	0.86	0.04	0.33
Wealth index	-0.01	0.02	-0.19	-0.24	0.00***
Landless	0.13	0.14	0.12	-0.01	0.76
No farm	0.07	0.07	0.07	0.01	0.70
Prior peace education	0.28	0.28	0.30	0.03	0.34
Participating in a peace group	0.40	0.39	0.42	0.01	0.83
Resident war experience					
Refugee	0.58	0.58	0.59	0.01	0.81
Displaced	0.43	0.44	0.38	-0.02	0.67
Violence experience index (0-13)	4.24	4.17	4.62	0.44	0.12
Resident land and interpersonal conflict					
House spot or farmland taken during war	0.10	0.10	0.09	-0.01	0.51
Any land conflict since end of war	0.24	0.24	0.23	-0.03	0.45
Had a money conflict in 2008	0.09	0.09	0.09	0.01	0.69
Any burglary or armed robbery in 2008	0.13	0.13	0.15	0.02	0.54
Victim of witchcraft in 2008	0.09	0.08	0.12	0.03	0.19
Had a dispute at a water source in past 6 mo.	0.08	0.07	0.10	0.03	0.10*

Panel B: Community-level balance

Baseline covariate	Mean			Regression difference	
	All	Kept	Dropped	Coeff.	p-value
	(1)	(2)	(3)	(4)	(5)
Town population	2,026.13	2,047.29	1,916.03	-125.06	0.55
Town education level	5.16	5.17	5.11	-0.07	0.83
Number of tribes in town	2.62	2.63	2.58	0.17	0.54
% of community members in largest tribe	0.82	0.82	0.81	-0.02	0.55
Town wealth index	-0.02	0.01	-0.20	-0.22	0.00***
# of services available in town (0-14)	5.61	5.63	5.54	-0.21	0.65
# of resources within 2 hrs of comm. (0-5)	1.45	1.41	1.63	0.29	0.03**
Distance to nearest road in hours, rainy season	1.07	1.03	1.32	0.32	0.29
Town exposure to war violence	4.28	4.19	4.73	0.51	0.06*
% of town losing land during war	0.10	0.10	0.11	0.01	0.77
Index of progressive political beliefs in town	3.85	3.87	3.71	-0.18	0.06*
Index of progressive ethnic attitudes in town	5.78	5.75	5.97	0.22	0.32
% of town with prior peace education	0.28	0.28	0.29	0.03	0.36
% of town participating in a peace group	0.40	0.39	0.41	0.01	0.79
% of town accepting of inter-religious marriage	0.66	0.66	0.68	0.04	0.32
% of town reporting assault	0.19	0.17	0.25	0.08	0.07*
% of town reporting a land dispute	0.24	0.24	0.25	-0.01	0.82
% of town reporting witchcraft victimization	0.09	0.08	0.14	0.05	0.12
% of town reporting	0.05	0.05	0.07	0.02	0.11
% of town reporting violent dispute at water source	0.07	0.06	0.08	0.01	0.85
Violent strike or ethnic dispute in town	0.10	0.10	0.13	0.01	0.87
Witch killing or tral by ordeal in town	0.09	0.09	0.10	0.02	0.74
Rape or murder in town	0.15	0.17	0.08	-0.09	0.09*

D   Average treatment effects on land, money, and interpersonal disputes

In the manuscript we show that ADR training reduced the incidence of threats and violence associated with land disputes. We observe similar patterns looking at land, money, and interpersonal disputes together, in Table D.3. After one year, 30% of control community respondents reported at least one of these disputes. We see roughly the same number of disputes after three years, though this may be due to our inclusion of a much more detailed measure of domestic disputes in the 3-year endline.

After one year, respondents in treatment communities were 2 percentage points more likely to report a dispute, an increase of 7% relative to the control group, though this difference was not statistically significant. Disputants in treatment communities were also 5 percentage points (7.5%) more likely to report a resolution. After three years, as with land disputes, we see no statistically significant increase in disputes or resolutions.

We do, however, see a long-run reduction in dispute severity. After three years, one-third of disputes in control communities involved property destruction or interpersonal violence; in treatment communities, this proportion dropped by almost 7 percentage points—a 21% decline relative to control. This effect remains statistically significant or nearly so after correcting for multiple comparisons in the entire column.

E   Heterogeneous treatment effects on security of property results

What explains the apparently adverse effects on improvements and security of property rights that we observe in the manuscript? Most informal institutions emerge endogenously over time, and may serve purposes that internationally funded NGOs and other third parties may not consider acceptable—for example, entrenching the power of already privileged groups (Helmke and Levitsky 2004). Indeed, as discussed in the manuscript and in our previous study, informal institutions in Liberia (and in other developing countries) often favor the powerful, e.g. those with wealth or connections to local leadership.

This treatment was designed to deal with specific failures in existing informal (and formal) institutions. These included communication breakdown (leading to information asymmetries) and forum shopping, both of which undermined enduring bargains. The program also sought to be accessible to all community members, but it did not aim to explicitly change existing power dynamics.

An exogenously allocated ADR program brought to communities by an NGO will inevitably interact with these existing power structures. The degree to which ADR complements or competes with them (Helmke and Levitsky 2004) may determine whether existing power structures are strengthened by the program, potentially to the benefit of those who already occupy privileged positions (e.g. Goldstein and Udry 2008).

The ADR program we evaluate aimed to induce exogenous behavioral change, but one of the key reasons for its feasibility, legitimacy, and success was the way it complemented existing informal institutions, as opposed to replacing them with new ones. Indeed, some of the "new" ADR norms and skills that implementers sought to deliver, including talking through disputes and using social pressure to enforce bargains, existed prior to the program. While the program emphasized equitable treatment of all residents, formal protections of rights (often required to protect vulnerable groups, e.g. Edwards 1986) were outside the scope of the program. To the extent that existing informal institutions in Liberia actively discriminated against certain groups (Isser et al. 2009), the ADR program may have helped to perpetuate marginalization.

Moreover, while an explicit goal of the program was to treat enough people that ADR norms and skills would be accessible to all residents, obtaining access to communities required the consent and even support of

Table D.3: Program impacts on number, length, severity, and resolution of all dispute types

	1-year endline							3-year endline						
Dependent Variable	Control mean (1)	N (2)	ITT (3)	ITT / control mean (%) (4)	Est. p-val (5)	WY Adj. p-val (6)	Holms Adj p-val (7)	Control mean (8)	N (9)	ITT (10)	ITT / control mean (%) (11)	Est. p-val (12)	WY Adj. p-val (13)	Holms Adj p-val (14)
Panel A: Outcomes for all residents														
Any serious dispute	0.299	5,435	0.022 [0.018]	7.2	0.220	0.533[a]	0.525	0.306	4,011	0.012 [0.017]	4.1	0.456	0.920[b]	0.912
Any unresolved dispute	0.118	5,435	-0.013 [0.011]	-11.0	0.250	0.533[a]	0.525	0.064	4,011	-0.004 [0.009]	-6.6	0.623	0.920[b]	0.912
Any threats, property damage, or violence								0.101	4,011	-0.015 [0.010]	-15.2	0.111	0.513[b]	0.444
Panel B: Conditional on dispute														
Resolved dispute	0.676	1,670	0.051 [0.024]**	7.5	0.033	0.164[a]	0.125	0.767	1,227	-0.019 [0.026]	-2.5	0.467	0.920[b]	0.912
Resolved via informal mechanism	0.247	1,671	0.018 [0.025]	7.4	0.470	0.533[a]	0.525	0.409	1,227	-0.019 [0.027]	-4.6	0.487	0.920[b]	0.912
Any threats, property damage, or violence								0.331	1,227	-0.069 [0.026]***	-20.7	0.009	0.102[b]	0.053
Property damage or violence								0.190	1,227	-0.027 [0.022]	-14.1	0.222		
Threats								0.274	1,227	-0.072 [0.024]***	-26.2	0.003		
Property damage								0.048	1,227	-0.019 [0.011]*	-40.0	0.085		
Violence								0.173	1,227	-0.018 [0.021]	-10.2	0.411		

Notes: Intent-to-treat (ITT) effects with baseline covariates and district fixed effects.. Estimates are weighted by the inverse probability of sampling. Standard errors are clustered by community. Columns (6) and (13) show Westfall-Young multiple hypothesis corrected p-values for the 1 and 3-year endlines, respectively,
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
[a] indicates multiple hypothesis corrections within the family of 1-year results.
[b] indicates multiple hypothesis corrections within the family of 3-year results.

local leaders. Given the difficulty of instilling new skills and norms and the high cost of enforcing compliance with them, the program's success may have depended on the extent to which these local leaders and other powerful individuals believed they would benefit from it (Veit 2011). The presence of a foreign-funded NGO may have also lent further legitimacy to the local leaders that hosted it and allowed it to operate in their communities (Autesserre 2010). If the most powerful residents felt emboldened by the program, this may have led them them to impose unfair solutions to disputes involving less powerful members of the community.

   If this is the case, then the declines in security and property improvements should be concentrated among those with the least power in their communities. To explore this possibility, Table E.4 reports three separate heterogeneity analyses, interacting treatment with three measures of status related to land (see Appendix I.1 on page xiv and Appendix I.2 on page xv for additional analyses):

1. An indicator for strong political connections (measured as a kin relationship with the town chief),

2. An indicator for market-based ownership of the plot (i.e. whether the respondent acquired ownership through a market transaction, which tends to endow stronger rights than customary allocation), and

3. An indicator for owning land, rather than renting or borrowing.

We measured these variables with the intention of studying heterogeneity, but they were not pre-specified. Hence we must interpret results with some caution. Moreover, political connections are likely endogenous to other individual characteristics, including gender, age, and ethnicity. We report heterogeneous treatment effects along these dimensions in Appendix I.1, and focus on our three more direct measures of political connections here.

   Treatment effects on security of property rights do appear to depend on status, albeit only weakly. With the addition of the interaction terms above, the coefficient on the treatment indicator is now interpreted as the effect on the low-powered, and the coefficient on the interaction term is the difference between the low and high-powered. Thus a negative treatment effect and a positive interaction suggests that low-powered individuals bear the brunt of the decline in property security. This is generally what we see, though the interaction is not always statistically significant. The fall in security of property rights is concentrated among politically unconnected individuals, those without market-based tenure, and those who do not own their land (Column 1, the effect of *Treatment* in all three panels). The effects on security of property rights are null among residents with political connections, those with market-based tenure, and those who own their land (Column 1, *Sum of Treatment and Interaction* in all three panels).

   The other results are more mixed. Relative to control, all treatment community residents were less likely to make improvements to their plots, regardless of their status. And all treatment group residents had larger farm plots relative to control, though this effect is more pronounced among politically connected individuals (Column 3, first panel) and those with market tenure (Column 3, second panel). Interestingly, this effect is also more pronounced among those who do not own their land (Column 3, third panel), perhaps because renting land is less expensive than buying.

   In the long term, engineering informal institutions seems to have strengthened the property rights of some over others, resulting in a net *decrease* in security of property rights in treatment communities. We did not anticipate this result, and did not collect specific qualitative data that might help explain it. However, political power structures property rights, and the program was unlikely to change local power structures. Appendix Table I.4 on page xvi shows that at the 3-year endline, fewer low-powered individuals reported inheritance rights over houses and farms, or the right to conduct a property survey. This descriptive information gives some initial idea of the scope of adverse effects, and suggests that an analysis of how ADR

Table E.4: Heterogeneity in land security and investment, 3-year endline

Independent variable	Security rights index, z-score (1)	Property investment index, z-score (2)	Size of land (house and farm) (3)
Is politically connected			
Treatment	-0.126	-0.046	2.235
	[0.037]***	[0.031]	[1.137]**
Covariate	0.005	0.069	0.916
	[0.042]	[0.038]*	[1.370]
Treatment x Covariate	0.140	-0.022	1.496
	[0.058]**	[0.054]	[1.956]
Sum of Treatment and Interaction	0.014	-0.068	3.731
	[0.047]	[0.046]	[1.653]**
Has market Tenure			
Treatment	-0.129	-0.013	2.689
	[0.063]**	[0.050]	[2.082]
Covariate	0.543	0.126	-5.666
	[0.046]***	[0.037]***	[1.644]***
Treatment x Covariate	0.083	-0.044	-0.152
	[0.067]	[0.054]	[2.303]
Sum of Treatment and Interaction	-0.046	-0.056	2.537
	[0.031]	[0.029]**	[1.057]**
Owns own land			
Treatment	-0.087	-0.049	3.247
	[0.031]***	[0.028]*	[1.034]***
Covariate	0.309	0.008	7.207
	[0.053]***	[0.044]	[1.710]***
Treatment x Covariate	0.058	-0.017	-3.106
	[0.076]	[0.064]	[2.513]
Sum of Treatment and Interaction	-0.030	-0.067	0.141
	[0.072]	[0.060]	[2.327]

Notes: 3-year heterogeneous treatment effects by political connectedness, market tenure, and land ownership, with baseline covariates and district fixed effects. Estimates are weighted by the inverse probability of sampling. Standard errors are clustered by community. Like table 2, results are plot-level.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

programs shape intra-household bargaining warrants future exploration. The effects on investment are more mixed.

It is not clear whether endogenous informal institutions are more or less likely to be inclusive compared with those implemented by third parties. On the one hand, inclusive change may only be possible and sustainable when residents can effectively apply pressure on local power structures. Residents' ability to apply pressure of this sort may depend on even longer-run social and political dynamics that external interventions are simply unable to change. On the other hand, third parties may be able to pressure locally powerful individuals to reform, or may be able to introduce inclusive systems more quickly and effectively themselves. We leave these possibilities for future research to explore.

F   Average treatment effects on community-level disputes

The primary focus of the intervention was on household-level disputes, and statistically we are far more powered for such disputes becaue of the larger number of observations. But the implementing organizations were hopeful that the intervention would impact common community-level disputes as well. Also, the existing literature and our theory raised concerns about unintended consequences.

Table F.1 reports impacts on these community-level disputes. After one year, the ADR campaign led to increases in witch hunts and disputes between elders and youths. Youth-elder disputes increased by almost 40% (or 4 percentage points) over the control group mean of 11%. More striking, witch hunts nearly doubled, from 1.5% to 3.8%—an increase of 153%. In our previous study we interpreted this as troubling evidence that greater informality can have the unintended consequence of encouraging communities to adjudicate disputes outside the bounds of the law. These two effects together drove a 19% (8.5 percentage point) increase in the prevalence of any violence at the community-level.

These effects did not endure over time. The difference in community-level disputes between treatment and control communities is no longer significant after three years. If anything, in most cases disputes declined (though most of these results are not statistically significant). For example, incidents of intertribal violence, already a rare event, fell by 1.6 percentage points (78%) in treatment communities. Witch hunts fell by .8 percentage points (72%) in treatment communities as well. We view this latter result as especially important given the spike in witch hunts after one year.

On the other hand, violent strikes and protests increased by 1.6 percentage points, which represents a substantively huge (albeit only marginally statistically significant) 912% increase over the control group mean. Peaceful strikes and protests increased as well, though not statistically significantly. It is possible that the program induced treatment community residents to be more proactive in protesting actions they disapproved of—even violently.[7] But this is speculative, and ultimately we cannot be sure. Whatever the explanation, these community-level results suggest that the potential for adverse effects continues to be a serious concern, even as individual-level disputes around land, money, and other interpersonal issues become increasingly peaceful over time.

Table F.1: Effect on community-level disputes

	1-year endline							3-year endline						
Dependent Variable	Control mean (1)	N (2)	ITT (3)	ITT / control mean (%) (4)	Est. p-val (5)	WY Adj. p-val (6)	Holms Adj p-val (7)	Control mean (8)	N (9)	ITT (10)	ITT / control mean (%) (11)	Est. p-val (12)	WY Adj. p-val (13)	Holms Adj p-val (14)
Any Violence	0.442	940	0.085 [0.037]**	19.3	0.021	0.278[a]	0.193	0.622	971	-0.057 [0.057]	-9.1	0.319	0.709[b]	0.735
Level of community violence	0.790	940	0.152 [0.083]*	19.2	0.070	0.558[a]	0.478	0.984	971	-0.131 [0.096]	-13.3	0.171	0.601[b]	0.677
Intertribal violence	0.028	940	0.008 [0.013]	30.0	0.512	0.903[a]	0.884	0.021	971	-0.016 [0.008]**	-77.8	0.039	0.451[b]	0.331
Violent strike or protest	0.061	940	-0.004 [0.016]	-7.1	0.782	0.955[a]	0.952	0.002	971	0.015 [0.009]	912.4	0.104	0.601[b]	0.585
Youth-elder dispute	0.110	940	0.044 [0.029]	40.2	0.124	0.676[a]	0.603	0.103	967	0.003 [0.023]	3.4	0.880	0.879[b]	0.880
Peaceful strike or protest	0.100	940	0.037 [0.025]	37.1	0.144	0.676[a]	0.606	0.059	971	0.010 [0.019]	16.0	0.613	0.878[b]	0.850
Interfamily land disputes	0.274	940	0.030 [0.037]	11.1	0.408	0.892[a]	0.878	0.548	971	-0.071 [0.066]	-12.9	0.282	0.696[b]	0.735
Conflicts with other towns	0.154	940	-0.005 [0.033]	-3.5	0.870	0.955[a]	0.952	0.171	970	-0.038 [0.029]	-22.2	0.194	0.618[b]	0.677
Witch hunts	0.015	940	0.023 [0.013]*	153.3	0.087	0.624[a]	0.517	0.011	971	-0.008 [0.005]*	-71.8	0.085	0.590[b]	0.551
Trial by ordeal	0.048	940	0.019 [0.018]	39.4	0.298	0.849[a]	0.830	0.070	971	-0.027 [0.018]	-39.1	0.120	0.601[b]	0.592

Notes: Intent-to-treat (ITT) effects with baseline covariates and district fixed effects.. Estimates are weighted by the inverse probability of sampling. Standard errors are clustered by community. Columns (6) and (13) show Westfall-Young multiple hypothesis corrected p-values for the 1 and 3-year endlines, respectively,
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
[a] indicates multiple hypothesis corrections within the family of 1-year results.
[b] indicates multiple hypothesis corrections within the family of 3-year results.

Table G.2: Estimated aggregate effects of the program on violent disputes among the 30,000 households in treatment communities: land disputes only

	ATE		Net effect		Net Effect imputed
Dependent Variable	1-year endline (1)	3-year endline (2)	1-year endline (3)	3-year endline (4)	for missing year (5)
Any threats, property damage, or violence	-0.010	-0.012	-303	-325	-942
Threats	-0.006	-0.010	-183	-275	-687
Property damage or violence in land dispute	-0.012	-0.007	-365	-177	-813
Property damage	-0.013	-0.005	-406	-139	-817
Violence	-0.007	-0.004	-206	-101	-459
Property damage + violence (land)	-0.020	-0.009	-611	-240	-1,276

Notes: Regression estimates use the same specification as in Table 1. 3-year residents are weighted by the inverse probability of sampling. Standard errors are clustered by community.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

G   Aggregate impacts

How much violence did the program actually reduce, and at what cost? We can approximate the aggregate impact on violence by multiplying our treatment effects at each endline by the roughly 30,000 households living in the 86 treatment communities. We do not have an estimate for the year between the two endlines, but can interpolate that gap year by multiplying the total disputes by 1.5.[8]

Table G.2 reports our results. These are conservative estimates in the sense that we only know if at least one dispute resulted in a violent act. We do not know if more than one violent act of the same type occurred, and these aggregate effects assume they did not. Also, we interviewed one adult per household, about all household disputes. But households had 8 members on average, with multiple adults, and to the extent that the respondent forgot or was not aware of a brother's, spouse's or parent's dispute, they may have underreported violence.

We estimate that the program resulted in 671 fewer threats and 805 fewer disputes with property destruction or violence. If we treat property destruction and violence as separate incidents, we estimate that the program led to 1,269 fewer acts of violence (excluding threats). The program cost $1.2 million to implement in all 86 villages.[9] This translates to a cost of $946 for every act of property destruction or violence. This is more than twice Liberia's income per capita. Even if lives are saved, this is likely to be considerably more expensive than other potentially life-saving foreign aid investments.

H   Intent-to-treat effects for intense treatment

16 of the 85 treatment communities received an intense treatment of 30-40% more workshops. Table H.1 reports impacts by intensity. At the 1-year endline we saw little effect of treatment intensity on our main outcomes of interest. At the 3-year endline we find that intense treatment leads to a reduction in treatment respondents reporting threats or violence.

I   Heterogeneous treatment effects

Table I.1 reports heterogeneous treatment effects on land disputes and resolution along gender, age, wealth, religion, ethnicity, prior (pre-treatment) exposure to peace education, and current (post-treatment) membership in a peace group. In general, we see little evidence of treatment effect heterogeneity. The most obvious exception is that the program appears to have increased the prevalence of unresolved land disputes among ethnic and religious minorities, both in absolute terms and relative to non-minorities.

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[7]Our qualitative work suggests that protests in rural Liberian communities often target unpopular decisions by local leaders (such as levying a local fee or tax or implementing a community labour requirement) or the actions of foreign companies extracting natural resources, or are related to friction between ethno-religious groups or frustration with government's lack of progress in resolving criminal cases.

[8]The census suggests these 86 communities have roughly 240,000 residents in total, and the survey respondents report an average household size of 8.

[9]This includes all adminstrative costs, all expenses lost to the delays, and many of the start-up costs associated with scaling this program for the first time. Scaling further could create economies of scale. Hence the $1.2 million cost is conservative as a cost denominator.

Table H.1: Effects of intense treatment

	1-year endline			3-year endline		
Dependent variable	Normal treatment (1)	Intensive treatment (2)	Sum (3)	Normal treatment (4)	Intensive treatment (5)	Sum (6)
Attended program if treated	0.251	0.086	0.336			
	[0.023]***	[0.045]*	[0.040]***			
Any serious land dispute	0.010	-0.030	-0.020	0.007	-0.013	-0.006
	[0.018]	[0.030]	[0.028]	[0.012]	[0.016]	[0.014]
Any threats, property damage, or violence	-0.008	-0.008	-0.016	-0.013	-0.006	-0.019
	[0.013]	[0.021]	[0.019]	[0.007]*	[0.009]	[0.007]**
Security rights index, z-score				-0.083	0.008	-0.075
				[0.044]*	[0.064]	[0.054]
Improvement index, z-score				-0.066	0.027	-0.039
				[0.039]*	[0.061]	[0.056]

Notes: Effects are with baseline covariates and district fixed effects. Communities randomly assigned to intense treatment received 30 to 40% more workshops. Estimates for the 3-year endline are weighted by the inverse probability of sampling. Standard errors are clustered by community.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table I.1: Heterogeneity by demographics and peace background on conflict

	For all residents		Conditional on a dispute ocurring	
Independent variable	Any land dispute (1)	Any unresolved dispute (2)	Any threats, property damage or violence (3)	Resolved land dispute (4)
Female				
Treatment	-0.002	0.005	-0.229	-0.062
	[0.015]	[0.008]	[0.066]***	[0.067]
Covariate	-0.018	-0.009	-0.045	0.035
	[0.012]	[0.008]	[0.071]	[0.071]
Treatment x Covariate	0.018	-0.008	0.079	0.081
	[0.018]	[0.010]	[0.104]	[0.102]
Sum of Treatment and Interaction	0.016	-0.002	-0.150	0.019
	[0.013]	[0.006]	[0.074]**	[0.070]
20-40 years old				
Treatment	0.014	0.005	-0.192	-0.133
	[0.014]	[0.006]	[0.071]***	[0.075]*
Covariate	0.007	0.005	0.066	-0.164
	[0.016]	[0.008]	[0.119]	[0.101]
Treatment x Covariate	-0.012	-0.006	-0.001	0.201
	[0.017]	[0.009]	[0.107]	[0.109]*
Sum of Treatment and Interaction	0.002	-0.001	-0.193	0.068
	[0.013]	[0.007]	[0.071]***	[0.069]
Below median wealth				
Treatment	0.005	-0.004	-0.203	-0.112
	[0.016]	[0.007]	[0.073]***	[0.068]
Covariate	0.025	-0.004	0.026	-0.134
	[0.025]	[0.010]	[0.084]	[0.093]
Treatment x Covariate	0.010	0.012	0.031	0.165
	[0.020]	[0.009]	[0.112]	[0.094]*
Sum of Treatment and Interaction	0.015	0.007	-0.172	0.052
	[0.013]	[0.006]	[0.076]**	[0.063]
Any ethnic minority				
Treatment	0.009	-0.001	-0.185	-0.008
	[0.011]	[0.005]	[0.051]***	[0.050]
Covariate	0.017	-0.007	0.000	0.000
	[0.024]	[0.015]	[0.000]	[0.000]
Treatment x Covariate	-0.013	0.025	-0.068	-0.153
	[0.026]	[0.013]*	[0.143]	[0.138]
Sum of Treatment and Interaction	-0.004	0.023	-0.253	-0.161
	[0.024]	[0.012]*	[0.130]*	[0.128]
% town peace education at baseline				
Treatment	0.030	0.006	-0.206	-0.049
	[0.017]*	[0.007]	[0.106]*	[0.097]
Covariate	0.023	0.024	-0.204	-0.009
	[0.038]	[0.017]	[0.260]	[0.194]
Treatment x Covariate	-0.079	-0.019	0.086	0.083
	[0.051]	[0.024]	[0.327]	[0.270]
Sum of Treatment and Interaction	-0.049	-0.013	-0.120	0.033
	[0.039]	[0.019]	[0.238]	[0.191]
% town peace group at baseline				
Treatment	0.014	0.009	-0.251	0.048
	[0.021]	[0.009]	[0.124]**	[0.109]
Covariate	-0.003	-0.014	-0.264	0.390
	[0.035]	[0.014]	[0.208]	[0.173]**
Treatment x Covariate	-0.016	-0.019	0.152	-0.185
	[0.048]	[0.021]	[0.273]	[0.248]
Sum of Treatment and Interaction	-0.002	-0.010	-0.099	-0.137
	[0.032]	[0.014]	[0.165]	[0.155]

Notes: 3-year endline heterogenous treatment effects varying by demographic characteristics, with baseline covariates and district fixed effects. Standard errors are clustered by community. Results can be compared to Table 3 (citation omitted), however the demographic characteristic *Muslim* has been dropped, as the variables *Muslim* and *Any ethnic minority* are collinear, with only 3 non-Muslim ethnic minorities.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table I.2: Heterogeneity by demographics and peace background on security and investment

Independent variable	Security rights index, z-score (1)	Property investment index, z-score (2)	Size of land (house and farm) (3)
Female			
Treatment	-0.082	-0.086	4.318
	[0.041]**	[0.038]**	[1.358]***
Covariate	-0.097	-0.187	-1.290
	[0.042]**	[0.035]***	[1.323]
Treatment x Covariate	0.008	0.067	-3.070
	[0.056]	[0.048]	[1.821]*
Sum of Treatment and Interaction	-0.074	-0.020	1.247
	[0.041]*	[0.033]	[1.287]
20-40 years old			
Treatment	-0.078	-0.063	3.282
	[0.041]*	[0.036]*	[1.341]**
Covariate	-0.031	0.072	3.184
	[0.056]	[0.044]	[1.726]*
Treatment x Covariate	-0.000	0.021	-1.014
	[0.056]	[0.048]	[1.823]
Sum of Treatment and Interaction	-0.078	-0.042	2.268
	[0.042]*	[0.035]	[1.307]*
Below median wealth			
Treatment	-0.068	-0.013	5.270
	[0.043]	[0.037]	[1.404]***
Covariate	-0.049	0.110	5.097
	[0.070]	[0.062]*	[1.947]***
Treatment x Covariate	-0.029	-0.066	-4.656
	[0.058]	[0.049]	[1.886]**
Sum of Treatment and Interaction	-0.097	-0.079	0.614
	[0.040]**	[0.035]**	[1.288]
Any ethnic minority			
Treatment	-0.083	-0.047	2.678
	[0.032]***	[0.028]*	[1.010]***
Covariate	0.129	0.000	1.544
	[0.073]*	[0.000]	[2.754]
Treatment x Covariate	0.037	-0.043	0.854
	[0.080]	[0.071]	[2.853]
Sum of Treatment and Interaction	-0.045	-0.090	3.532
	[0.075]	[0.069]	[2.714]
% town peace education at baseline			
Treatment	-0.196	-0.156	3.173
	[0.056]***	[0.045]***	[1.782]*
Covariate	-0.202	-0.203	3.056
	[0.122]*	[0.110]*	[4.231]
Treatment x Covariate	0.428	0.383	-2.008
	[0.167]**	[0.141]***	[5.535]
Sum of Treatment and Interaction	0.232	0.228	1.165
	[0.124]*	[0.108]**	[4.160]
% town in peace group at baseline			
Treatment	-0.108	-0.106	1.366
	[0.061]*	[0.049]**	[1.988]
Covariate	-0.127	-0.078	-0.940
	[0.110]	[0.085]	[3.518]
Treatment x Covariate	0.074	0.137	3.579
	[0.142]	[0.112]	[4.514]
Sum of Treatment and Interaction	-0.034	0.031	4.945
	[0.094]	[0.076]	[2.934]*

Notes: 3-year heterogenous treatment effects varying by demographic characteristics, with baseline covariates and district fixed effects. Standard errors are clustered by community. As in Table 2, all results are plot-level. *Muslim* has been dropped, as the variables *Muslim* and *Any ethnic minority* are collinear, with only 3 non-Muslim ethnic minorities.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$..

Table I.3: Summary statistics of conflict variables by demographic groups

	All 3-year endline residents		Gender		Age		Below median wealth		Any ethnic minority		Any peace education in town at baseline		Any peace group in town at baseline	
	Mean	SD	Men	Women	above 40	20-40	No	Yes	No	Yes	No	Yes	No	Yes
Pct. of 3-year endline residents			0.48	0.52	0.49	0.51	0.50	0.50	0.87	0.13	0.07	0.93	0.03	0.97
Panel A: Land dispute outomes for all residents														
Any serious land dispute	0.09	0.28	0.10	0.08	0.08	0.10	0.09	0.08	0.09	0.08	0.05	0.09	0.12	0.09
Any unresolved land dispute	0.02	0.15	0.03	0.02	0.02	0.03	0.03	0.02	0.02	0.02	0.00	0.02	0.04	0.02
Any threats, property damage, or violence	0.04	0.18	0.04	0.03	0.03	0.04	0.04	0.03	0.04	0.03	0.01	0.04	0.02	0.04
Property damage or violence in land dispute	0.02	0.13	0.02	0.01	0.01	0.02	0.02	0.02	0.02	0.02	0.01	0.02	0.01	0.02
Threats	0.03	0.17	0.03	0.03	0.03	0.03	0.04	0.03	0.03	0.02	0.01	0.03	0.02	0.03
Property damage	0.01	0.09	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.00	0.01	0.00	0.01
Violence	0.02	0.13	0.02	0.01	0.01	0.02	0.01	0.02	0.02	0.02	0.00	0.02	0.01	0.02
Panel B: Conditional on a land dispute														
Length of maximum land conflict	14.50	28.07	14.95	13.95	16.20	13.11	17.17	11.49	14.35	15.63	14.42	14.50	14.49	14.50
Resolved land dispute	0.66	0.47	0.65	0.67	0.69	0.64	0.61	0.72	0.66	0.68	0.87	0.65	0.67	0.66
Any threats, property damage, or violence	0.40	0.49	0.41	0.40	0.36	0.44	0.42	0.38	0.41	0.32	0.27	0.41	0.20	0.41
Property damage or violence	0.21	0.41	0.23	0.18	0.16	0.24	0.17	0.25	0.21	0.22	0.13	0.21	0.07	0.21
Threats	0.35	0.48	0.35	0.36	0.33	0.37	0.39	0.31	0.37	0.24	0.20	0.36	0.13	0.36
Property damage	0.09	0.29	0.10	0.08	0.07	0.11	0.09	0.10	0.09	0.10	0.07	0.09	0.00	0.10
Violence	0.18	0.39	0.20	0.16	0.14	0.22	0.15	0.22	0.18	0.20	0.07	0.19	0.07	0.19
Witchcraft	0.07	0.26	0.08	0.06	0.03	0.10	0.09	0.05	0.08	0.02	0.07	0.07	0.00	0.07
Panel C: General dispute outcomes for all residents														
Any serious dispute	0.31	0.46	0.31	0.30	0.25	0.36	0.34	0.27	0.32	0.24	0.25	0.31	0.35	0.30
Any unresolved dispute	0.06	0.25	0.07	0.06	0.06	0.07	0.08	0.05	0.06	0.06	0.03	0.07	0.12	0.06
Any threats, property damage, or violence	0.09	0.29	0.09	0.10	0.07	0.11	0.11	0.08	0.10	0.05	0.06	0.10	0.09	0.09
Panel D: Conditional on a dispute														
Resolved dispute	0.76	0.43	0.78	0.74	0.74	0.77	0.76	0.76	0.76	0.75	0.76	0.76	0.75	0.76
Resolved via informal mechanism	0.41	0.49	0.40	0.41	0.39	0.41	0.47	0.32	0.41	0.37	0.37	0.41	0.41	0.40
Any threats, property damage, or violence	0.31	0.46	0.29	0.33	0.30	0.31	0.33	0.29	0.32	0.21	0.26	0.31	0.25	0.31
Property damage or violence	0.18	0.39	0.16	0.20	0.16	0.20	0.18	0.19	0.19	0.12	0.14	0.19	0.11	0.19
Threats	0.25	0.44	0.24	0.27	0.25	0.25	0.28	0.22	0.26	0.17	0.21	0.26	0.23	0.25
Property damage	0.04	0.20	0.05	0.03	0.04	0.04	0.04	0.05	0.04	0.04	0.04	0.04	0.02	0.04
Violence	0.17	0.38	0.15	0.19	0.15	0.19	0.17	0.18	0.18	0.11	0.11	0.18	0.11	0.17

Table I.4: Summary statistics of security and improvement variables by demographic groups

	All 3-year endline residents		Gender		Age		Any ethnic minority		Politically connected		Has market tenure		Owns plot	
	Mean	SD	Men	Women	above 40	20-40	No	Yes	No	Yes	No	Yes	No	Yes
Pct. of 3-year endline residents			0.48	0.52	0.49	0.51	0.87	0.13	0.68	0.32	0.15	0.85	0.85	0.15
Property investment index, z-score	0.00	1.00	0.12	-0.12	-0.01	0.01	0.01	-0.04	-0.04	0.08	-0.13	0.02	0.00	-0.02
Monetary value of improvement, house and farm, (z-score)	-0.00	1.00	0.07	-0.06	-0.01	0.01	-0.00	0.02	-0.01	0.02	-0.10	0.02	0.01	-0.03
Non-monetary improvement, house and farm (z-score)	-0.00	1.00	0.07	-0.07	-0.00	0.00	-0.00	-0.04	-0.03	0.07	-0.09	0.02	-0.00	0.02
Made an improvement, house and farm	0.35	0.48	0.42	0.28	0.34	0.36	0.36	0.32	0.33	0.39	0.30	0.36	0.35	0.33
Security rights index, z-score	0.00	1.00	0.11	-0.11	0.05	-0.05	0.03	-0.23	-0.02	0.04	-0.49	0.09	-0.04	0.22
Feels secure in boundaries, house and farm	0.87	0.34	0.90	0.84	0.89	0.85	0.86	0.89	0.86	0.89	0.85	0.87	0.85	0.94
Has ability to inherit, house and farm	0.80	0.40	0.85	0.75	0.84	0.76	0.80	0.81	0.77	0.86	0.68	0.82	0.78	0.90
Has ability to sell, house and farm	0.26	0.44	0.28	0.24	0.26	0.25	0.27	0.15	0.27	0.23	0.17	0.27	0.25	0.31
Has ability to pawn, house and farm	0.26	0.44	0.28	0.23	0.26	0.25	0.27	0.16	0.26	0.24	0.17	0.27	0.25	0.30
has ability to survey, house and farm	0.61	0.49	0.67	0.56	0.63	0.59	0.62	0.54	0.59	0.65	0.46	0.64	0.59	0.70

J Additional family groupings for conflict regressions

Table J.1 shows the results of multiple hypothesis corrections similar to those in Tables 1 and D.3. However rather than correct for multiple hypotheses within land conflict outcomes and general conflict outcomes separately, we group outcomes across conflict types. Because outcome families for grouping are larger in size, the corrections in TableJ.1 are more conservative. Despite this more conservative approach, adjusted significance is broadly similar to results reported in the paper.

K Minimum detectable effect analysis

Table K.2 reports minimum detectable effect calculations. Our estimation of the minimum difference that we are powered to detect assumes a significance value of $\alpha = .05$ and 80% power. The calculation takes into account our clustered design via the number of clusters (communities), the number of observations within each cluster, as well as the intra-cluster variance and intra-cluster correlation of the outcome variables. Results show that we are powered enough to be confident in our most significant results.

L Component variables for norms, attitudes and skills indices

We used a battery of 49 survey questions to measure attitudes, skills, and norms. We organize these questions into six thematic areas: (1) Bias, (2) Defection, (3) Forum Choice, (4) Managing Emotions and Avoiding Violence, (5) Mediation, and (6) Negotiation. These six categories reflect core areas of the ADR curriculum used during the intervention. We formulated questions on attitudes to identify the respondent's personal beliefs about a particular behavior (for example: *Do you think that it is good idea to ask friends for help when resolving a dispute?*). For questions that focused on skills, we asked whether respondents actually practiced a specific behavior themselves (for example: *Do you help to cool other people's temper during a dispute?*). Finally, for questions that covered descriptive social norms, we asked questions about community behavior, not specifically about the respondent (for example: *In this community do people take sides during disputes because one of the parties is their friend?*). For these latter questions, the community was the reference group.[10]

We create an index for each thematic area, adding and standardizing questions so the index has zero mean and unit standard deviation. The indices are constructed such that more positive values indicate greater alignment with the messages of the training. Table L.1 displays all the questions in each index.

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[10]These questions were prefaced by the following preamble: "I'm going to ask you some questions about different different things when people talk palava *in this community*. For each thing, I will ask you first what you yourself think—whether you agree or you disagree? Then I will ask you how many people *in this community* can do that thing."

Table J.1: Program impacts on number, length, severity, and resolution of disputes with cross-conflict family groupings for adjusted p-values

Dependent Variable	Control mean (1)	N (2)	ITT (3)	ITT / control mean (%) (4)	Est. p-val (5)	WY Adj. p-val (6)	Holms Adj p-val (7)	Control mean (8)	N (9)	ITT (10)	ITT / control mean (%) (11)	Est. p-val (12)	WY Adj. p-val (13)	Holms Adj p-val (14)
				Endline 1							Endline 2			
Panel A: Land dispute outomes for all residents														
Any serious land dispute	0.221	5,435	0.003 [0.016]	1.2	0.867	0.879[a]	0.868	0.087	4,011	0.008 [0.011]	8.8	0.473	0.976[b]	0.986
Any unresolved land dispute	0.070	5,435	-0.020 [0.008]**	-28.0	0.015	0.172[a]	0.111	0.024	4,011	0.002 [0.005]	6.4	0.744	0.976[b]	0.986
Any threats, property damage, or violence	0.122	5,435	-0.010 [0.012]	-8.1	0.397	0.846[a]	0.868	0.041	4,011	-0.012 [0.006]**	-29.3	0.039	0.444[b]	0.329
Property damage or violence in land dispute	0.091	5,435	-0.012 [0.009]	-13.2	0.183			0.021	4,011	-0.007 [0.004]	-31.2	0.117		
Threats	0.114	5,435	-0.006 [0.011]	-5.3	0.596			0.035	4,011	-0.010 [0.006]*	-28.9	0.069		
Property damage	0.041	5,435	-0.013 [0.006]**	-32.4	0.029			0.010	4,011	-0.005 [0.003]*	-52.3	0.072		
Violence	0.077	5,435	-0.007 [0.008]	-8.7	0.416			0.017	4,011	-0.004 [0.004]	-21.4	0.361		
Panel B: Conditional on a land dispute														
Length of maximum land conflict								13.247	353	3.642 [2.889]	27.5	0.209	0.854[b]	0.846
Resolved land dispute	0.684	1,212	0.072 [0.027]***	10.5	0.009	0.131[a]	0.080	0.668	353	-0.024 [0.046]	-3.6	0.604	0.976[b]	0.986
Any threats, property damage, or violence	0.554	1,212	-0.024 [0.035]	-4.3	0.496	0.879[a]	0.868	0.476	353	-0.192 [0.047]***	-40.4	0.000	0.008[b]	0.001
Property damage or violence	0.411	1,212	-0.037 [0.030]	-9.1	0.213			0.243	353	-0.090 [0.042]**	-37.2	0.035		
Threats	0.515	1,212	-0.013 [0.035]	-2.5	0.713			0.408	353	-0.157 [0.048]***	-38.6	0.001		
Property damage	0.186	1,212	-0.051 [0.025]**	-27.4	0.041			0.114	353	-0.067 [0.027]**	-58.5	0.016		
Violence	0.349	1,212	-0.022 [0.028]	-6.3	0.442			0.202	353	-0.056 [0.042]	-28.0	0.186		
Witchcraft								0.065	353	0.035 [0.026]	54.3	0.182		
Panel C: General dispute outcomes for all residents														
Any serious dispute	0.299	5,435	0.022 [0.018]	7.2	0.220	0.705[a]	0.775	0.306	4,011	0.012 [0.017]	4.1	0.456	0.976[b]	0.986
Any unresolved dispute	0.118	5,435	-0.013 [0.011]	-11.0	0.250	0.731[a]	0.775	0.064	4,011	-0.004 [0.009]	-6.6	0.623	0.976[b]	0.986
Any threats, property damage, or violence								0.101	4,011	-0.015 [0.010]	-15.2	0.111	0.704[b]	0.650
Panel D: Conditional on a dispute														
Resolved dispute	0.676	1,670	0.051 [0.024]**	7.5	0.033	0.266[a]	0.208	0.767	1,227	-0.019 [0.026]	-2.5	0.467	0.976[b]	0.986
Resolved via informal mechanism	0.247	1,671	0.018 [0.025]	7.4	0.470	0.879[a]	0.868	0.409	1,227	-0.019 [0.027]	-4.6	0.487	0.976[b]	0.986
Any threats, property damage, or violence								0.331	1,227	-0.069 [0.026]***	-20.7	0.009	0.197[b]	0.093
Property damage or violence								0.190	1,227	-0.027 [0.022]	-14.1	0.222		
Threats								0.274	1,227	-0.072 [0.024]***	-26.2	0.003		
Property damage								0.048	1,227	-0.019 [0.011]*	-40.0	0.085		
Violence								0.173	1,227	-0.018 [0.021]	-10.2	0.411		

Notes: Intent-to-treat (ITT) effects with baseline covariates and district fixed effects.. Estimates are weighted by the inverse probability of sampling. Standard errors are clustered by community. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Columns (6) and (13) show Westfall-Young multiple hypothesis corrected p-values for the 1 and 3-year endlines, respectively, with superscript letters a and b representing the separate family groupings for multiple hypothesis adjustment.

Table K.2: Minimum detectable effects of main conflict variables

Dependent Variable	Treatment effect at 3-year endline (1)	Minimum detect-able effect (2)	Treatment effect as % of MDE (3)
Panel A: Land dispute outcomes for all residents			
Any serious land dispute	0.008	0.008	0.958
Any unresolved land dispute	0.002	0.003	0.581
Any threats, property damage, or violence	-0.012***	0.004	3.062
Panel B: General dispute outcomes for all residents			
Any serious dispute	0.012	0.015	0.799
Any unresolved dispute	-0.004	0.006	0.663
Any threats, property damage, or violence	-0.015	0.006	2.395

Notes: Column (1) reports treatment effects in Tables 1and D.3. Column (2) represents the minimum detectable effect in absolute terms computed using the `clusterPower` package in R. Column (3) reports the size of our treatment effect as a proportion of the minimum detectable effect.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table L.1: Questions on Attitudes, Norms and Skills

Bias index
When you have a dispute with someone, is it okay to gossip about that person?
In the community, to what degree do people in disputes gossip about one another?
When you have dispute with someone, it's okay to tell small lies?
In this community, how many people can tell small lies in when they in a dispute?
When two persons have a dispute, is it okay to take one side because that person is your good friend?
In this community, how many people can take one side when two persons have a dispute, just because that person is their good friend?

Defection index
After making an agreement to solve a dispute, is it okay not to go by the agreement?
In this community, after making an agreement to solve a dispute, how many people will not go by the agreement?
Is it okay to carry your dispute to many elders and chiefs at the same time to find someone that can agree with you?
To what degree do people carry their dispute to many elders and chiefs at the same time to find someone that can agree with them?
Is it okay to take your dispute to the police or court when you not satisfied with what the elder or chief say?
To what degree do people take their disputes to the police or court when they not satisfied with what the elder or chief say?
Is it okay to carry your dispute to some authorities where you know you can get advantage?
To what degree do people carry their disputes to authorities where they know they can get advantage?

Empathy and reflection
Since 2012, how many times did you listen to the other person without talking back even though the other person was wrong?
Since 2012 how many times did you convince yourself to talk to the person you were in a dispute with even if you thought the other person was wrong?
Since 2012 how many times did you think about how the other person in a dispute was really feeling?
Since 2012 how many times did you think about whether you wronged the other person?
Since 2012 how many times did you think about what you would do if you were in the shoes of the person you were in the palava with?

Forum choice index
Is it good to talk to the other person you have the dispute with first to solve your dispute before involving any other people?
To what degree do people talk to the person they have the dispute with FIRST to solve the dispute before involving any other people?
Is it good to go to people in the community who are good at resolving disputes FIRST before going to any other places?
To what degree do disputants go to people in the community who are good at resolving disputes FIRST before going to any other places?
Is it good to go to the police or court FIRST before going to any other places?
To what degree do people go to the police or court FIRST before going to any other places?

Avoiding violence and managing emotions index
Is it okay to spoil a person's property in a dispute because that person spoiled your property?
To what degree does one person in a dispute spoil another person's property in a dispute because that person spoiled their property?
To what extent do you talk to the person in a dispute without getting angry, even if the other person is getting angry with you?
When the other person in a dispute is getting angry, how many times do you talk to the other person to cool the person's temper?
Do you avoid saying bad things about the other person during a dispute?

Mediation
When you have a dispute with someone, it's okay to ask your friends or neighbors to help you advise that person?
To what degree do people ask their friends or neighbors to help them advise the other person that they have a dispute with?
When other people have a dispute, is it good to bring them together to solve their problem?
To what degree do people in the community bring persons who have disputes together to solve their problems?
Is it okay to help two neighbors in a dispute find a solution, even if one is wrong?
Since 2012 how many times did people in the community help neighbors who are in a dispute find a solution even if one is wrong?
Since 2012 how many times did you help bring people in a dispute together?
Since 2012 how many times did you help people in a dispute reach an agreement so that they both are satisfied?
Since 2012 how many times did you help people in a dispute understand each other?
Since 2012 how many times did you listen to people in a dispute so that they can cool their tempers?

Negotiation
When you have dispute with someone is it ok to find a solution where you are both satisfied even if you are right and they are wrong?
To what degree to people in a dispute with one another find a solution where they are both satisfied with even if one is right and the other is wrong?
Is it ok when you have a dispute with someone to give them something to make them feel better and accept the result?
To what degree do people who are in a dispute with another person give that other person something to make themselves feel better to accept the result?
For a dispute you had in 2012, how many times did you use talking and discussion to resolve the dispute?
Since 2012 how many times did you talk to make the other disputant understand what you wanted from the solution?
Since 2012 how many times did you propose a solution to a dispute that the other person agreed with?
Since 2012 how many times did you make a compromise with the other party to a dispute so that you could settle the matter?
Since 2012 how many times did you reduce a punishment or fine after an agreement so that you could settle the matter peacefully?