Interest-based Negotiation over Natural Resources: Experimental Evidence from Liberia*

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Abstract

We use a randomized control trial to study whether an interest-based negotiation (IBN) training for community leaders in Liberia improves their ability to strike beneficial deals related to their land and forests. We use environmental assessments, lab-in-the-field, and surveys and find that trainees are 27% more likely to reach a beneficial agreement, and when they conclude deals, their payoffs are 37% larger. Our mediation analysis and structural estimates both indicate that IBN increases trainees' capacity to identify positive-sum agreements. We also document a reduction (0.26 standard deviations) in the exploitation of communal forestland in trainees' communities.

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1. Introduction

Across the Global South, the demand for land and timber is increasing, and rural communities have new opportunities to negotiate with outside investors over natural resources (Davis, D'Odorico, and Rulli 2014). While initially hailed as opportunities for rural development, there is concern that these investments can detract from communities' well-being. A report from the World Bank warns that "instead of generating sustainable benefits, [many land investments] contributed to asset loss and left local people worse off than they would have been without the investment" (Deininger and Byerlee 2011, 71). Similar issues arise in negotiations between rural communities and small-scale logging operators (known locally as "pit-sawers"): in Liberia a majority of community members surveyed in USAID (2017) view pit-sawing unfavorably, with conflicts emerging around whether the royalties paid to communities offset the costs of deforestation.

Where communal land is implicated, community leaders help negotiate the terms of natural resource extraction with concessionaires and, more frequently, pit-sawers (Christensen, Hartman, and Samii 2021b). A prominent explanation for disadvantageous agreements is that these leaders cannot effectively negotiate.[1] The UN's Special Rapporteur on the Right to Food argues that "strengthening the negotiation capacity is vital. And that capacity cannot be of governments alone. Local communities must also be empowered" (Laishley 2009).

This paper experimentally evaluates whether interest-based negotiation (IBN) skills change the kinds of deals community leaders strike. Existing research focuses on why people fail to reach mutually beneficial agreements due to self-serving biases (e.g., exaggerated assessments of one's outside option) that impede negotiations (e.g., Babcock and Loewenstein 1997; Bazerman et al. 2000; Tsay and Bazerman 2009). Yet, the problem in Liberia and elsewhere is not bargaining impasse. Agribusiness and timber deals are getting done, but some of these agreements leave communities worse off. In a series of behavioral games, we find that community leaders in rural Liberia frequently agree to deals even when they would have been better off walking away: nearly half (47%) never reach a deal worth more than their outside option; over one-quarter (27%) reach agreements that, on average, pay them less than their outside option.[2] This behavior betrays two mistakes that are common among untrained negotiators (Fisher and Davis 1987). First, they think of negotiations as a zero-sum interaction, in which the goal is to maximize their position along a single dimension (often a sale or rental price). Second, they fixate on reaching the agreement that pays them the best price, overlooking that they may be better off walking away.

IBN is an approach taught to thousands in business, law, and policy schools around the world, which tries to correct common negotiation mistakes. IBN training stresses that parties should focus on their interests (and

1. Other explanations focus on agency problems: leaders conclude deals that generate large side payments but offer little to constituents (see Christensen, Hartman, and Samii 2021a, for a discussion of accountability issues). These agency problems notwithstanding, we show below that many leaders in rural Liberia lack the negotiation skills needed to reach agreements that benefit themselves or their constituents.

2. We find little evidence of self-serving biases leading to an impasse: our respondents virtually never refuse or let the clock run out on a deal that pays them more than the stated value of their outside option.

not specific demands), which can reveal opportunities to reach multi-dimensional agreements that benefit both parties. It also teaches individuals to prepare for any negotiation by carefully appraising their outside option, so that they do not agree to a deal that leaves them worse off than simply walking away.

We use a randomized controlled trial to evaluate whether a 12-hour training in IBN enables leaders from 120 communities in rural Liberia to more effectively negotiate over their land and forest resources. In surveys and lab-in-the-field simulations administered six months after the training, we find that trainees recall and deploy key concepts: our mean effects indexes related to knowledge and use of IBN skills increase by over 0.2 standard deviations. Trainees are 20% more likely to correctly define IBN and recognize that negotiations can result in win-win agreements.[3] In our lab-in-the-field measures — three incentivized negotiation simulations around potential land and logging deals — we find that trained leaders are 27% more likely to reach a beneficial agreement, and when they make a deal, they earn 37% more than leaders from control communities who did not participate in the IBN training.

We explore whether trainees' success is attributable to two mechanisms: IBN may increase trainees *capacity* to find valuable deals, or it may improve their ability to *appraise* their outside option, reducing the likelihood that they agree to a deal that is inferior to walking away. Both our mediation analysis and structural estimates indicate that the effects we uncover are primarily attributable to trainees' increased capacity to find more beneficial deals. For the mediation analysis, we construct indexes of intermediate outcomes related to capacity and appraisal and find that the indirect effect of capacity is more than five times that of appraisal. This aligns with our structural model, which imposes a decision-theoretic framework to derive estimates of capacity and appraisal from respondents' negotiation outcomes, without relying on intermediate survey outcomes. Our structural estimates imply that the training improved capacity but did not increase trainees' assessment of their outside options and actually may have amplified self-defeating biases in sub-groups with little or no education.

Finally, the improvements we uncover in our behavioral games carry over to real-world behaviors related to natural resource use. In treatment communities, we find increased engagement in forest management and reductions in external forest use (e.g., logging), with no decline in the benefits that flow from such investments. These findings are consistent with leaders in treatment communities demanding more of outside investors who want to exploit communal forestland for agriculture or logging, resulting in fewer higher-value deals.[4]

We contribute to the literature in three ways. First, we expand the small existing literature on the effects of IBN and show that an IBN is effective in improving the community leaders' ability to negotiate over natural resources. Ashraf et al. (2020) find that IBN training for 8th-grade girls in Zambia increases their

3. A shorter module in the training stressed maintaining a positive relationship with one's negotiating partner and provided strategies for diffusing conflict. We do not, however, find meaningful changes in trainees' interpersonal skills.

4. Our structural estimates imply that older, educated men experienced the largest improvements in their negotiation capacity, and such individuals also play more influential roles in governing communal forestland.

future school attendance by eight to ten percent.[5] Participating girls can better convey to their parents why continued education can be a "win-win" for the girls and their parents, who may depend on their daughters in old age. Blattman, Hartman, and Blair (2014) show that training in alternative dispute resolution, which incorporates some elements of IBN, reduces violent land disputes in Liberia (see also Hartman, Blair, and Blattman 2021).

Second, our analysis of mechanisms helps explain why the IBN training improves negotiation outcomes. While the existing literature has focused on self-serving biases (Babcock, Wang, and Loewenstein 1996), IBN presumes two other cognitive constraints that handicap untrained negotiators: (1) haggling over a single dimension, they do not consider all potential agreements; (2) fixated on reaching an agreement, they effectively discount the value of walking away. Our mediation and structural analysis both indicate that the IBN training improved outcomes by loosening the first constraint, increasing trainees' capacity to identify a positive-sum agreement. Our control-group data suggests the second constraint applies in our context — untrained negotiators frequently agree to deals worth less than their outside option — but we do not find that the IBN training is corrective. If anything, our structural analysis suggests that the training may have amplified this bias for some sub-groups: the bargaining behavior of trainees with limited or no education suggests that they were particularly keen to make deals relative to their untrained counterparts, implying that they discounted the returns to walking away. IBN training emphasizes that win-win agreements often exist; it needs to do a better job conveying that not all deals are worth making.

Third, our study contributes to a broader body of work that evaluates the returns to business training. Policymakers spend over one billion dollars annually training businesses in low- and middle-income countries, yet rigorous evaluations of trainings show mixed results (for a recent meta-analysis, see McKenzie et al. 2020). Some trainings emphasize relational skills, including "mindset" and personal initiative skills (e.g., Campos et al. 2017; Dammert and Nansamba 2019; Ubfal et al. 2020). Most of these studies find statistically insignificant effects on profit (McKenzie et al. 2020). Other interventions focus on "harder" business skills such as accounting, management, marketing (e.g., Dimitriadis and Koning 2020; Williams et al. 2020). Again, most studies cannot reject the null of no change in profits, though a meta-analysis of these studies reports 12% improvement. Focusing on one specific element of a business curriculum (negotiation), as opposed to a bundle of skills, we show that relatively low-cost training (less than $200 per trainee) transmits valuable knowledge and skills. We expect such training to be valuable in settings where, first, would-be trainees have neither been taught, nor otherwise learned, to avoid common negotiation mistakes and, second, where neither party can use (the threat of) coercion to insist upon a certain outcome.

5. Hardy, Kagy, and Song (2021) also study negotiation. However, their focus is not on negotiation skills, but rather on how the parties' endowments affect their ability to extract value in market transactions.

2. Intervention and Conceptual Framework

2.1 IBN Training

Many people — and economic models of bargaining — approach negotiation as an adversarial and often zero-sum exercise (Osborne and Rubinstein 1990). Parties focus on a single dimension (e.g., sale price) and attempt to reach an agreement, with each party trying to maximize their payoff. This type of negotiation is referred to as positional: parties stake out positions along whatever dimension is being bargained over. Some negotiations are invariably positional, such as haggling over food prices at a market. But many bargains could be multi-dimensional: when a concessionaire wants to lease land from a Liberian community, negotiations need not restrict attention to the annual lease payment but could cover investments in infrastructure and amenities, training and employment opportunities, or royalties.

In regarding all negotiations as positional, people tend to make two mistakes. First, they do not seize opportunities to negotiate over multiple dimensions and, in doing so, realize more beneficial agreements. Second, they fixate on reaching the agreement that maximizes their position, forgetting that they can, and sometimes should, just walk away (Fisher 1981). In our control group in the absence of training, over one-quarter of community leaders reach agreements in simulations that, on average, pay them less than the stated value of their outside option. The latter would have been better off had they never sat down to negotiate.

IBN training works to correct these mistakes. First, it challenges individuals to enumerate their interests and recognize that many different agreements can advance those interests. For example, in negotiating with a concessionaire, a community may want to increase wage labor. This might be achieved through employment on the concession, work for subcontractors building the infrastructure or amenities, or education and training programs that increase employment in other sectors. Demanding that the company provide a certain number of jobs — a common position — may not maximally advance the community's interest, especially if it is cheaper for the company to provide other types of employment opportunities that the community values. Second, IBN also asks individuals to appraise their best alternative to a negotiated agreement (BATNA) before entering into any negotiation. This reminds individuals that the payoff to walking away can be substantial, and they are better off refusing agreements that are inferior to this outside option. A concession agreement may, for example, promise some new jobs but still be inadvisable if it displaces agricultural activities that many more households depend on.

To understand how IBN training can improve negotiation outcomes, we study an intensive 12-hour IBN training provided by a Liberian NGO, Parley Liberia. The curriculum is adapted from courses taught in business, law, and policy schools.[6] The training consists of three modules: (1) preparing to negotiate, particularly identifying one's interests and BATNA; (2) identifying potential agreements and evaluating

6. The training draws on the widely taught *Getting to Yes* from Fisher (1981), but also integrates concepts from other texts that repeatedly appear on syllabi from major business, law, and policy schools in the US (see Christensen et al. 2021, for a full listing).

whether these advance one's interests relative to that BATNA; and (3) building and maintaining a positive relationship. Most of the training (roughly 80 percent) is devoted to teaching the first two modules. Staff from Parley Liberia tailored the content to the Liberian context, integrating familiar examples, adjusting terminology, and teaching the course in Liberian English. On average, sessions included twelve trainees per trainer.

2.2 Conceptual Framework

IBN can increase individuals' payoffs through two mechanisms: (1) it enlarges the set of deals they consider; and (2) it increases their disagreement payoff, reducing the likelihood that they agree to a deal that leaves them worse off. We develop (and later estimate) a decision-theoretic model that features both mechanisms. Let $\theta(D_i) \in \mathbb{R}_1^+$ represent the most attractive deal that an individual can negotiate, where $D_i = \mathbb{1}(\text{IBN})$ indicates whether i received the IBN training. Every individual also has an outside option that they value at $\beta + u_{Di}$, where $\beta > 0$ and $u_{Di} \sim F_D(\cdot)$. Our observation that, in the absence of the training, individuals accept deals worth less than their outside option suggests that for a substantial number of people, $u_0 << 0$ — their beliefs about their outside option are biased downward.

An individual will reach an agreement only if $\theta(D_i) \geq \beta + u_{Di}$ and will otherwise walk away. IBN could enhance individuals' capacity to reach better deals ($\theta(1) > \theta(0)$), which should increase rates of agreement and surplus. It could also improve individuals' ability to appraise their BATNA ($E[u_{1i}] > E[u_{0i}]$), raising their threshold for agreeing to a deal. The latter should reduce the rate of agreement and has ambiguous effects on the average surplus, as positive biases (i.e., over-estimation of the outside option) could preclude beneficial agreements.

3. Research Design

3.1 Context

Since 2002, deforestation has resulted in the loss of 14% of Liberia's total tree cover (Global Forest Watch 2022), with the largest threat coming from local chain-saw millers, who produce timber for primarily domestic consumption (USAID 2017). Our trainees are drawn from sixty rural communities in Bong County, Liberia. Nearly all of Bong County falls in Liberia's "hinterland" — a legal term for the interior of the country, located further than forty miles from the coast — where private land titles are relatively rare. Land here is typically governed by a customary property rights system, in which a community leaders grant access and allocate benefits that flow from investments on communal land (Christensen, Hartman, and Samii 2021b). In our study area, chain-saw millers and other investors negotiate with a community's chief and other leaders (e.g., elders) if they want to operate in the community's forestland.

3.2 Study Design

Our study design is summarized in Appendix Figure A.1.[7] We first identified 138 eligible communities in Bong County (see Appendix Section A).[8] We selected 120 of these communities for the study to minimize the potential for cross-community spillovers (Christensen et al. 2021). Using a two-by-two factorial design, we randomly assign 60 of these communities to the IBN training; another 60 were assigned to a citizen monitoring program that is separately analyzed in Christensen, Hartman, and Samii (2021a). We used ancillary data (e.g., climate, road access, forest loss) to ensure that candidate randomizations satisfied a balance criterion (Bruhn and McKenzie 2009). Appendix Table A.5 shows balance from our final treatment assignment. We provide additional details about the blocking and randomization procedure in Appendix Section C.3.

The IBN training was held from May to June 2018. In every community, we identified six community leaders; in treatment communities, these leaders received the IBN training. Leaders had to hold one of the following positions within their community: town chief, women's leader, midwife, youth leader, chief elder, landlord, hunter leader, or teacher.[9] This ensures, firstly, that respondents in treatment and control hold similar positions; randomly sampled controls would not provide a compelling counterfactual for village leadership. Second, individuals in these roles are more likely to be involved in decisions about how to manage their community's natural resources (Appendix Table A.4 summarizes demographics).

3.3 Measurement

We started endline data collection six months after the IBN training to ensure that knowledge gains were not short-lived. We surveyed 713 community leaders, who each completed three negotiation simulations; surveyed five randomly selected households; and conducted 118 independent environmental assessments of communal forestland to measure forest use. The environmental assessment is based on objective assessments by trained experts who do not have a connection to the community.[10] Appendix Section B describes these instruments.

To capture how individuals negotiate, we use a lab-in-the-field approach, with respondents participating in three incentivized simulations.[11] We wrote these simulations to resemble real-world interactions in our study area and be distinct from any training exercises. In each simulation, the respondent controls a natural

7. We pre-registered this study: AEA registry (AEARCTR-0007986) and EGAP (20171221AA). All pre-specified analysis can be found in Appendix Section D.2 with deviations listed in Appendix Section G.1.

8. To be eligible, a community needed to have communal forestland, and its leadership needed to express interest in participating. All respondents separately gave informed consent.

9. Appendix Table A.2 shows that the same share held these positions in our treatment and control communities.

10. We did not conduct a baseline survey due to funding constraints. Two control communities did not consent to the environmental assessment, because they did not approve of outsiders entering sacred communal land.

11. Respondents received prizes of soap and cooking spices for concluding deals that paid them more than the disagreement payoffs specified in the scripts.

resource endowment (e.g., farmland), and they are approached by a buyer interested in exploiting that endowment. Respondents were read a script describing their endowment and its current yield (e.g., the dollar value of their annual harvest), completed a comprehension check, and were then given ten minutes to negotiate with the buyer. We reminded respondents before every simulation that they can "walk away at any time" and end the simulation. Such simulations are commonly used to assess students' ability to negotiate (for additional details, see Appendix Section B.1).

This measurement strategy has several advantages. First, the simulation scripts provide a dollar-valued assessment of how much the endowment yields, anchoring the respondents' BATNA. Second, we set the rules of the negotiation, particularly who respondents negotiate with and how that counterpart behaves. We trained enumerators to serve as the buyer and fully specified their strategy, i.e., what to do in response to the respondents' behavior (see Christensen et al. 2021, for the full scripts). Variation in respondents' outcomes is not a consequence of negotiating with buyers who vary in their interests, resources, or sophistication; we also include enumerator fixed effects in our analysis. Moreover, our buyers' strategies did not depend on respondents' assertiveness or confidence, which allows us to rule out self-presentation as a mechanism. Third, negotiations over natural resources are common in our study area — individuals report pit-sawing activity in 98% of the villages in our sample — but they are not happening every week or month (as lease or timber agreements typically cover longer stretches of time) and may involve multiple community members. The simulations provide statistical power through multiple, individualized observations.

Finally, the simulations are designed to capture whether participants can reach positive-sum agreements and avoid outcomes that pay them less than the current return on their endowment. For example, in one of the simulations, the respondent is approached about leasing their farmland to construct a cellphone tower. A respondent with high negotiation capacity should first uncover that the buyer only needs part of the seller's land and, second, that the cellphone tower can be built in a rocky lot that does not otherwise produce crops. Thus, an agreement exists that allows the seller to collect the lease payment and maintain their agricultural production while permitting the buyer to proceed with construction — a clear win for both parties. On the flip side, this simulation also permits agreements in which the seller leases all or most of their land for a rate well below the value of their annual harvest. The surplus achieved by the buyer measures their capacity to incorporate dimensions beyond price — in this example, how much and what quality of land to lease, rather than just the rental amount — and, in doing so, envision a larger set of possible agreements. It also captures their ability to appraise possible agreements and walk away from those paying them less than their BATNA.

We group variables related to the same hypothesis and construct mean-effects indexes a la Kling, Liebman, and Katz (2007) (see Appendix Section B.4). Effects on these indexes are in terms of control-group standard deviations.

4. Average Treatment Effects

4.1 Estimation

We use a centered-interaction specification from Lin (2013) to estimate the average treatment effect (ATE).[12] Equation (1) includes covariates for the cross-randomized treatment; block fixed effects; simulation fixed effects (when appropriate); enumerator fixed effects; and a set of respondent characteristics (age, gender, education, leadership position, simulation order). We cluster our standard errors on community, which is the unit of randomization.

$$\begin{aligned}
Y_{sibc} = \alpha &+ \beta \mathbb{1}(\text{IBN})_{bc} && (\beta = \text{ATE}) \\
&+ \phi_1 \tilde{\mathbb{1}}(\text{CM})_{bc} + \phi_2 \mathbb{1}(\text{IBN})_{bc} \times \tilde{\mathbb{1}}(\text{CM})_{bc} && \text{(Other Treatment)} \\
&+ \sum_{b=1}^{B-1} [\phi_{3b} \tilde{\mathbb{1}}_b + \phi_{4b} \mathbb{1}(\text{IBN})_{bc} \times \tilde{\mathbb{1}}_b] && \text{(Block FEs)} \\
&+ \sum_{s=1}^{2} [\phi_{5s} \tilde{\mathbb{1}}_s + \phi_{6s} \mathbb{1}(\text{IBN})_{bc} \times \tilde{\mathbb{1}}_s] && \text{(Simulation FEs)} \\
&+ \sum_{k}^{K} [\phi_{7k} \tilde{X}_{k,ibc} + \phi_{8k} \mathbb{1}(\text{IBN}) \times \tilde{X}_{k,ibc}] + \varepsilon_{sibc} && \text{(Covariates)}
\end{aligned} \tag{1}$$

We also hypothesized that agreeing to a deal generates a larger surplus for trainees than for non-trainees. This implies that the training moderates the effect of reaching an agreement on the respondents' payoff. To assess this, we include in Equation (1) an indicator for whether an agreement was reached, and the interaction between that indicator and the treatment (see Appendix Section C.6).

4.2 Results

We had very high compliance: over 90% of the invited trainees recall attending the IBN training, including its location and duration (see Appendix Table A.7). We report intent-to-treat estimates; these are only slightly attenuated relative to treatment-on-the-treated estimates. Appendix Table A.6 provides control-group levels for all pre-specified outcomes.

Knowledge and skill deployment. We start by assessing whether individuals can recall information taught in the training six months later. We find that trainees are 20% more likely to correctly define IBN and recognize that negotiations can be positive-sum, i.e., that win-win agreements may exist.[13] Aggregating these knowledge questions into a mean-effects index, we find an increase of 0.34 standard deviations (see Table 1).

12. The ˜ operator in Equation (1) indicates that a variable has been centered.
13. Appendix Table A.7 includes treatment effects on the sub-components of all mean-effects indexes.

Table 1: Average Treatment Effects of IBN on Simulation and Survey Outcomes

Panel A: Effect of IBN				
	ATE	Std. Error	*p*-value	N
H1: Knowledge of Negotiation Skills†	0.335	(0.068)	0.00	705
H2: Knowledge of Inter-personal Skills†	-0.082	(0.076)	0.28	705
H3: Deployment of IBN Skills†	0.214	(0.084)	0.01	705
H4: Deployment of Inter-personal Skills	0.025	(0.014)	0.06	2115
H5: Positive Surplus	0.060	(0.023)	0.01	2115
H6: Total Surplus	2.742	(1.472)	0.07	2115
Panel B: Effect of Agreement on Surplus				
	QOI	Std. Error	*p*-value	N
H7: Differential Effect of Agreement on Surplus for Trainees	4.845	(2.41)	0.05	2115

Table 1: Average treatment effect estimates on negotiation outcomes using Equation (1). Standard errors in parentheses are clustered at the community level. † stands for mean-effects index. QOI stands for quantity of interest.

We show that trainees better apply this knowledge while negotiating: our mean effects index "Deployment of IBN skills" increases by 0.21 standard deviations. Trainees are more likely to invoke concepts, such as referring to their "bottom line" (i.e., BATNA), and also 44% more likely to discover a win-win deal during one of the simulations.

While it was a shorter module, the training also conveyed the value of maintaining a positive relationship while negotiating and discussed strategies for diffusing conflict. We do not, however, find a substantively large effect on whether respondents display anger or frustration during the simulations (as recorded by enumerators), just 0.03 standard deviations. The control-group levels for this variable were quite high (93% did not display anger), leaving little room for improvement.

Negotiation success. We next look at (H5) whether the training affected the likelihood that an individual achieved a "positive surplus" in a simulation, defined as reaching an agreement that exceeds the disagreement payoff noted in the simulation script. We also analyze a continuous measure of the total surplus achieved during a simulation. If someone walks away from a simulation, we code their total surplus in that simulation as zero. If someone reaches an agreement, then we subtract the value of that agreement from the disagreement payoff in the simulation script. The total surplus can be negative if an individual agrees to a deal that is less than the disagreement payoff: for example, an individual leases their land for $50 when they could have made $100 selling crops grown on the same land.

Among our non-trainees, we find that 47% do not earn a positive surplus in any of the three simulations they play. Averaging across the three simulations, 27% of non-trainees have a negative average surplus. These levels indicate the frequency of negotiation mistakes absent IBN training: over a quarter would have been better off if they had immediately walked away form every negotiation. We find that the IBN training increases the probability of earning a positive surplus by 27%, and it raises the total surplus earned by $2.74 USD, which is a 42% increase.

The higher average surplus among trainees reflects effects on the extensive margin (i.e., whether to make a deal) and the intensive margin (i.e., the value of agreements). To better isolate the effect on the intensive margin, we estimate the effect of reaching an agreement on the total surplus conditional on having received training. In the simulations that end in deals, we find that trainees earn 37% more (H7 in Table 1).

5. Mechanisms

Our conceptual framework features two mechanisms: training in IBN could (1) increase trainees' *capacity* to reach valuable deals and (2) improve their ability to *appraise* their outside option, reducing the likelihood that they conclude an agreement worth less than their BATNA. Our mediation analysis assesses whether improvements in skills related to these two mechanisms affect outcomes. Our structural estimation allows us to estimate a latent feature of the negotiation process — namely, respondents' beliefs about the value of their outside option — and evaluate the effects on such beliefs.

5.1 Mediation Analysis

Our pre-specified hypotheses reflect a simple causal logic: trainees acquire skills, deploy these, and more effectively negotiate. To study whether the training affected capacity or appraisal skills, we re-group our survey measures, creating two new indexes. For example, whether an individual recognizes the potential for a win-win deal enters our capacity index; whether an individual refers to their outside option contributes to our appraisal index. While motivated by theory, these indexes were not pre-specified. As a check, we take all of the measures in our capacity and appraisal indexes and estimate the first two principal components. We find that the first principal component is correlated with our capacity index ($\rho = 0.67$). The second component is instead correlated with our appraisal index ($\rho = 0.99$).[14]

We start by visualizing the relationship between our capacity and appraisal indexes and negotiation outcomes. In Figure 1, we residualize the indexes and our surplus measure using the pre-specified covariates and then compute averages for each community. In the left and right panels, we see that communities whose leaders attended the training fall to the right of leaders from control communities, suggesting a "first-stage" effect of IBN on individuals' capacity and appraisal skills. The "reduced-form" effect of IBN on the total surplus from Table 1 is reflected in IBN communities tending to fall north of control communities. The dashed line in the figure is the bi-variate relationship between the mediator and the outcome. On the left, we see that communities where trainees have more capacity skills also achieved larger surpluses; on the right, the association between appraisal and surplus is muted.

14. Appendix Table A.15 shows our mediation analysis using these principal components.

Figure 1: Relationship between Mediators and Surplus at Community-level

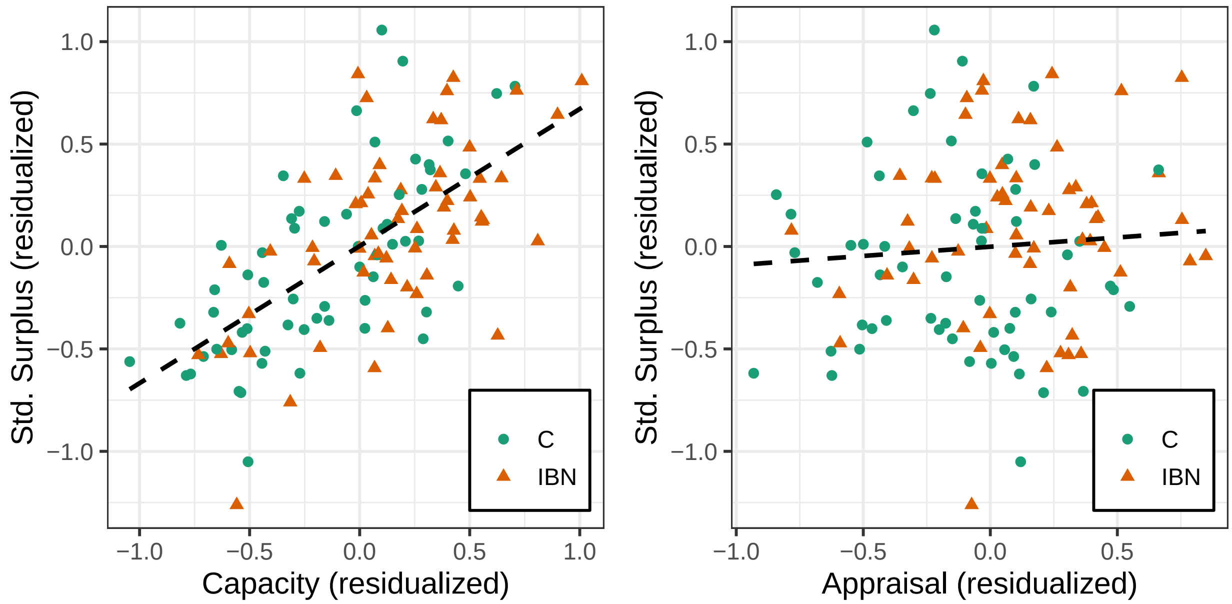

Figure 1 presents a graphical representation of the mediation analysis. The figures are constructed in three steps. First, we residualize the standardized surplus (i.e., a mean effects index of the total surplus achieved in the three simulations), capacity, and appraisal with the pre-specified covariates. Second, we average at the community level: each green dot represents a control community, while each red triangle represents a treated community. Third, we produce a scatter plot with the community-level observations and a dashed, best-fit line.

Appendix Table A.14 shows the results from a mediation analysis of our individual-level data. Consistent with our conceptual framework, we find that capacity and appraisal fully mediate the effect; the direct effect is small and insignificant. The indirect effect of capacity is more than seven times larger than appraisal skills. Interpreting our estimates in Appendix Table A.14 as causal requires sequential ignorability and the independence of the mediators (Heckman and Pinto 2015; Imai et al. 2011). In this analysis, we control for (1) all pre-specified covariates, which includes age, gender, education, position, and several design features; and (2) include both mediators in the "second-stage" regression. This increases our confidence that associations between the mediator and surplus do not simply reflect omitted respondent characteristics or simultaneous changes in the other mediator.

5.2 Structural Estimation

Our mediation analysis has two limitations: first, it assumes that we can combine survey questions to generate meaningful measures of capacity and appraisal; and second, it cannot quantify individuals' (potentially biased) beliefs about their outside options. A structural approach helps to overcome both limitations: by assuming a particular decision-theoretic model, the outcomes of the simulations — specifically, what price people negotiate and whether they accept a deal — reveal effects on individuals' negotiation capacity and beliefs about their outside option.

In our model, we represent individuals' negotiation capacity as $\theta(D_i)$. We use a linear specification $\theta(D_i) = \theta_i + D_i \cdot k$, where $k \in \mathbb{R}^1$ is the extent to which the IBN training adds (or subtracts) from individuals' negotiation capacity. Our model characterizes idiosyncratic valuation of the outside option as $\beta + u_{Di}$, where u_{Di} captures how an individual's beliefs depart from the objective value of the outside option (β). We impose a distributional assumption that $u_{Di} \sim \mathcal{N}(-\delta_0 + D_i\delta_1,\, \sigma^2)$, where $\{\delta_0, \delta_1\} \in \mathbb{R}^2$. If $\delta_0 < 0$, then untrained respondents tend to undervalue their outside option. If $\delta_1 > 0$, then trainees more generously appraise their outside option and, thus, have a higher threshold for making a deal. In the estimation, we allow $\widehat{k}$ and $\widehat{\delta}_1$ to be positive or negative, so that we can also detect any adverse effects of the training.

Regardless of whether an individual agrees, we know the maximum "negotiated price" that they could have earned given their behavior, because we set the rules for the simulations. We can estimate k using Equation (1), where this negotiated price is the outcome.[15] In Table 2, we report our estimate of $\widehat{k}$ for the full sample (row 1), as well as for subgroups defined by three covariates: whether an individual is above median age (52), female, and has more than primary education. Overall, we find a positive effect on negotiation capacity, in line with our mediation analysis. We recover particularly large estimates for older, educated men.[16]

Table 2: Structural estimates

Age	Education	Gender	N	$\widehat{k}$		$\widehat{\delta}_1$	
All	All	All	705	3.49	(1.77)	-0.11	(0.08)
$\geq$ Median	$>$ Primary	Male	50	10.01	(5.33)	-0.03	(0.27)
$\geq$ Median	$\leq$ Primary	Female	150	3.87	(3.39)	-0.19	(0.19)
$<$ Median	$>$ Primary	Male	130	1.26	(4.09)	-0.02	(0.23)
$<$ Median	$\leq$ Primary	Male	127	3.07	(3.38)	0.01	(0.18)
$\geq$ Median	$\leq$ Primary	Male	150	4.01	(3.53)	-0.38	(0.19)
$<$ Median	$\leq$ Primary	Female	83	2.28	(4.08)	-0.09	(0.23)

Table 2: Structural estimates for the full sample (row 1) and sub-groups defined by age (above and below median), education (above and below primary education), and gender. $\widehat{k}$ is our estimate of the IBN training's effect on negotiation capacity; $\widehat{\delta}_1$, our estimate of IBN training's effect on appraisal of the outside option. Sub-groups with less than 40 participants are not reported for power considerations. Standard errors in parentheses are clustered at the community level.

Our decision-theoretic model implies that an individual will agree to a deal only if the negotiated price exceeds their appraisal of their outside option. Agreeing to a deal can, thus, be expressed using a latent-index model, where $\text{Agree}_i = \mathbb{1}\{\theta_i(D_i) - \beta \geq \sigma u_i - \delta_0 + \delta_1 D_i\}$. We measure $\theta(D_i)$ as the negotiated price,

15. We also use the total surplus and the proportion of individuals who agree in each group to construct Lee Bounds to partially identify k (see Appendix Table A.17). We find that our estimates of k across sub-groups are highly correlated using the negotiated price or total surplus ($\rho = 0.87$).

16. We also estimate the ATE on our indexes for capacity and appraisal from the mediation analysis. We find that $\widehat{k}$ and the ATE on capacity are correlated at 0.61 across sub-groups.

set β to the stated value of the outside option in the script, and assume that u_i is standard normal. We can, thus, estimate δ_1 using a probit model, in which we regress whether agreement on an indicator for treatment (D_i) and the negotiated price $\theta_i(D_i)$.

Overall, we find that the training *lowered* trainees' appraisal of their outside options ($\widehat{\delta_1} < 0$): conditional on a negotiated price, trainees were more inclined to agree than their untrained counterparts. This negative estimate is the largest among older, uneducated respondents. Trainees, especially in those sub-groups, appear eager to reach an agreement, which implies that they under-value their outside option relative to control.

In the final columns of Appendix Table A.16, we calculate the change in the probability of agreement that is attributable to the training's estimated effects on capacity (k) and appraisal (δ_1). Among older, educated men, trainees are seven percentage points more likely to reach an agreement than their untrained counterparts, and this difference is entirely attributable to the increase in capacity. By contrast, older men with less than primary education are twenty points more likely to agree to a deal and three-quarters of that difference is attributable to their depressed appraisal of their outside option.

With respect to the appraisal mechanism, our mediation and structural analyses yield slightly different results. The mediation analysis suggests a small positive effect of appraisal skills on respondents' surplus (Appendix Table A.14), while the structural analysis implies that trainees *reduced* their appraisal of their outside option (Table 2). In the former exercise, we construct an appraisal index — combining knowledge measures that we assume relate to respondents' evaluation of their outside options — and assess whether that index mediates negotiation outcomes. In the latter, we assume that respondents' appraisal of their outside option determines whether they agree to a deal (conditional on the price they can negotiate) and recover estimates of their biases that best match their observed behavior. Looking across our sub-groups, we find that treatment effects on the appraisal index and $\widehat{\delta_1}$ are only weakly correlated at 0.137. Our measures of appraisal from these two exercises are not capturing the same information: our survey-based index measures, for example, whether someone can define a BATNA; our structural estimates capture how they apply that concept and, in so doing, become more choosy about the deals they agree to. The two analyses point to a gap between alerting respondents to common mistakes and correcting those mistakes.

6. Effects on Real-World Forest Use

We randomized at the community level and can explore whether the training affected real-world forest use measured six months after the training. In Table 3, we find a reduction of 0.26 standard deviations ($p = 0.08$) in forest use by external actors (primarily, pit-sawers) in treatment communities. Respondents report reduced forest use by external actors, and independent environmental assessments, which are not contaminated by demand effects, also uncover less activity related to agriculture, logging, or mining on communal forestland.[17] Trainees report more engagement around forest use in their communities and are more likely to report that their community has a rule against logging on communal forestland without permission. While external forest use is lower, respondents do not report receiving fewer benefits from external investments in their community forest. These findings are consistent with trainees — many of whom play influential roles in managing their community's forest land (see Appendix Table A.18) — setting a higher bar for the agreements they reach with external investors.[18] Although the effect is not statistically significant, we find that trainees demand a higher average price for clear-cutting their communal forestland, suggesting that their appraisal of the forest has increased.

Table 3: Average Treatment Effects of IBN on Community Forest Use

Outcome	ATE	Std. Error	*p*-value	N
Preferences around Forest Use				
Does *Not* Want to Reduce Logging Activity	0.031	(0.020)	0.136	705
Price Demanded to Clear Forest (logged)	0.151	(0.264)	0.568	705
Engagement around Forest Use				
Count of Neighbors Consulted about Forest in Last Week	0.850	(0.497)	0.090	677
Rule in Community against Logging w/o Permission	0.091	(0.029)	0.002	703
Forest Use by External Actors†	-0.256	(0.143)	0.077	705
External Forest Use (EA)°	-0.455	(0.284)	0.115	693
External Logging or Investment (SVY)°	-0.197	(0.141)	0.165	703
Benefits from External Forest Use†	0.050	(0.125)	0.691	705

Table 3: Average treatment effect estimates on negotiation outcomes using Equation 1. Standard errors in parentheses are clustered at the community level. † stands for mean-effects index; ○ stands for selected components of mean-effects index. Exploratory analysis that was not pre-specified.

We can also rule out two alternative explanations. First, we do not find that trainees prefer less logging; if anything, trainees are more likely to oppose reducing logging on communal forestland. Second, we do not see evidence of spatial spillovers — namely, control communities that lie close to treatment communities do not see increased forest use by external actors (see Appendix Table A.12). Pit-sawers are not simply displaced to nearby control communities.

Finally, one might worry that trainees' gain is to the detriment of other households in their communities. In Appendix Table A.11, we show that randomly selected households (who are never eligible for the IBN training) do not report significantly fewer benefits from external forest use or less satisfaction with their leadership in treatment communities. The absence of such within-community spillovers suggests that the IBN training is not exacerbating accountability problems that exist in these communities with unelected leaders.

17. Appendix Table A.13 presents effects on remotely sensed deforestation, where we find a statistically insignificant reduction.

18. Appendix Table A.18 uses data from our control group to show that educated men play larger roles in managing communal forestland. Our structural estimates in Table 2 imply that these types of trainees saw improvements in capacity, with no meaningful adverse effects on appraisal.

7. Conclusion

This paper studies an intensive IBN training, designed to enable community leaders in rural Liberia to better negotiate over their natural resources. Using a set of behavioral games, we find that trainees are 27% more likely to reach beneficial agreements, and those deals pay them 42% more relative to the performance of untrained leaders from control communities six months after the training. The changes in these behavioral outcomes correspond to reductions in real-world forest use without a decline in the benefits that flow from such investments. These findings are consistent with trained leaders in treatment communities demanding more of investors who want to exploit communal forestland.

Both our mediation analysis and structural estimates indicate that the positive effects we uncover are primarily attributable to trainees' increased capacity to find beneficial (i.e., positive-sum) deals. We do not find that the training improved individuals' appraisal of their outside options. If anything, our structural estimates show that the training made people, especially trainees with limited education, keen to strike deals, which is consistent with them undervaluing the benefits associated with walking away. Future IBN training should better emphasize that while win-win agreements can exist, not all deals are win-win or worth making.

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Supporting Information

Interest-based Negotiation over Natural Resources: Experimental Evidence from Liberia

Following text to be published online.

Contents

A. Sampling

A.1 Evaluation Sample

In collaboration with Liberia Chainsaw and Timber Dealers Union and our implementing partner, we identified communities in Bong County hosting active pit-sawing (also referred to as chainsaw milling) crews and with community forests. Given concerns about the unsustainable growth of unregulated chainsaw milling, our evaluation sample was drawn (primarily) from these communities.

Communities that do not have a communal forest — a forested area where individuals from the community enjoy usufruct rights — are further excluded from the evaluation sample. This includes communities where the community forest is only used for traditional purposes (e.g., secret society meetings) and, thus, can not be entered by outsiders. This exclusion criterion was motivated by a community monitoring treatment, which was cross-randomized with the IBN training that is the focus of this pre-analysis plan.

Table A.1 presents the descriptive statistics on the study sample as well as the Bong County and Liberia.

Table A.1: Characteristics of Sampled Communities

Feature	Mean	Median	SD	Min	Max	Missing	N
Liberia							
Population	259.40	53.00	1177.74	1.00	41182.00	0	13365
Urban	0.04	0.00	0.19	0.00	1.00	0	13365
Under 18	0.46	0.48	0.12	0.00	1.00	0	13365
Literate	0.35	0.33	0.23	0.00	1.00	0	13365
No School	0.74	0.76	0.21	0.00	1.00	0	13365
Wealth Index	0.93	0.80	0.75	0.00	2.56	0	13365
Displaced by War	0.47	0.43	0.41	0.00	1.00	0	13365
Bong County							
Population	125.04	39.00	693.58	1.00	30380.00	0	2667
Urban	0.02	0.00	0.15	0.00	1.00	0	2667
Under 18	0.46	0.48	0.11	0.00	0.80	0	2667
Literate	0.27	0.24	0.20	0.00	1.00	0	2667
No School	0.82	0.86	0.18	0.00	1.00	0	2667
Wealth Index	0.76	0.60	0.67	0.00	2.56	0	2667
Displaced by War	0.37	0.13	0.41	0.00	1.00	0	2667
Study Sample							
Population	300.04	127.75	437.27	12.50	2639.00	0	120
Urban	0.05	0.00	0.21	0.00	1.00	0	120
Under 18	0.46	0.47	0.06	0.12	0.65	0	120
Literate	0.31	0.31	0.14	0.03	0.63	0	120
No School	0.78	0.80	0.14	0.48	1.00	0	120
Wealth Index	0.73	0.59	0.49	0.00	2.41	0	120
Displaced by War	0.36	0.25	0.34	0.00	1.00	0	120

Table A.1: Descriptive statistics on sampled communities from census.

A.2 Sampling of Respondents

We used a random walk to randomly select four households, stratified by quarter (i.e., neighborhood). We also surveyed the chief and five community leaders, who had to hold one of the following positions: (1) Town Chief, (2) Quarter Chief, (3) Women's Leader, (4) Midwife, (5) Youth Leader, (6) Hunter Leader, (7) Chief Elder, or (8) Teacher. By virtue of their positions, community leaders tend to be more involved in decision-making. More importantly, only leaders who held these positions could be recruited for the negotiation training (see Section 3.3). All consenting respondents completed an in-person survey and received a small gift of soap as a thank you. Only the community leaders completed the negotiation simulations described in Section B.1.

Table A.2: Composition of trainees in treated and control communities

Position	Control	IBN
Town Chief	16%	17%
Women's Leader	16%	16%
Midwife	17%	16%
Youth Leader	16%	15%
Chief Elder	17%	18%
Landlord	15%	16%
	97%	98%

Table A.2: Composition of trainees in treated and control communities.

Table A.3: Characteristics of Households in Sampled Communities

Feature	Mean	Median	SD	Min	Max	Missing	N
Female	0.26	0	0.44	0	1	0	476
Age	43.35	42	12.43	18	85	0	476
Any Edu.	0.63	1	0.48	0	1	0	476
Any Sec. Edu.	0.34	0	0.47	0	1	0	476
Born in Community	0.79	1	0.41	0	1	0	476
Owns Land	0.45	0	0.50	0	1	0	476
Christian	0.99	1	0.08	0	1	9	467
Kpelle	0.88	1	0.32	0	1	0	476
Bassa	0.05	0	0.22	0	1	0	476

Table A.3: Descriptive statistics on households in sampled communities. Owns Land is a dummy equal to 1 if the respondent owns land. Kpelle and Bassa are two ethnicities in Liberia.

Table A.4: Characteristics of Negotiation Sample

Feature	Mean	Median	SD	Min	Max	Missing	N
Female	0.35	0	0.48	0	1	8	705
Age	52.23	52	14.15	19	99	8	705
Any Edu.	0.50	0	0.50	0	1	8	705
Any Sec. Edu.	0.28	0	0.45	0	1	8	705
Born in Community	0.81	1	0.39	0	1	8	705
Owns Land	0.55	1	0.50	0	1	8	705
Christian	0.99	1	0.08	0	1	16	697
Kpelle	0.89	1	0.31	0	1	8	705
Bassa	0.06	0	0.23	0	1	8	705

Table A.4: Descriptive statistics on the negotiation sample. Owns Land indicates whether an individual owns land; Kpelle and Bassa are major ethnic groups in Bong County, Liberia.

B. Measurement

B.1 Negotiation Simulations

The simulations always involved two enumerators and the respondent. One enumerator was allied with the respondent as the seller. This enumerator told the respondent that they would serve as a "trusted advisor" during the negotiations: "You will counsel me on what to say and do. You can ask me to say what you are feeling – to ask questions, raise problems, make offers." During piloting, we found that respondents were more comfortable and communicative if they had someone on their side and did not have to directly interact with the buyer. The enumerator allied with the respondent was not allowed to coach or guide the respondent or re-interpret the respondent's directives. Their role was strictly circumscribed: they passed information between the respondent and the buyer.

The second enumerator played the buyer. To try and ensure that every respondent played against the same buyer, the enumerators were given strict instructions about how to play (e.g., what counteroffers they could make, what deals they could accept). We filmed enumerators during piloting and coached them to increase compliance with these instructions prior to data collection.

The enumerator allied with the respondent read the script of the simulation. They then asked a set of comprehension questions to ensure that the respondent understood key details. If the respondent missed any of these comprehension checks, the enumerator went back over the scenario. We provide the text of the three negotiation simulation, including the instructions followed by the enumerator (i.e., buyer) and the comprehension checks in the pre-analysis plan (Christensen et al. 2021).

The respondent was told that each simulation would last a maximum of ten minutes. They were reminded: "It is ok if you don't make a deal in that time, and you can always 'walk away' if you think you can't make a good deal." We told respondents that they would receive a small bonus for reaching a good deal but did not reveal the formula to respondents.

The simulations could be played in three different orders:

1. (a) Telecom, (b) Woodbuyer, (c) Peanut Farmer;
2. (a) Woodbuyer, (b) Peanut Farmer, (c) Telecom; and
3. (a) Peanut Farmer, (b) Telecom, (c) Woodbuyer

We randomized which ordering the respondent played in. As we note below, in our analysis of control-group data, we find that playing the peanut-farmer simulation first had a demoralizing effect and include this in our covariate adjustment (Christensen et al. 2021). .

B.2 Household Survey

We administered an in-person survey to the heads of all sampled households and the community leaders.

B.3 Environmental Assessment

At endline enumerators completed an independent environmental assessment (EA) modeled on the patrols conducted under the citizen monitoring program. Two enumerators were given three hours to complete an EA and instructed to take a "wide walk and try and see as much of the community forest as possible." They could be accompanied by someone from the community (often a requirement for an outsider to secure entry), but this could not include a trained citizen monitor. Enumerators were provided with a simple map of the community forest that was drawn by a key informant (who also could not be a citizen monitor) the day before the EA took place. During the EA, enumerators used a mobile survey to record and geo-locate forest use activities (e.g., small-scale logging, charcoal production).

B.4 Index Construction

When multiple outcome variables fall under a hypothesis, we construct a mean-effects index (Kling, Liebman, and Katz 2007). To create an index from K variables, we first reverse the scale where necessary such that a higher value indicates a better outcome across all variables. We then compute $\widetilde{y}_i = \frac{1}{K}\sum^{K}\left(\frac{y_{ik} - \mu_{0k}}{\sigma_{0k}}\right)$, where μ_{0k} and σ_{0k} are the estimated control-group mean and standard deviation for outcome k. Our estimates thus represent standard deviation differences relative to the control group. Following Kling, Liebman, and Katz (2007), in case y_{ik} is missing but another sub-component of the family is measured, we impute the mean from the same treatment arm.

C. Research Design

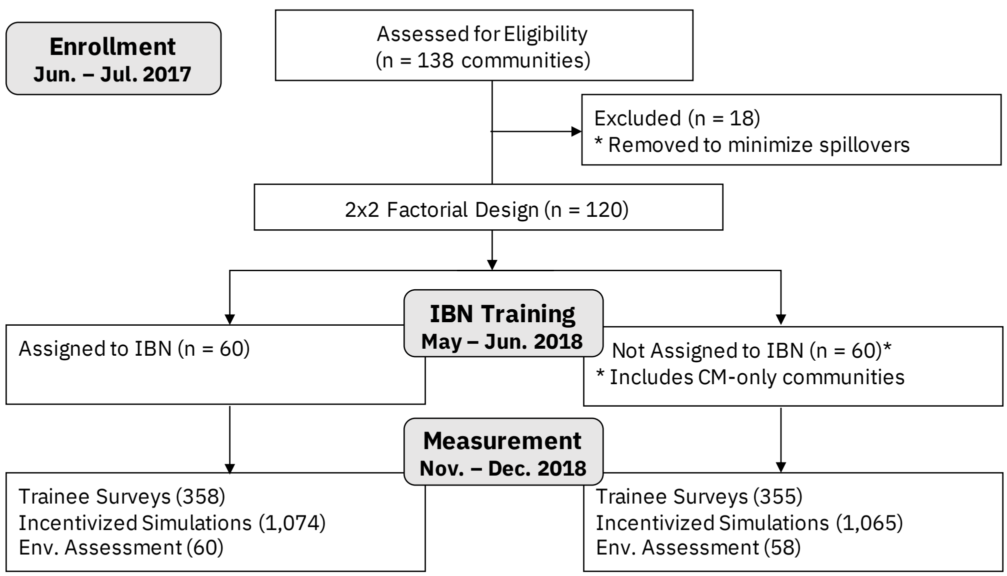
C.1 Ethics and Permissions

Institutional Review Boards at UCLA (18-001684), UCL (10205/003), and NYU (FY2017-912) have approved the study. All subjects gave consent to participate in our study. Two communities (Foequelleh and Kpolyoyah) do not permit outsiders in their community forest and refused the environmental assessment.

Parley Liberia consulted with government and local authorities prior to implementation and data collection to obtain their permission to operate in their communities. Parley Liberia also received a written endorsement of the project from the Bong County Superintendent, Selena Polson Mappy.

C.2 Study Design

Figure A.1: Study Design



C.3 Randomization

We have a balanced full-factorial design that crosses the IBN training with a community monitoring program that is the subject of a separate study. We assigned treatments using a restricted, blocked randomization. The blocking is done in two stages. First, we created district-blocks that consisted of groupings of geographically close districts. These district blocks group districts as follows:

1. Salala and Suakoko,
2. Fuamah and Sanayea,
3. Zota and Panta-Kpa,

4. Jorquelleh, and

5. Kokoya and Saclepea.

Then, within each of these district blocks, we applied a second level of blocking based on minimum-Mahalanobis distance clustering on the covariates listed in Table A.3. This created blocks of four communities each. Randomization took place within these blocks of four to one of four conditions: (1) Control, (2) Community Monitoring, (3) Negotiation, or (4) Community Monitoring and Negotiation.

The restriction on the randomization applies what Bruhn and McKenzie (2009) refer to as the "big stick," which limits the set of possible assignments to those that satisfy a covariate balance criterion. We produced 50,000 candidate randomizations and then accepted as candidate randomizations the 6,003 for which the minimum naive p-value of the F-test from a regression of each of these blocking covariates on the treatment indicators was above 0.30. We then randomly selected one of the 6,003 randomizations as our actual random assignment. This is displayed in Figure A.2. Morgan and Rubin (2012) point out that heavily restricted randomization can yield departures from uniform first- and second-order assignment probabilities, and when this is the case, one needs to account for such variation for unbiased inference. In our case, the departures appear to be very mild, as shown in the pre-analysis plan (Christensen et al. 2021). As such, we analyze the data as if we used complete block random assignment.

Figure A.2: Treatment Assignment

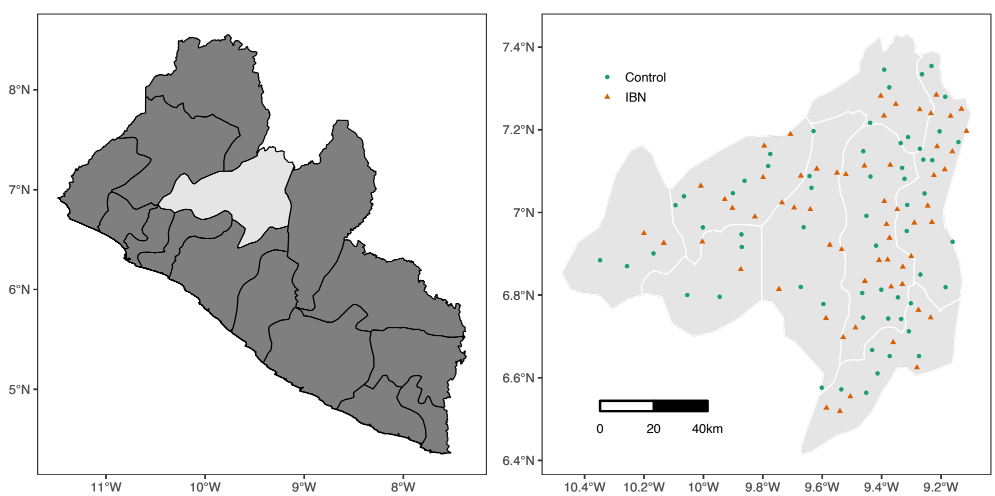


Figure A.2: Treatment assignment for the 120 communities in the evaluation sample. Communities were organized into blocks and then randomized into one of four groups: (1) Control and (2) IBN training.

Community locations and eligibility were difficult to assess ex-ante due to incomplete or inaccurate administrative data. Moreover, we could not verify that every community in our sample had a communal forest. As such, we use the 18 communities that we trimmed to maximize the distance between units as replacement sites. These replacement sites were then ordered on the basis of their Mahalanobis distance from the covariate values of other sites within their respective district-clusters. These replacement sites were to be drawn upon in this ordering in case any of the assigned sites was inaccessible, ineligible, or otherwise unavailable for use in the experiment.

Minimizing Geographic Spillovers. To minimize the risk of spatial spillovers, we deliberately trimmed our evaluation sample prior to randomization. Our algorithm for trimming is straightforward. Suppose that N units are eligible for inclusion in the evaluation sample, but we can only afford to include $M < N$. For each community $i \in N$, we computed the minimum (great-circle) distance between i and all other units $-i$. We determined the pair of units that are most proximate and eliminated one unit in this pair, leaving us with $N-1$ eligible units. We repeat this process until M units remained. In our case $N = 138$, and we could afford data collection and programming in $M = 120$ communities.

We did not run a baseline survey. The endline data described below was collected in November and December 2018. To limit attrition, we tracked down and surveyed a small number of respondents in January and February of 2019.

C.4 Balance

We did not conduct a baseline survey. Publicly available pre-treatment data at the community level are used to assess balance. Table A.5 presents the balance tests.

Table A.5: Balance Table

Measure	Control Mean	Control SD	IBN	Standard Error	*p*-value	N
Population 2012 (Landscan)	807.68	(1510.67)	-232.51	(207.08)	0.26	120
Nightlights 2013 (NOAA)	0.11	(0.69)	-0.09	(0.1)	0.37	120
Nightlights 2012 (NOAA)	0.07	(0.53)	-0.07	(0.07)	0.33	120
Elevation (Worldclim)	249.45	(55.09)	7.16	(6.46)	0.27	120
Precipitation (Worldclim)	2140.07	(151.07)	-30.25	(18.73)	0.11	120
Temperature (Worldclim)	254.20	(5.4)	-0.64	(0.46)	0.17	120
Forest Loss (Global Forest Change)	0.14	(0.03)	-0.01	(0.01)	0.23	120
Distance to Monrovia	160.02	(32.66)	4.07	(2.9)	0.16	120
Distance to Primary Road (LISGIS)	9.97	(7.96)	1.31	(1.19)	0.27	120
Distance to Any Road (LISGIS)	2.11	(2.72)	0.82	(0.48)	0.09	120
Longitude	-9.53	(0.31)	0.04	(0.02)	0.12	120
Latitude	6.96	(0.21)	0.01	(0.03)	0.59	120

Table A.5: Balance table estimated using community-level data.

C.5 Estimation

Given random assignment of the negotiation treatment, we improve precision in estimating the ATE by fitting the following centered-interaction specification (Lin 2013):

$$
\begin{aligned}
Y_{sibc} = \alpha &+ \beta \mathbb{1}(\text{NEG})_{bc} \\
&+ \phi_1 \tilde{\mathbb{1}}(\text{CM})_{bc} + \phi_2 \mathbb{1}(\text{NEG})_{bc} \times \tilde{\mathbb{1}}(\text{CM})_{bc} \\
&+ \sum_{b=1}^{B-1} [\phi_{3b} \tilde{\mathbb{1}}_b + \phi_{4b} \mathbb{1}(\text{NEG})_{bc} \times \tilde{\mathbb{1}}_b] \\
&+ \sum_{s=1}^{2} [\phi_{5s} \tilde{\mathbb{1}}_s + \phi_{6s} \mathbb{1}(\text{NEG})_{bc} \times \tilde{\mathbb{1}}_s] \\
&+ \sum_{k}^{K} [\phi_{7k} \tilde{X}_{k,ibc} + \phi_{8k} \mathbb{1}(NEG) \times \tilde{X}_{k,ibc}] + \varepsilon_{sibc}
\end{aligned} \tag{2}
$$

where Y_{sibc} corresponds to the outcome for simulation s for individual i in district randomization block b and community c. $\mathbb{1}(\text{NEG})_{bc}$ is an indicator variable for whether community c in block b was selected for the negotiation training. (For individual-level outcomes, we omit the s subscript; for community-level outcomes, we omit si subscripts.) We control for whether the community also received a second randomized treatment arm (subject to a separate analysis), which was a citizen monitoring program ($\mathbb{1}(\text{CM})_{bc}$). The $\tilde{\cdot}$ operator means that the variable is centered. We include district block fixed effects ($\mathbb{1}_b$, omitting one because of the constant term) and, for analyses that estimate average effects across simulations, simulation fixed effects ($\mathbb{1}_s$, omitting one because of the constant term). We also include additional individual-level covariates in $\mathbf{X}_{ibc}$: (1) the respondent's educational attainment; (2) age; (3) gender; (4) role in their community; (5) whether the respondent was randomly assigned to play the peanut-farmer simulation first; and (6) fixed effects for the enumerators who administered the simulations.[19]

The term β is our average treatment effect estimate for the negotiation training. (Because the $\tilde{\mathbb{1}}(\text{CM})_{bc}$ term is centered, β estimates the marginal average treatment effect of monitoring, averaging over communities both with and without the citizen monitoring.)

As educational attainment and gender are included in $\mathbf{X}_{ibc}$, we can recover the moderation analysis specified in Section 7 from this same equation. ϕ_{8k} is the coefficient on the interaction of the centered covariates with our treatment indicator. These coefficients estimate the deviation from the ATE within the subgroup of interest. We cluster our standard errors on community, which is the unit of assignment.

C.6 Moderated-Mediator Analysis

Recall that Hypothesis (H7) proposes that the treatment will moderate the extent to which agreement will translate into surplus. This is a "moderated mediator" hypothesis: the treatment moderates the mediation

19. As we note in Section B.1, among respondents in control communities, playing the peanut-farmer simulation first appeared to have a demoralizing effect.

effect of agreement.

To test this, we work with a specification that takes the same form as Equation 3, except that we also include an indicator for agreement as well as the interaction between agreement and the treatment:

$$
\begin{aligned}
\text{Surplus}_{sibc} = \alpha &+ \beta_1 \mathbb{1}(\text{NEG})_{bc} + \beta_2 \mathbb{1}(\text{Agree})_{sibc} + \beta_3 \mathbb{1}(\text{NEG})_{bc} \times \mathbb{1}(\text{Agree})_{sibc} \\
&+ \phi_1 \tilde{\mathbb{1}}(\text{CM})_{bc} + \phi_2 \mathbb{1}(\text{NEG})_{bc} \times \tilde{\mathbb{1}}(\text{CM})_{bc} \\
&+ \sum_{b=1}^{B-1} [\phi_{3b} \tilde{\mathbb{1}}_b + \phi_{4b} \mathbb{1}(\text{NEG})_{bc} \times \tilde{\mathbb{1}}_b] \\
&+ \sum_{s=1}^{2} [\phi_{5s} \tilde{\mathbb{1}}_s + \phi_{6s} \mathbb{1}(\text{NEG})_{bc} \times \tilde{\mathbb{1}}_s] \\
&+ \sum_{k}^{K} [\phi_{7k} \tilde{X}_{k,ibc} + \phi_{8k} \mathbb{1}(NEG) \times \tilde{X}_{k,ibc}] + \varepsilon_{sibc}
\end{aligned} \tag{3}
$$

Hypothesis (H7) amounts to proposing that β_3 would be positive.

D. Additional Analysis

D.1 Control group levels

Table A.6: Control-group Levels for Pre-Specified Outcomes

Outcome	Mean	SD	Min	Max	N
MNP: Manipulation Checks					
Attended Negotiation Training	0.01	0.10	0	1	186
Correctly Reports Length of Training	0.00	0.00	0	0	186
Correctly Reports Location of Training	0.01	0.07	0	1	186
H1: Knowledge of IBN					
Correctly Defines IBN	0.67	0.47	0	1	186
Distinguishes Interest and Position	0.55	0.50	0	1	186
Count of IBN Concepts Invoked	0.58	0.50	0	1	186
Recognizes Potential for Win-Win	0.63	0.48	0	1	186
H2: Knowledge of Inter-personal Skills					
Count of Tactics Listed to Build a Positive Relationship	2.14	0.78	1	5	186
Acknowledges Importance of Positive Relationship	0.47	0.50	0	1	186
H3: Deployment of IBN Skills					
Count of IBN Skills Used in Peanut-Farmer Simulation	0.97	0.81	0	4	186
Count of Questions asked about Buyer	0.56	0.65	0	2	186
Count of Solutions Discovered in Woodbuyer Simulation	0.28	0.50	0	2	186
H4: Deployment of Inter-personal Skills					
Does Not Display Anger or Frustration	0.93	0.26	0	1	558
H5: Positive Surplus					
Achieves Surplus Greater than Zero	0.22	0.41	0	1	558
H6: Total Surplus					
Surplus Achieved	6.55	26.21	-50	60	558

Table A.6: Summary statistics for pre-specified outcomes using data from respondents who were not assigned to either the negotiation training or the other cross-randomized intervention.

We also use data from our control group to look at the probability of agreement conditional on the negotiated surplus, the negotiated price minus the BATNA specified in the simulation script.

Figure A.3: Probability of Agreement Conditional on the Negotiated Surplus in Control Group

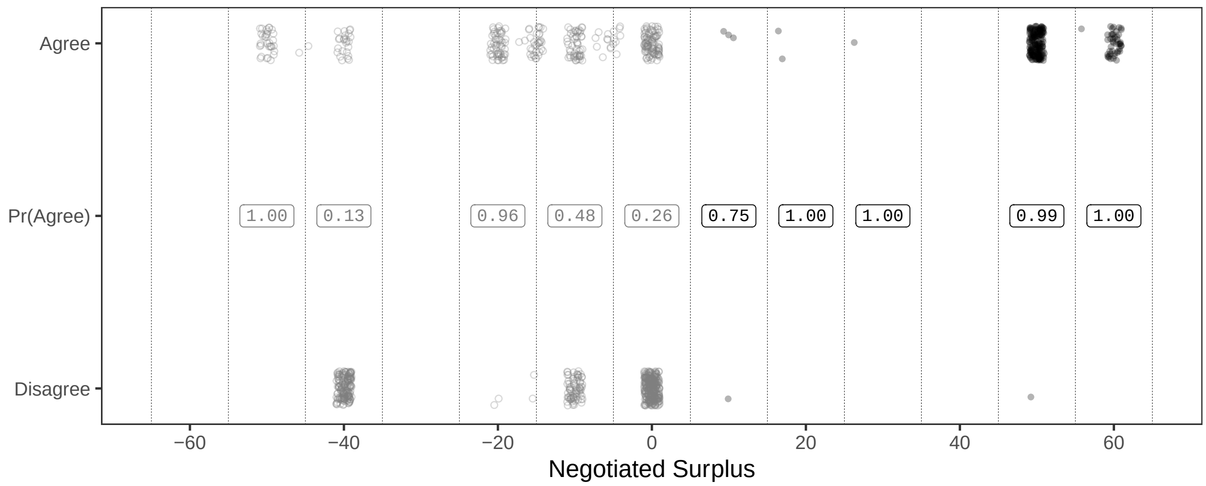


Figure A.3: Using data from our control group, we calculate the surplus that a respondent could have earned in a negotiation, which is the price they negotiate minus the value of the outside option as specified in the simulation script. We plot this value along the x-axis, jittering the points to prevent over-plotting, and whether they agreed along the y-axis. We group observations into bins that are ten units wide and calculate the probability of agreeing to a deal in each of those bins; these probabilities are printed in the middle of the figure.

D.2 Full PAP Analysis

Tables A.7 and A.8 present the pre-specified analysis with and without covariate adjustment discussed in Christensen et al. (2021).

Table A.7: Pre-specified Outcomes with Covariate Adjustment

Outcome	ATE	Std. Error	*p*-value	N
MNP: Manipulation Checks				
Mean-effects Index	11.637	(0.252)	0.00	705
Attended Negotiation Training	0.916	(0.021)	0.00	705
Correctly Reports Length of Training	0.930	(0.02)	0.00	705
Correctly Reports Location of Training	0.926	(0.02)	0.00	705
H1: Knowledge of IBN				
Mean-effects Index	0.335	(0.068)	0.00	705
Correctly Defines IBN	0.128	(0.031)	0.00	705
Distinguishes Interest and Position	0.039	(0.038)	0.31	705
Count of IBN Concepts Invoked	0.105	(0.04)	0.01	705
Recognizes Potential for Win-Win	0.125	(0.035)	0.00	705
H2: Knowledge of Inter-personal Skills				
Mean-effects Index	-0.082	(0.076)	0.28	705
Count of Tactics Listed to Build a Positive Relationship	0.029	(0.059)	0.62	705
Acknowledges Importance of Positive Relationship	-0.078	(0.038)	0.04	705
H3: Deployment of IBN Skills				
Mean-effects Index	0.214	(0.084)	0.01	705
Count of IBN Skills Used in Peanut-Farmer Simulation	0.135	(0.071)	0.06	705
Count of Questions asked about Buyer	0.037	(0.058)	0.52	705
Count of Solutions Discovered in Woodbuyer Simulation	0.125	(0.046)	0.01	705
H4: Deployment of Inter-personal Skills				
Does Not Display Anger or Frustration	0.025	(0.014)	0.06	2115
H5: Positive Surplus				
Achieves Surplus Greater than Zero	0.060	(0.023)	0.01	2115
H6: Total Surplus				
Surplus Achieved	2.742	(1.472)	0.07	2115
H7: Moderated-Mediator				
Differential Effect of Agreement on Surplus for Trainees	4.845	(2.41)	0.05	2115

Table A.7: Pre-specified Outcomes with Covariate Adjustment. Standard errors clustered on community.

Table A.8: Pre-specified Outcomes without Covariate Adjustment

Outcome	ATE	Std. Error	*p*-value	N
MNP: Manipulation Checks†	11.728	(0.267)	0.00	713
Attended Negotiation Training	0.923	(0.023)	0.00	713
Correctly Reports Length of Training	0.937	(0.021)	0.00	713
Correctly Reports Location of Training	0.934	(0.021)	0.00	713
H1: Knowledge of IBN†	0.385	(0.076)	0.00	713
Correctly Defines IBN	0.156	(0.045)	0.00	713
Distinguishes Interest and Position	0.045	(0.036)	0.21	713
Count of IBN Concepts Invoked	0.118	(0.039)	0.00	713
Recognizes Potential for Win-Win	0.138	(0.035)	0.00	713
H2: Knowledge of Inter-personal Skills†	-0.073	(0.071)	0.31	713
Count of Tactics Listed to Build a Positive Relationship	0.046	(0.062)	0.46	713
Acknowledges Importance of Positive Relationship	-0.083	(0.037)	0.03	713
H3: Deployment of IBN Skills†	0.267	(0.085)	0.00	713
Count of IBN Skills Used in Peanut-Farmer Simulation	0.152	(0.073)	0.04	713
Count of Questions asked about Buyer	0.070	(0.058)	0.23	713
Count of Solutions Discovered in Woodbuyer Simulation	0.148	(0.043)	0.00	713
H4: Deployment of Inter-personal Skills	0.032	(0.014)	0.02	2139
H5: Positive Surplus	0.068	(0.023)	0.00	2139
H6: Total Surplus	3.166	(1.472)	0.03	2139
H7: Moderated-Mediator	4.578	(2.283)	0.05	2139

Table A.8: Pre-specified Outcomes without Covariate Adjustment. † stands for ean-effects index. Standard errors clustered on community. Standard errors clustered on community.

D.3 Pre-specified heterogeneous treatment effects

Tables A.9 and A.10 present the pre-specified heterogeneous treatment effects discussed in Christensen et al. (2021).

Table A.9: Heterogeneous Treatment Effects for Above Primary Education

Outcome	ATE	HTE	SE	p	N
H1: Knowledge of IBN†	0.335	0.018	(0.176)	0.92	705
Correctly Defines IBN	0.128	0.009	(0.091)	0.92	705
Distinguishes Interest and Position	0.039	0.146	(0.084)	0.09	705
Count of IBN Concepts Invoked	0.105	-0.139	(0.091)	0.13	705
Recognizes Potential for Win-Win	0.125	0.006	(0.1)	0.95	705
H2: Knowledge of Inter-personal Skills†	-0.082	0.021	(0.18)	0.91	705
Count of Tactics Listed to Build a Positive Relationship	0.029	-0.128	(0.148)	0.39	705
Acknowledges Importance of Positive Relationship	-0.078	0.097	(0.093)	0.30	705
H3: Deployment of IBN Skills†	0.214	-0.090	(0.247)	0.72	705
Count of IBN Skills Used in Peanut-Farmer Simulation	0.135	0.088	(0.194)	0.65	705
Count of Questions asked about Buyer	0.037	-0.018	(0.16)	0.91	705
Count of Solutions Discovered in Woodbuyer Simulation	0.125	-0.139	(0.122)	0.26	705
H4: Deployment of Inter-personal Skills	0.025	0.015	(0.036)	0.67	2115
H5: Positive Surplus	0.060	-0.032	(0.055)	0.57	2115
H6: Total Surplus	2.742	-1.004	(3.423)	0.77	2115

†: Mean-effects index. Standard errors clustered on community.

Table A.9: Pre-specified heterogeneous treatment effects by education. † stands for ean-effects index. Standard errors clustered on community. Standard errors clustered on community.

Table A.10: Heterogeneous Treatment Effects for Women

Outcome	ATE	HTE	SE	p	N
H1: Knowledge of IBN†	0.329	0.051	(0.147)	0.73	705
Correctly Defines IBN	0.126	0.043	(0.072)	0.55	705
Distinguishes Interest and Position	0.036	0.031	(0.081)	0.70	705
Count of IBN Concepts Invoked	0.103	0.001	(0.085)	0.99	705
Recognizes Potential for Win-Win	0.125	-0.015	(0.084)	0.86	705
H2: Knowledge of Inter-personal Skills†	-0.081	0.314	(0.157)	0.05	705
Count of Tactics Listed to Build a Positive Relationship	0.025	0.266	(0.115)	0.02	705
Acknowledges Importance of Positive Relationship	-0.075	0.059	(0.084)	0.49	705
H3: Deployment of IBN Skills†	0.208	-0.320	(0.173)	0.07	705
Count of IBN Skills Used in Peanut-Farmer Simulation	0.131	-0.281	(0.156)	0.07	705
Count of Questions asked about Buyer	0.032	-0.182	(0.113)	0.11	705
Count of Solutions Discovered in Woodbuyer Simulation	0.124	-0.040	(0.078)	0.61	705
H4: Deployment of Inter-personal Skills	0.027	-0.053	(0.031)	0.09	2115
H5: Positive Surplus	0.058	-0.021	(0.039)	0.58	2115
H6: Total Surplus	2.626	-1.111	(2.591)	0.67	2115

Table A.10: Pre-specified heterogeneous treatment effects by gender. † stands for ean-effects index. Standard errors clustered on community. Standard errors clustered on community.

D.4 Within-Community Spillovers

Four households (non-trainees) were randomly sampled in each community. We estimate the ATE on the non-trainees sample to observe if there are with-in community spillover. Table A.11 shows that the changes in material benefits from external forest use are similar to trainees. In addition, we do not observe change in satisfaction with leadership. Namely, in control communities, 10.5% of HHs report being unsatisfied with leadership, while in communities with IBN trainees, 11.6% of HHs.

Table A.11: Within-Community Spillovers

Outcome	ATE	Std. Error	*p*-value	N
Benefits from External Forest Use†	0.073	(0.167)	0.662	476
Satisfaction with Leadership				
Overall satisfaction	-0.028	(0.040)	0.434	476
Satisfaction related to the community forest	-0.013	(0.033)	0.690	476

Table A.11: Within-community spillover from four households (non-trainees) randomly sampled in each community. Standard errors in parentheses are clustered at the community level. † stands for mean-effects index. * stands for sample restricted to control communities.

D.5 Spatial Spillovers

Table A.12 presents the estimates from equation $Y_{sic} = \alpha_s + \beta$ Distance to IBN $+ \varepsilon_{sic}$. We restrict attention to control communities and measure distance to the nearest IBN community (mean = 6.2 km).

Table A.12: Spatial spillover

Outcome	Estimate ($\widehat{\beta}$)	Std. Error	*p*-value	N*
H1: Knowledge of IBN†	-0.003	(0.016)	0.87	355
H2: Knowledge of Inter-personal Skills†	0.003	(0.015)	0.84	355
H3: Deployment of IBN Skills†	0.028	(0.022)	0.24	355
H4: Deployment of Inter-personal Skills	0.003	(0.002)	0.32	6,333
H5: Positive Surplus	0.003	(0.005)	0.60	6,333
H6: Total Surplus	0.066	(0.230)	0.78	6,333
Expl: Forest Use by External Actors	-0.011	(0.028)	0.71	351

Table A.12: Estimates from the spatial spillover. Standard errors in parentheses are clustered at the community level. † stands for mean-effects index. * stands for sample restricted to control communities.

D.6 Analysis of Remotely Sensed Deforestation

Table A.13 presents the ATE on remotely sensed deforestation. The outcome is the count of deforested pixels (30 m^2 / pixel) on a circular area centered on activities detected in the environmental assessment. We chose the area based on the distances covered in the EAs (in control)

Table A.13: Analysis of remotely senses deforastation

Outcome	ATE	Std. Error	p	N
Deforestation in CF (Area = 0.79 sq km.)	-16.011	(41.915)	0.703	120
Deforestation in CF (Area = 1.85 sq km.)	-16.607	(60.515)	0.784	120

Table A.13: Average treatment effect estimates on the count of deforested pixels (30 m^2 / pixel) on a circular area based on the distance covered in the environmental assessment. Each specification includes covariates for forest stock and pre-treatment deforestation. Standard errors in parentheses are clustered at the community level.

E. Mediation Analysis

E.1 Capacity and Appraisal Indexes

We construct mean-effects indexes a la Kling, Liebman, and Katz (2007) to measure capacity and appraisal. The first index combines answers from correctly defining IBN, understating the importance of agreements that work for both parties, importance of developing strategies to improve relationship and preparation to understand the interest of the other party as well as behaviour from the simulations — i.e., respondent is able to find win-win agreements in the “telecom” and “woodbuyer” simulation. On the other hand, the second one combines answers from correctly defining IBN, understanding the difference between interest and position and how to appraise their outside option.

E.2 Full mediation analysis

Table A.14: Mediation Analysis

Panel A: First-Stage Estimates			
	Appraisal	**Capacity**	**Std. Surplus**
Treatment	0.245 (0.063)***	0.309 (0.073)***	-0.005 (0.065)
Capacity			0.499 (0.033)***
Appraisal			0.082 (0.033)**
Panel B: Decomposition of the Total Effect of IBN on Std. Surplus			
Total Effect	0.169 (0.078)**	**Indirect: Capacity**	0.154 (0.037)***
Direct Effect	-0.005 (0.065)	**Indirect: Appraisal**	0.020 (0.010)**

Table **??**: First- and second-stage from mediation analysis. Standard errors in parentheses are clustered at the community level. Significance: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

E.3 Mediation analysis with principal component analysis (PCA) index

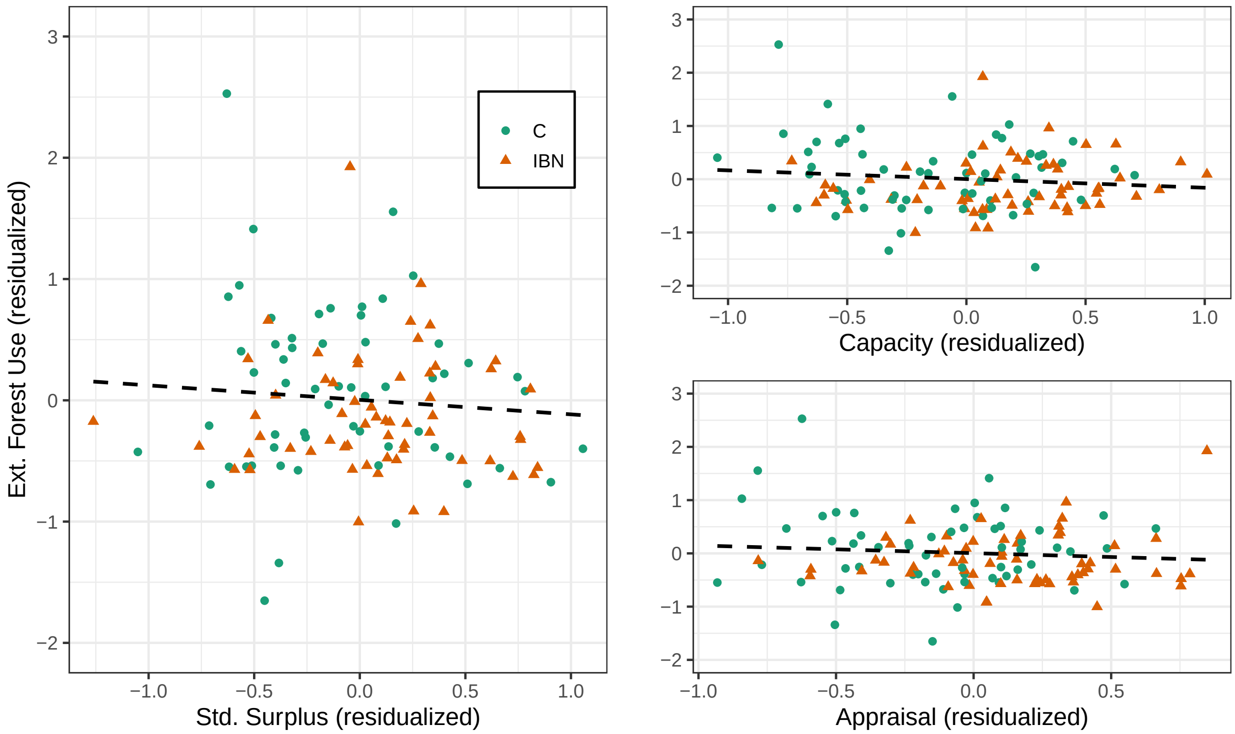
We also compute the indices for capacity and appraisal by using principal component analysis (PCA). Using PCA, the first component loads more on variables we related to appraisal; while the second component loads on variables we related to capacity: cor(PC_1, appraisal) = 0.999 and cor(PC_2, capacity) = 0.669.

Table A.15: Mediation Analysis using Principal Components

Panel A: First-Stage Estimates			
	Appraisal	**Capacity**	**Surplus**
Treatment	0.254	0.268	0.043
	(0.069)	(0.104)	(0.076)
Capacity			0.289
			(0.23)
Appraisal			0.19
			(0.142)
Panel B: Decomposition of the Total Effect of IBN on Std. Surplus			
Total	0.169	**Indirect: Capacity**	0.078
	(0.085)		(0.073)
Direct	0.043	**Indirect: Appraisal**	0.048
	(0.076)		(0.033)

Table A.15: Mediation analysis estimates with PCA indices. Panel A presents the first stage estimates, while Panel B presents the decomposition of the total effect of the IBN training on the surplus. Bootstrapped standard errors in parentheses are clustered at the community level.

E.4 Mediation of Forest Use



F. Structural Model

F.1 Change in the probability of agreement due to capacity (k) and appraisal (δ_1)

Table A.16: Structural estimates

Age	Education	Gender	N	Avg. Surplus	$\widehat{k}$	$\widehat{\delta_1}$	$\Pr(A)\Big\|_{k=0}^{k_1}$	$\Pr(A)\Big\|_{-\delta_0}^{-\delta_0+\delta_1}$
All	All	All	705	2.7	3.49	-0.11	0.04	0.04
$\geq$ Median	$>$ Primary	Male	50	8.5	10.01	-0.03	0.07	0.00
$\geq$ Median	$\leq$ Primary	Female	150	3.5	3.87	-0.19	0.02	0.04
$<$ Median	$>$ Primary	Male	130	3.4	1.56	-0.02	0.03	0.01
$<$ Median	$\leq$ Primary	Male	127	2.0	3.07	0.01	0.00	0.00
$\geq$ Median	$\leq$ Primary	Male	150	1.8	4.01	-0.38	0.05	0.15
$<$ Median	$\leq$ Primary	Female	83	1.4	2.28	-0.09	0.04	0.03

Table A.16: Structural estimates for the full sample (row 1) and sub-groups defined by age (above and below median), education (above and below primary education), and gender. $\widehat{k}$ is our estimate of the IBN training's effect on negotiation capacity; $\widehat{\delta_1}$, our estimate of IBN training's effect on appraisal of the outside option. Sub-groups with less than 40 participants are not reported for power considerations. $\Pr(A)|_{k=0}^{k_1}$ calculates the change in the probability of agreeing that is attributable to the estimated effect on capacity. $\Pr(A)|_{-\delta_0}^{-\delta_0+\delta_1}$ calculates the change in the probability of agreeing that is attributable to the estimated effect on appraisal.

F.2 Estimation of Lee Bounds for Negotiation Capacity

Lee (2009) bounds:

- Assume that treatment increases the rate of agreement (monotonicity)
- Estimate effect of treatment on the probability of agreement, q
- Remove share q from top and bottom of treatment group distribution and re-estimate

Intuition: suppose the share who agree due to treatment have the best and worst observed outcomes, and then remove these observations to construct bounds

Table A.17: Lee bounds for structural estimates

Age^{+}	Edu^{+}	Fem	N	Lower Bound	$\widehat{k'}$	Upper Bound
All	All	All	1,070	0.62	2.43	5.88
✓	✓	✗	65	18.49	20.11	30.39
✓	✗	✓	240	1.16	4.48	6.74
✗	✓	✗	210	4.94	6.05	7.19
✗	✗	✗	198	0.62	2.43	5.88
✓	✗	✗	195	-6.41	0.00	9.02
✗	✗	✓	128	-4.60	0.05	6.02

Table : Age^{+}: Above Median Age (52); Edu^{+}: Above Primary Education.

F.3 Involvement in Forest Governance across Sub-groups

Table A.18: Participation in and influence over decisions about community forest use in the control group

Age$^+$	Edu$^+$	Fem	Town Chief	Landlord or Elder	1(Member CF)	Number meetings CF	Count of Neighbors Consulted about CF in Last Week	1(Property rights for land)
✓	✓	✗	0.08	0.32	0.11	0.58	2.91	0.74
✓	✗	✓	0.00	0.04	0.06	0.24	0.39	0.51
✗	✓	✗	0.08	0.17	0.06	1.03	4.58	0.63
✗	✗	✗	0.19	0.12	0.10	0.29	0.52	0.70
✓	✗	✗	0.17	0.46	0.06	0.43	1.47	0.74
✗	✗	✓	0.03	0.00	0.06	0.28	0.24	0.62

Table A.18: Descriptive statistics on participation in/influence over decisions about community forest (CF) use in the control group. Age$^+$ stands for above Median Age (52). Edu$^+$ stands for above Primary Education. 1(Member CF) is a dummy equal to 1 if the respondent or somebody in respondent household is member of the community forest (CF) committee. 1(property rights for land) is a dummy equal to 1 if the respondent reports owning the land with property rights.

G. Analysis Plan

G.1 Deviation from the PAP

In this section, we report the deviation from the PAP (Christensen et al. 2021). We do not test for attrition as we collected only endline data. Instead, we regress treatment on pre-specified covariates and enumerators fixed effects. We reject the joint test that the pre-specified covariates predict the treatment status. The F-statistics of the joint test is 0.194 with a p-value of 0.999.

G.2 Exploratory analysis

In this section, we list all the exploratory analysis that we carry out in the paper:

- Construction of the appraisal and capacity index
- Mediation analysis
- Structural estimation

G.3 Variable Definitions

Variables constructed from Household Survey (SVY), Environmental Assessment (EA), Negotiation Simulations (SIM).

Table A.19: Variable Definitions

Measure	Source	Definition
Manipulation Checks		
Attended Negotiation Training	SVY	Attended Negotiation Training
Correctly Reports Length of Training	SVY	Correctly Reports Length of Training
Correctly Reports Location of Training	SVY	Correctly Reports Location of Training
Knowledge of Negotiation Skills (H1)		
Correctly defines IBN	SVY	Correctly defines IBN
Knowledge of IBN interests	SVY	Correctly distinguishes interest vs. position
IBN concept recall index	SVY	Count of IBN concepts recalled (0-3)
Recognizes Potential for Win-Win	SVY	Recognizes Potential for Win-Win Agreement
Knowledge of Inter-personal Skills (H2)		
Acknowledges Importance of Positive Relationship	SVY	Acknowledges Importance of Positive Relationship
Count of Tactics Listed to Build a Positive Relationship	SVY	Count of Tactics Listed to Build a Positive Relationship (0-6)
Deployment of IBN Skills (H3)		
Count of questions asked about Buyer	SIM	Count of Questions asked about Buyer (0-2)
IBN skills index	SIM	Count of IBN Skills Used in Peanut-Farmer Simulation (0-5)
IBN solutions index	SIM	Count of Solutions Discovered in Woodbuyer Simulation
Deployment of Inter-personal Skills (H4)		
Respondent Does Not Display Anger or Frustration	SIM	Respondent Does Not Display Anger or Frustration the three simulations
Surplus and Agreement (H5-H7)		
Total surplus	SIM	Surplus achieved
Positive surplus	SIM	Indicator for surplus greater than zero
Agreement	SIM	Agreement reached
Negotiated price	SIM	Highest price negotiated during simulation, regardless of whether an agreement is reached
Real World Outcomes		
Rule in Community against Logging w/o Permission	SVY	Respondent reports rules against deforestation in their community
Count of Neighbors Consulted about Forest in Last Week	SVY	Number of people respondent discusses the community forest with in Last Week
Number of meetings community forest (CF)	SVY	Number of community forest (CF) meetings attended since the President took office
Does Not Want to Reduce Logging Activity	SVY	Does Not Want to Reduce Logging Activity
Price Demanded to Clear Forest (logged)	SVY	Logged price required to clear-cut the community forest
External Forest Use	EA	Count of external forest-use activities (concessions, mining, pitsawing) in the community forest (detected)
External Logging or Investment	SVY	Index of external forest-use activities (concessions, mining, pitsawing) in the community forest over the previous 3 months (self-reported)
Benefits from External Forest Use	SVY	Index of benefits received from external forest-use (money, building materials, roads or bridges, other tokens, other services)
Overall satisfaction	SVY	Self-reported level of satisfaction with rules and decisions made by the leaders of this community
Satisfaction related to the community forest	SVY	Self-reported level of satisfaction with rules and decisions made about the community forest

Data Sources: Household Survey (SVY), Environmental Assessment (EA), Negotiation Simulations (SIM).

Appendix References

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