

# Engineering Informal Institutions: Long-Run Impacts of Alternative Dispute Resolution on Violence and Property Rights in Liberia

**Alexandra C. Hartman**, University College London

**Robert A. Blair**, Brown University

**Christopher Blattman**, University of Chicago

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 United Nations and civil society mass education campaign to promote alternative dispute resolution 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 toward nonviolent norms. Treated residents also report larger farms, although 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.

In settings where formal justice systems are absent or overloaded, informal institutions resolve disputes and protect property rights.<sup>1</sup> Policy makers frequently try to improve informal institutions, but can more just, equal, or peaceful systems be engineered? This article 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 (Lieberman and Henry 1986; Mnookin 1998).

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 (Blattman, Hartman, and Blair 2014). This article 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

Alexandra C. Hartman (alexandra.hartman@ucl.ac.uk) is an associate professor of political science and public policy at University College London, London WC1H 9QU. Robert A. Blair (robert\_blair@brown.edu) is the Joukowsky Family Assistant Professor of Political Science and International and Public Affairs at Brown University, Providence, RI 02912. Christopher Blattman (blattman@uchicago.edu) is the Ramalee E. Pearson Professor of Global Conflict Studies at the University of Chicago, Chicago, IL 60637.

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1. 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).

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 it is especially remarkable given that the intervention lasted only two months in each village and that we measured outcomes three years after implementation.

### **THEORETICAL FRAMEWORK**

In rural areas of many less developed countries, formal systems for resolving disputes are ineffective or nonexistent. 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.

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, 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, Jamison, and Sheridan 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 (e.g., changes in norms) 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 terms.

### **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, Pham, and Kreutzer 2011).

Liberians seldom use the formal justice system to resolve these disputes. Almost three-quarters of respondents 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, Blattman, and Hartman 2017). The government of Liberia and UN peacekeepers have long worried that local disputes could spark a national crisis.

### **Intervention**

In response to these challenges, in 2008 the United Nations High Commission for Refugees (UNHCR) and a nongovernmental organization, 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, Lubkemann, and N'Tow 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 with friends and family lessons learned from the workshops and apply them outside of the training. Facilitators also lived and slept in communities on weekdays and used their interactions with both participants and nonparticipants after hours to demonstrate and reinforce messages from the training and 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.

### Sample selection

UNHCR and JPC worked in three 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 5,000 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.

### 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 or-

dered phases over 21 months, from March 2009 to November 2010.<sup>2</sup> 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 one-year and three-year endlines, respectively. In each round we surveyed roughly 20 residents per community, sampled using the “random route” method (see app. B; apps. A–L available online). We also surveyed four local leaders—typically a town chief and a female, youth, and minority leader.<sup>3</sup> As we show in appendix A, random assignment produced the expected degree of balance along covariates.

We estimate intent-to-treat (ITT) effects via the following 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;  $\alpha_d$  are district fixed effects; and  $\epsilon_{ij}$  is an individual error term, clustered by community.<sup>4</sup>

### 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

2. 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%–40% more workshops.

3. For the three-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 shows that dropping communities does not substantively change our treatment effect estimates.

4. This specification differs from our previously published results in that we estimate an ITT effect rather than a complier average causal effect. Our results are substantively unchanged regardless. In the one-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.

Table 1. Effects on Incidence, Length, Severity, and Resolution of Land Disputes: One- and Three-Year Endlines

Dependent Variable	One-Year Endline						Three-Year Endline							
	Control Mean	N	ITT	ITT / Control Mean (%)	Estimated p-Value	WY Adjusted p-Value <sup>a</sup>	Sidak-Holm Adjusted p-Value	Control Mean	N	ITT	ITT / Control Mean (%)	Estimated p-Value	WY Adjusted p-Value <sup>b</sup>	Sidak-Holm Adjusted p-Value
Outcomes for all residents:														
Any serious land dispute	.221	5,435	.003 (.016)	1.2	.867	.887	.867	.087	4,011	.008 (.011)	8.8	.473	.872	.854
Any un-resolved land dispute	.070	5,435	-.020** (.008)	-28.0	.015	.080	.057	.024	4,011	.002 (.005)	6.4	.744	.878	.854
Any threats, property damage, or violence	.122	5,435	-.010 (.012)	-8.1	.397	.725	.781	.041	4,011	-.012** (.006)	-29.3	.039	.266	.182
Property damage or violence in land dispute	.091	5,435	-.012 (.009)	-13.2	.183			.021	4,011	-.007 (.004)	-31.2	.117		
Threats	.114	5,435	-.006 (.011)	-5.3	.596			.035	4,011	-.010* (.006)	-28.9	.069		
Property damage	.041	5,435	-.013** (.006)	-32.4	.029			.010	4,011	-.005* (.003)		.072		
Violence	.077	5,435	-.007 (.008)	-8.7	.416			.017	4,011	-.004 (.004)	-21.4	.361		

Conditional on a land dispute: Length of maximum land conflict															
Resolved land dispute	.684	1,212	.072*** (.027)	10.5	.009	.075	.045	.668	13.247	353	3.642 (2.889)	27.5	.209	.680	.609
Any threats, property damage, or violence	.554	1,212	-.024 (.035)	-4.3	.496	.773	.781	.476		353	-.192*** (.047)	-40.4	.000	.008	.000
Property damage or violence	.411	1,212	-.037 (.030)	-9.1	.213			.243		353	-.090** (.042)	-37.2	.035		
Threats	.515	1,212	-.013 (.035)	-2.5	.713			.408		353	-.157*** (.048)	-38.6	.001		
Property damage	.186	1,212	-.051** (.025)	-27.4	.041			.114		353	-.067** (.027)	-58.5	.016		
Violence	.349	1,212	-.022 (.028)	-6.3	.442			.202		353	-.056 (.042)	-28.0	.186		
Witchcraft								.065		353	.035 (.026)	54.3	.182		

Note. Intent-to-treat (ITT) effects with baseline covariates and district fixed effects. Estimates are weighted by the inverse probability of sampling. Standard errors (in parentheses) are clustered by community.

<sup>a</sup> Westfall-Young multiple hypothesis corrected *p*-values for the one-year endline.

<sup>b</sup> Westfall-Young multiple hypothesis corrected *p*-values for the three-year endline.

\* *p* < .1.

\*\* *p* < .05.

\*\*\* *p* < .01.

alone. Moreover, given the length of time between the intervention and the three-year endline, it seems very unlikely that social desirability bias persisted without any change in actual underlying norms and skills.

### Preregistration and multiple comparisons

Both endlines were conducted before use of the social science registry became widespread, and our primary and secondary outcomes were not preregistered. We use our previously published one-year results to prespecify primary outcomes for the three-year analysis. The three-year survey also added questions on a number of secondary outcomes, especially security of property rights and skills and norms of dispute resolution (see table L.1). While these were not prespecified, the previous article 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, Young, and Wright (1993) free step-down resampling method for the family-wise error rate, which captures the probability that at least one of the true null hypotheses will be falsely rejected, using randomization inference, and the more mechanical Šidák-Holm method.

## RESULTS

### 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 whether 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 the control. These effects did not persist, however, and after three years we see no evidence of a continued increase in the rate of resolution or a decrease in the length of disputes. We previously speculated that the program was especially effective in resolving the most long-standing 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 long-standing 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 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 ( $p < .001$ ). In table D.3 we show that training had similar effects on threats and violence related to other types of disputes as well.

### Security of property rights

We measured four dimensions of property rights over particular plots of land in the three-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 on improvements. We use these measures to construct standardized indexes 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 2.77 acres larger than the control community average. On the other hand, farm and house plots in treatment communities scored .085 standard deviations lower on our index of security of property rights. Respondents in treatment communities also scored .066 standard deviations lower on our improvement index. The coefficient on our fallowing index is negative as well, although substantively small and not statistically significant. These results are inconsistent with expectations from our previous study, although 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 app. 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

Table 2. Effects on Investment and Security of Property Rights: Three-Year Endline

Dependent Variable	N	Control Mean	ITT	p-Value		
				Estimated	Westfall-Young Adjusted	Šidák-Holm Adjusted
Security rights index, z-score	4,011	.045	-.085** (.037)	.024	.120	.071
Improvement index, z-score	4,011	.023	-.066* (.037)	.079	.207	.151
Index of fallow land for farm	3,666	.003	-.004 (.044)	.926	.921	.926
Size of farm	3,598	37.481	2.773*** (1.051)	.009	.064	.036

Note. Three-year intent-to-treat (ITT) effects with baseline covariates and district fixed effects. Estimates are weighted by the inverse probability of sampling. Standard errors (in parentheses) are clustered by community. Unlike in tables 1 and D.3, results are at the plot level, where there are two types of plots: house or farm. Not all respondents tend to plots, which causes varying *N*s across variables.

\*  $p < .1$ .

\*\*  $p < .05$ .

\*\*\*  $p < .01$ .

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 nongovernmental organization programs that must work with local leaders for access). Our results lend some credence to these concerns.

### 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; and respondents' dispute resolution skills, such as whether they tried to resolve a dispute directly with another party in the last year.<sup>5</sup> When measuring social norms, we use the community as the reference group.

Table 3 reports effects on indexes 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, although 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 did not reduce the incidence, length, or resolution of disputes in the long term but did mitigate violence.

### 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 app. 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, although 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

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

Table 3. Effect on Norms, Attitudes, and Skills: Three-Year Endline

Dependent Variable	N	Control Mean	ITT	p-Value		
				Estimated	Westfall-Young Adjusted	Šidák-Holm Adjusted
Index of all norms combined	4,011	-.026	.028 (.038)	.469	.856	.895
Bias index	4,011	-.009	-.002 (.046)	.962	.997	.997
Defection index	4,011	-.043	.045 (.041)	.274	.796	.853
Empathy index	4,010	.002	.030 (.033)	.363	.856	.895
Forum choice index	4,011	-.028	.031 (.037)	.400	.856	.895
Managing emotions index	4,011	-.031	.067** (.031)	.032	.267	.228
Mediation index	4,011	.003	-.062* (.037)	.094	.489	.499
Negotiation index	4,011	.002	.002 (.027)	.945	.997	.997

Note. Three-year intent-to-treat (ITT) effects with baseline covariates and district fixed effects. Estimates for three-year endline residents are weighted by the inverse probability of sampling. Standard errors (in parentheses) are clustered by community.

\*  $p < .1$ .

\*\*  $p < .05$ .

\*\*\*  $p < .01$ .

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.

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