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

Darin Christensen †
Alexandra Hartman ‡
Cyrus Samii §
Alessandro Toppeta ¶

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Abstract

We experimentally evaluate 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 it raises the total surplus earned by $2.74 USD, which is a 42% increase. Our exploration of mechanisms indicates that the training increases trainees' capacity to identify valuable deals, but does not improve their appraisal of their outside option. We find a reduction (0.27 standard deviations) in the exploitation of communal forestland in treated communities.

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† Associate Professor of Public Policy and Political Science, UCLA. e: darinc@luskin.ucla.edu.

‡ Associate Professor of Political Science and Public Policy, UCL. e: alexandra.hartman@ucl.ac.uk.

§ Associate Professor of Politics, NYU. e: cds2083@nyu.edu.

¶ Assistant Professor of Labor Economics, SOFI, Stockholm University. e: alessandro.toppeta@sofi.su.se.

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 uses a randomized controlled trial (RCT) to evaluate whether interest-based negotiation (IBN) training changes the deals community leaders strike. A small body of work in behavioral economics studies 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 a bargaining impasse. Agribusiness and timber deals are getting done, but some of these agreements leave communities worse off. In negotiation simulations designed to mimic deals that communities could make, leaders in rural Liberia frequently agree to deals even when they would have been better off walking away: in our control group, nearly half (47%) never reach a deal worth more than their outside option; over one-quarter (27%) agree to deals 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

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 (see Appendix FigureA.2).

as zero-sum interactions, 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 (Murray 2011), which tries to correct common negotiation mistakes. IBN training stresses that parties should focus on their interests (and 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 (i.e., their best alternative to a negotiated agreement or BATNA), so that they do not agree to a deal that leaves them worse off than simply walking away.

We study 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 negotiation 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] Using our lab-in-the-field measures — three original incentivized negotiation simulations around potential land and logging deals — we find that trained leaders are 27% more likely to reach a beneficial agreement, and it raises the total surplus earned by $2.74 USD, which is a 42% increase.

We go beyond our pre-analysis plan and use both a mediation analysis and a structural model to explore whether trainees' success is attributable to two mechanisms. First, IBN may increase trainees *capacity* to find a wider range of possible deals. Second, 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 intervention increased trainees' capacity to identify more valuable deals. For the mediation analysis, we construct indexes of intermediate outcomes that capture a respondent's ability to recall concepts related to these two mechanisms. While the training increased knowledge along both dimensions by around 0.3 standard deviations, only the first dimension — knowledge related to identifying possible deals — appears to mediate the effect of the treatment. While trainees may learn what a BATNA is, they do not appear to apply this knowledge. These findings align with our structural model, which imposes a decision-theoretic framework to derive estimates of capacity and appraisal

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.

just from respondents' negotiation outcomes without relying on mediators constructed from survey outcomes. Our parameter estimates imply that the training improved trainees' capacity but did not improve trainees' assessment of their outside options. The structural results reinforce the mediation analysis, ruling out measurement error as an alternative statistical explanation for why we do not find a relationship between one of the mediators and negotiation success.

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 decision-makers in treatment communities demanding more of outside investors who want to exploit communal forestland for agriculture or logging, resulting in fewer deals, but ones that are higher-value.

We contribute to the literature in three ways. Relative to the ubiquity of negotiation courses, efforts to improve these skills have rarely been evaluated; past studies more often focus on how different types of people approach negotiations (Boothby, Cooney, and Schweitzer 2023; Recalde and Vesterlund 2023). We first expand the small existing literature on the effects of IBN and show that an IBN is effective at 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 future school attendance by eight to ten percent.[4] 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 mistakes made by untrained negotiators: first, haggling over a single dimension, they do not consider all potential agreements; and second, 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 correcting the first mistake, increasing trainees' capacity to identify valuable potential agreements. 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, while we see noisy improvements among important subgroups (e.g., chiefs), we do not find that the IBN training is corrective for most trainees.

4. 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.

The IBN training emphasizes that win-win agreements often exist; it needs to better convey 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 training show mixed results (for a recent meta-analysis, see McKenzie et al. 2020). Some training emphasizes 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 (McKenzie et al. 2020). 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.

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 beneficial agreements that advance their interests. 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, over one-quarter of untrained community leaders reach agreements in simulations that, on average, pay them less than the stated value of their outside option. These leaders 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 coaches 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 affects negotiation outcomes, we study an intensive 12-hour IBN training provided by a Liberian NGO, Parley Liberia. IBN is the most prominent approach to teaching negotiation, and our curriculum is adapted from courses taught in business, law, and policy schools.[5] Our training consists of three modules: (1) preparing to negotiate, particularly identifying one's interests and BATNA; (2) identifying potential agreements and evaluating 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. We did not attempt to un-bundle these modules; we also acknowledge that we studied a particular training, and there may be other effective approaches to teaching negotiation. 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: first, it enlarges the set of deals they consider; and second, 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

5. 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).

model that features both mechanisms. Let $\theta_i(D_i) \in \mathbb{R}_1^+$ 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 $\beta + u_i(D_i)$, where $\beta > 0$ and $u_i(D_i) \sim F_D(\cdot)$. $u_i(D_i)$ captures biases in how individuals construe their outside option, which could be positive or negative. 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, $u_i(0) < 0$.

An individual will reach an agreement only if $\theta_i(D_i) \geq \beta + u_i(D_i)$ and will otherwise walk away. IBN could enhance individuals' capacity to reach better deals ($\theta_i(1) > \theta_i(0)$), which should increase rates of agreement and surplus. It could also improve individuals' ability to appraise their BATNA ($u_i(1) > u_i(0)$), raising their threshold for agreeing to a deal. IBN does not supply information about a particular outside option (e.g., land values), which could also shift $u_i(1)$; rather, it encourages trainees to reckon their BATNA prior to negotiating and to walk away from any deal inferior to this outside option. This should reduce the rate of agreement (conditional on $\theta_i(D_i)$) 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 most felling 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's 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.

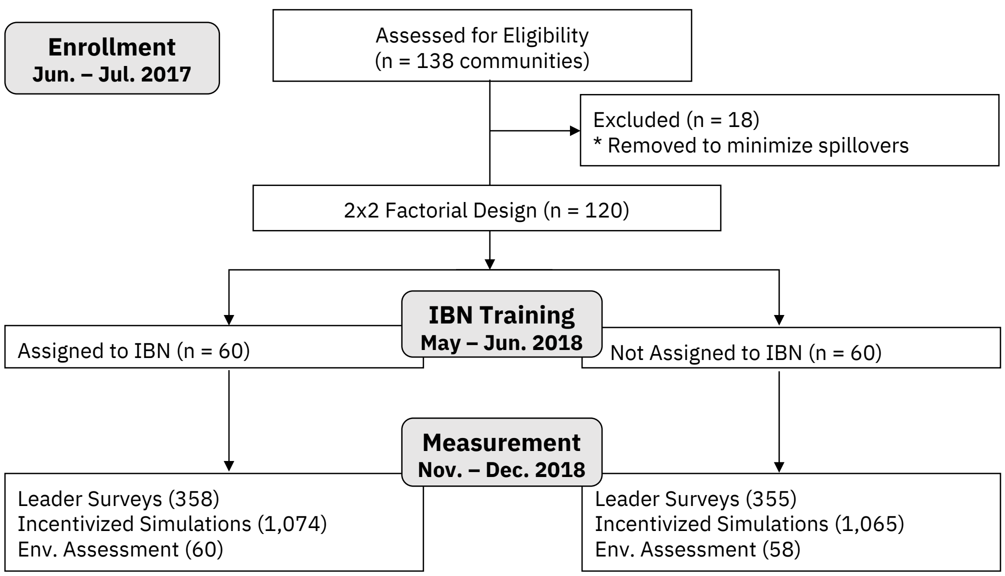
3.2 Study Design

Our study design is summarized in Figure 1.[6] We first identified 138 eligible communities in Bong County (see Appendix Section A). To be eligible, a community needed to have communal

6. 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.

forestland, and its leadership needed to express interest in participating and give informed consent. We selected 120 of these communities for the study to minimize the potential for cross-community spillovers (Christensen et al. 2021). We randomly assign 60 of these communities to the IBN training; the remaining 60 received no negotiation training of any kind.[7] 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.2.

Figure 1: Study Design



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.[8] 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).

7. The experiment is part of a larger study that also studied the impact of cross-randomized intervention on citizen monitoring of communal forests (Christensen, Hartman, and Samii 2021a). Our analysis focuses on 60 communities randomly assigned to negotiation and 60 communities that serve as the control group.

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

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.[9] 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. Instructions to respondents encouraged them to negotiate the best possible deal, and respondents received prizes of soap and cooking spices for concluding deals that paid them more than the disagreement payoffs specified in the simulation scripts. Beyond withholding prizes, we do not penalize respondents who reach agreements that pay them less than the disagreement payoff specified in the simulation script.[10] Respondents appear to invest effort in the simulations: the average simulation lasted three and half minutes in control; the training increased this average duration by just ten seconds.[11]

We wrote these simulations to resemble real-world interactions in our study area. Community leaders most often negotiate with small crews engaged in pitsawing or mining, and they bargain with these external actors about what cash or in-kind payments the community will receive in exchange for permission to enter or fell trees on communal forestland. The IBN training did not include any incentivized simulations; trainees were not more familiar with the format or incentive scheme than their counterparts in control. In each simulation, the respondent controls a natural 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

9. 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.

10. In consultation with our local partner, we decided that penalties (i.e., withdrawing payments) could generate anger among respondents. Our respondents are poor and might resent the (better-resourced) enumerators or NGO for withdrawing highly valued benefits over the outcome of a game.

11. Across both control and treatment, respondents who eventually "walk away" and refuse to make a deal spend longer negotiating than those who reach agreements.

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.

Fourth, 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.

Finally, our simulations are designed to capture as closely as possible the kinds of negotiations that communities undertake. The pitsawers who, at the time of the study, represented the largest source of deforestation in Liberia are local organizations, and as a result, investors included in the simulations have a similar profile. As part of the study, we collected descriptive data on who pitsawers negotiated with and identified that they did negotiate with the types of community members we trained and that community members could meaningfully negotiate deals.

We group variables related to the same hypothesis and construct mean-effects indexes as in 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 (Lin 2013) to estimate the sample average treatment effect (ATE) for outcome Y_{sibc} for negotiation simulation s for individual i in block b and community c.[12] Equation (1) includes covariates for the cross-randomized community monitoring treatment (CM); district fixed effects, which encompass our blocking strata; simulation fixed effects (when appropriate); enumerator fixed effects; and a set of respondent characteristics (age, gender, education, leadership position, simulation order).[13] We cluster our standard errors on community, which is the unit of randomization. Additional details on the blocking and randomization procedure can be found in Appendix Section C.2.

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

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.5). This analysis on the intensive margin relies on stronger assumptions: if treatment changes the types of people who reach agreements, this could confound our conditional-on-positives estimate. Fortunately, the characteristics we can observe (age, gender, education, position) appear to be balanced

12. The centered-interaction specification de-means the covariates (as indicated by the ˜ operator in Equation (1)) and interacts each with treatment. Lin (2013) shows that this specification improves precision in estimating the ATE.

13. For reasons of statistical power, we estimate an ATE for the IBN treatment that marginalizes over the sample's distribution of the cross-randomized CM treatment (as well as the other covariates). As such our quantity of interest is a sample ATE in a setting where the CM treatment is present as a background nuisance factor. In Appendix Section C.6, we fit an alternative specification suggested by Muralidharan, Romero, and Wüthrich (2023).

even among respondents who reach deals.[14] Moreover, our specification includes pre-specified covariates and also interacts these characteristics with treatment to limit possible confounding.

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.9). We report intent-to-treat estimates; these are only slightly attenuated relative to treatment-on-the-treated estimates. Appendix Table A.7 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.[15] Aggregating these knowledge questions into a mean-effects index, we find an increase of 0.34 standard deviations (see Table 1). This indicates that trainees could recall key concepts several months later.

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

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
Effect of Agreement on Surplus				
Effect of IBN on Agreement*	0.072	(0.03)	0.02	2115
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. †: mean-effects index. ⋆: the effect of IBN on the probability of agreement is theoretically ambiguous, so we did not pre-specify a directional hypothesis.

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

14. While training increases the likelihood of reaching an agreement by 7.2 percentage points (see Table 1), that effect does not vary significantly by age, gender, education, or position. Using an omnibus test, respondent characteristics do not predict treatment assignment even in the subset who reach agreements.

15. Appendix Table A.9 includes treatment effects on the sub-components of all mean-effects indexes. Appendix Table A.10 reproduces Appendix Table A.9 without covariate adjustments; treatment effects are essentially unaffected.

a bundle of relevant 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. They are also significantly more likely to find different solutions to a simulation. The latter result foreshadows our exploration of mechanisms, in which we find that the training increased individuals' capacity to discover mutually beneficial deals.

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 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 leaders who did not attend the training, 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. Appendix Figure A.2 shows high rates of agreement in control even when the negotiated deal pays less than the BATNA stated in the simulation script. 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. We do not find that the training has a different effect on the total surplus or rate of agreement when we restrict attention to the final simulation played by each respondent. Trainees do not appear to wait until the final simulation to bargain more aggressively, e.g., in hopes of maintaining a positive relationship by being agreeable in earlier rounds.

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. As noted above, we find balance on observables even after restricting attention to the respondents that reach agreements. In the simulations that end in deals, we find that trainees earn \$4.85 more, a 37% increase (H7 in Table 1).

5. Mechanisms

Our conceptual framework features two mechanisms: training in IBN could (1) increase trainees' capacity to identify valuable, mutually agreeable deals and (2) improve their ability to appraise their outside options, reducing the likelihood of agreeing to deals worth less than their BATNA.

We start by estimating the effect of the training on individuals' knowledge — whether they recall concepts related to these mechanisms. We create two new indexes: (1) knowledge of possible deals and (2) knowledge of outside option (Appendix E.1). In our surveys, we ask, for example, what should be considered before walking away from a negotiation. Individuals might respond that they should consider deals that could advance the other party's interests (i.e., they know more than one deal is possible). This response would contribute to their value on our first index. They may also consider their own "bottom line" (i.e., they know that a proposed deal may not best their outside option); such a response contributes to their value on our second index. While motivated by theory, these indexes were not pre-specified. As a check, we take all of the items in these indexes and estimate the first two principal components. We find that the second component is highly correlated with knowledge of possible deals ($r = 0.67$) ; the first component, with knowledge of outside option ($r = 0.99$). In Appendix Table A.19, we estimate a larger indirect effect for PC_2, which is consistent with our finding below that knowledge gains related to identifying possible deals account for more of the total, reduced-form effect.

We find, first, that the IBN training had a positive effect on both knowledge indexes. At the top of Table 2, we report that both improved by roughly 0.3 standard deviations. These knowledge gains are also apparent in Figure 2, which plots the average index value and average surplus in each community (after residualizing all variables using pre-specified covariates). In the left and right panels, communities whose leaders attended the training (triangles) tend to fall to the east of control communities (circles), suggesting the IBN training improved knowledge of both concepts. The (reduced-form) effect of the training on surplus reported in Table 1 is also reflected in treatment communities tending to fall north of control communities in these plots.

We find, however, that only the first index, knowledge of possible deals, is associated with higher surpluses in the negotiation simulations. The slopes of dashed lines in Figure 2 reflect the partial correlations (at the community-level) between our knowledge indexes and negotiated surplus.[16]

16. Appendix Figure A.4 shows the partial correlation between the knowledge indexes and surplus for treatment and control communities. We cannot reject the null hypotheses these slopes are the same across groups.

Figure 2: Relationship between Mediators and Surplus at Community-level

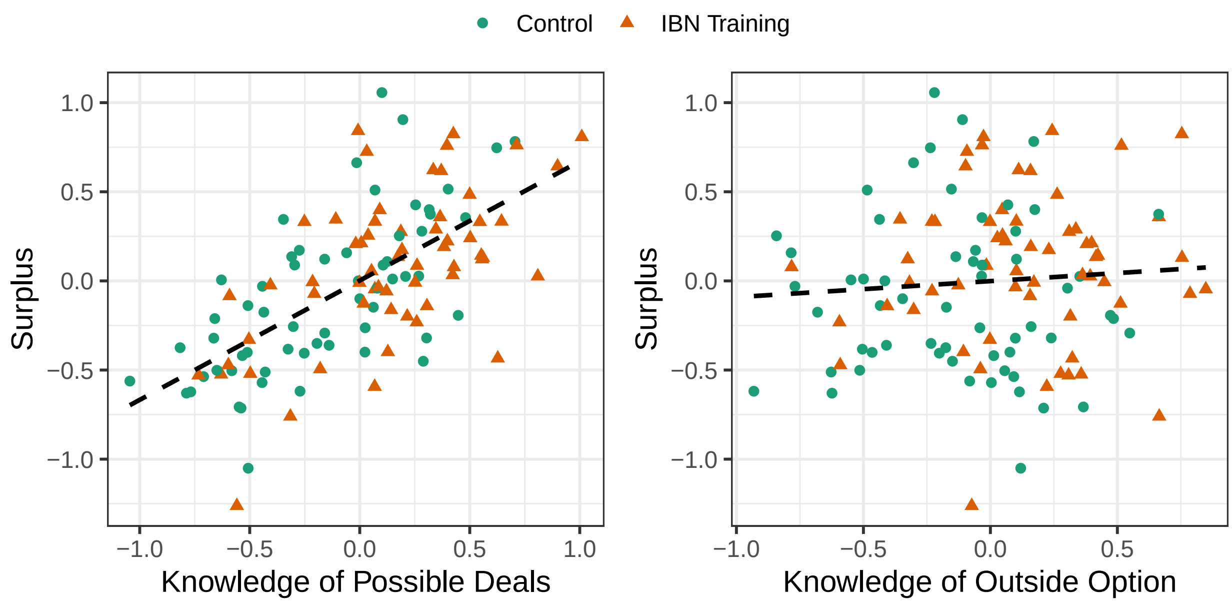


Figure 2 presents a graphical representation of the mediation analysis. The figures are constructed in three steps. First, we residualize the standardized surplus (i.e., a mean effects index of the total surplus achieved in the three simulations) and knowledge indexes with the pre-specified covariates. 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.

Where leaders score higher on our second knowledge index, communities do not achieve systematically larger surpluses. We conduct a mediation analysis using our individual-level data, decomposing the total effect of the IBN training (0.16) into the indirect effects of these knowledge indexes and a direct effect (see Table 2). Knowledge of possible deals generates a large indirect effect (0.15) that represents roughly 90% of the total effect; knowledge of outside options generates an indirect effect that is many times smaller (0.02).

To perform this decomposition, we estimate three regressions. Our RCT supports a causal interpretation of the first two: the reduced-form effect of the treatment on the outcome and then on the mediators. The third involves regressing the outcome on both mediators and the treatment.[17] To interpret our indirect-effect estimates as causal, we must assume that there are no unmeasured covariates that confound the relationship between our knowledge indexes and total surplus after conditioning on treatment status and pre-treatment covariates (Imai et al. 2011). This assumption would be violated if, for example, more inquisitive individuals learn more from the training and can

17. All three regressions also include pre-specified covariates, which include age, gender, education, position, and several design features.

more effectively negotiate. Given the strength of this assumption, these results should be regarded more cautiously.

Table 2: Mediation Analysis and Structural Estimates

Mediation Analysis	Effect of IBN on Knowledge Indexes		
	Possible Deals	Outside Option	
	0.31	0.25	
	(0.07)	(0.06)	
	Indirect Effects of Knowledge Index on Surplus		Direct Effect
	0.15	0.02	-0.01
	(0.04)	(0.01)	(0.07)
Structural Estimates	Effect of IBN on Model Parameters		
	Capacity ($\widehat{k}$)	Appraisal ($\widehat{\delta_1}$)	
	3.49	-0.11	
	(1.77)	(0.08)	

Table 2 presents the mediation analysis and the structural estimates. The top panel presents the mediation analysis with the "first-stage" estimates (i.e., the ATE on the mediators) and then the indirect effects of the mediators. The bottom panel presents the structural estimates for the full sample. $\widehat{k}$ is our estimate of the IBN training's effect on capacity; $\widehat{\delta_1}$, our estimate of the IBN training's effect on appraisal of the outside option. Standard errors in parentheses are clustered at the community level.

The IBN training increases knowledge on both dimensions, yet increasing participants' knowledge about the outside option does not seem to improve their negotiation outcomes. One statistical explanation is that our measure of knowledge is noisy, and this measurement error attenuates the estimated relationship between knowledge of outside options and surplus. Another plausible explanation is that individuals gained knowledge but struggled to apply it when negotiating. They could, for example, define a BATNA but could not operationalize it while negotiating.

To adjudicate between these, we adopt a more structural approach. We specify a decision-theoretic model and then use the outcomes of our negotiation simulations — what deals people negotiated and whether they agreed — to estimate the training's effect on participants' *capacity* to identify valuable deals and how they *appraise* their outside options. This approach does not require us to measure knowledge and, thus, avoids attenuation bias due to potential measurement error in our mediators.

To fully specify our model, we add a few assumptions to our conceptual framework. We represent an individual's negotiation capacity as $\theta_i(D_i) = \theta_i + D_i \cdot k$, where $k \in \mathbb{R}^1$ is the extent to which the IBN training adds or detracts from their capacity. Individuals idiosyncratically value their outside option at $\beta + u_i(D_i)$, where $u_i(D_i)$ captures how their beliefs depart from the objective value of the option, β. We assume that $u_i(D_i) \sim \mathcal{N}(-\delta_0 + D_i\delta_1,\ \sigma^2)$. If $\delta_1 > 0$, then trainees set a higher threshold for making a deal; their decisions imply a more generous appraisal of their outside option.

We estimate k by comparing the value of the most attractive deal negotiated by trainees versus leaders from control villages using Equation (1). We set the rules of the simulations, so we know what value an individual could have earned in each simulation given their tactics, regardless of whether they agree to a deal. We find a positive and statistically significant increase in capacity, which we report at the bottom of Table 2.[18]

Our decision-model implies that an individual will only agree to a deal if its value exceeds their outside option. Agreeing to a deal can, thus, be expressed using a latent-index model, where $\text{Agree}_i = \mathbb{1}\{\theta_i(D_i) - \beta \geq \sigma u_i - \delta_0 + \delta_1 D_i\}$, where u_i is distributed standard normal, and β is the stated value of the outside option in the simulation script. We estimate δ_1 using a probit model, in which we regress whether agreement has been reached on an indicator for treatment (D_i) and the negotiated value $\theta_i(D_i)$. More intuitively, $\widehat{\delta_1}$ will be positive if, when facing a deals of equivalent value, trainees are more likely to take their outside option and walk away.

At the bottom of Table 2, we find that the IBN training had a negative but statistically insignificant effect on trainee's appraisal of their outside option. Conditional on the deal negotiated, trainees were not more inclined to walk away, which implies that they do not place more value on their outside option relative to control. (If anything, the point estimates indicates trainees were more eager to make a deal.) These structural estimates reinforce our conclusion from the mediation analysis: the IBN training improves individuals' negotiation capacity but does not meaningfully improve their appraisal of their outside option.

Our null finding on appraisal cannot be blamed on noisy measures of respondents' knowledge, as these indexes do not enter the structural estimation. Trainees earn a larger surplus because they can envision more valuable deals, not because they are choosier about which deals they agreed to.

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 0.27 standard deviations ($p = 0.052$) in forest use by external actors (primarily, pit-sawers) in treatment communities. The index of forest use by external actors includes the count of external forest-use activities

18. We construct Lee bounds for capacity ($\widehat{k}$) following the procedure outlined in Appendix Section F.1. We find a lower bound of 0.62 and and upper bound of 5.88.

(concessions, mining, pitsawing) detected in the environmental assessment and self-reported in the survey over the previous 3 months.[19]

Respondents report reduced forest use by external actors. We do not believe demand effects contaminate these self-reports: large majorities of respondents in treatment and control communities prefer continued or intensified external forest use. If anything, IBN trainees express greater support for clearing communal forestland. Importantly, independent environmental assessments (EAs) also uncover less activity related to agriculture, logging, or mining on communal forestland. Enumerators conducting the EAs (which was limited to three hours) were provided with a simple map of the community forest drawn by a key informant the day before the EA took place and used a mobile survey to record and geo-locate forest use activities (see Appendix Section B.3). The EAs also help convey the magnitudes of these effects. In control communities, the EAs uncovered, on average, one site with either pitsawing, mining, or commercial agriculture, which we classify as external forest uses (mean = 1.04, sd = 1.52). In Appendix Table A.13, we find that the IBN training reduces this count by -0.46 or 30% of the control standard deviation — qualitatively similar to the effect we report on the overall index in Table 3. In Appendix Section D.4.1 we report suggestive evidence that these reductions are larger in communities that experienced higher rates of forest loss in the 18 months prior to the intervention. In communities with active pitsawing, respondents report that crews fell and mill 22 large trees on average. A back-of-the-envelope calculation implies that the IBN training preserved roughly 600 trees in the six months following the intervention.[20] Additionally, Appendix Table A.17 presents effects on remotely-sensed deforestation, where we find a statistically insignificant reduction.[21]

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 setting a higher bar for the agreements they reach with outside investors. In Appendix Section F.2, we present suggestive evidence that, among the subgroups most involved in decisions related to their community forests — namely, village chiefs and educated youth leaders (who often negotiate labor arrangements for logging or mining activities) — the IBN training improves their appraisal of

19. Appendix Table A.8 provides control-group levels for the real-world outcomes in Table 3. Appendix Table A.13 shows effects on the sub-components of the mean-effects indexes.

20. In qualitative work conducted at timber markets in our study area, we were told that pitsawers typically recover 80-120 planks per tree, and these planks are sold between $5-7 USD. Using the midpoints of these ranges, the IBN training conserved roughly $6,000 USD per community.

21. In addition to any prediction error, our remotely-sensed measures contain noise due to the infeasibility of demarcating the boundaries of communities or their forestland. We remotely sense clearing activity in circular buffers that encompass each town but only crudely approximate the forestland under the leaders' control. Accounting for the two additional tests reported in Appendix Table A.17 per Romano and Wolf (2005), the adjusted p value on the "Forest Use by External Actors" index increases to 0.06. One could instead apply a more conservative Bonferroni-style adjustment and double the p value reported in Table 3 to account for using an additional, remotely sensed measure of forest use.

their outside option. While the standard errors are large in this under-powered subgroup analysis, the point estimates suggest that these types of trainees, conditional on the deal they negotiate, are more inclined to walk away and take their outside option (see Appendix Table A.20). The negative and insignificant estimate of δ_1 in Table 2 is driven instead by the majority of our trainees who, based on their position, education, and gender (e.g., women's leaders, midwives, teachers), tend to be less engaged and influential and, thus, may not influence real-world forest use. This offers a way to reconcile our lab-in-the-field and real-world results: among leaders with sway over the community forest, the IBN training made them more discerning, which could have led to the reduction in external forest use reported in Table 3. Although the effect is not statistically significant, we also find that trainees would demand a higher average price for clear-cutting their communal forestland, suggesting that their appraisal of the forest has increased. Additionally, 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.

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

Outcome	ATE	Std. Error	*p*-value	N
Forest Use by External Actors†	-0.265	(0.135)	0.052	705
Benefits from External Forest Use†	0.054	(0.136)	0.691	705
Engagement around Forest Use				
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
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

Table 3: Average treatment effect estimates on real-world outcomes using Equation 1. Standard errors in parentheses are clustered at the community level. † stands for mean-effects index.

We can also rule out two alternative explanations. First, we do not find that trainees prefer less logging: while the effect is insignificant ($p = 0.14$), trainees are 3.1 percentage points more likely to favor sustained or increased logging on communal forestland. This also suggests that demand effects do not account for the reductions in self-reported forest use: trainees are not bashful about expressing their desire for greater external exploitation of communal forestland. Second, we do not uncover evidence of spatial spillovers — namely, control communities that lie closest to treatment communities do not see increased forest use by external actors (see Appendix Table A.16).[22]

22. These null findings with respect to spatial spillovers should be regarded cautiously for two reasons. First, we sampled villages to maximize the distance between communities and limit spillovers between proximate units assigned to different arms. The majority of control communities are located more than five kilometers away from the nearest village that received the IBN training. If spillovers only occur between neighboring villages, our sampling strategy

Finally, one might worry that trainees' gain is to the detriment of other households in their communities. In Appendix Table A.15, 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.

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 suggest that the training made people 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.

limits our ability to detect such displacement. Second, we conducted our endline surveys and environmental assessments roughly six months after the IBN training. If pitsawing crews require more time to relocate their operations, then we may have missed spillovers that occurred after we collected data.

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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 study.

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 simulations, 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). .

After each simulation, the enumerator was asked "how confident are you that the respondent understood the script?" In over 99.5% of simulations (all but 5 of 2,115), the enumerator indicated that they are "confident, they [the respondent] clearly followed."

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 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.1. 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.1: Treatment Assignment

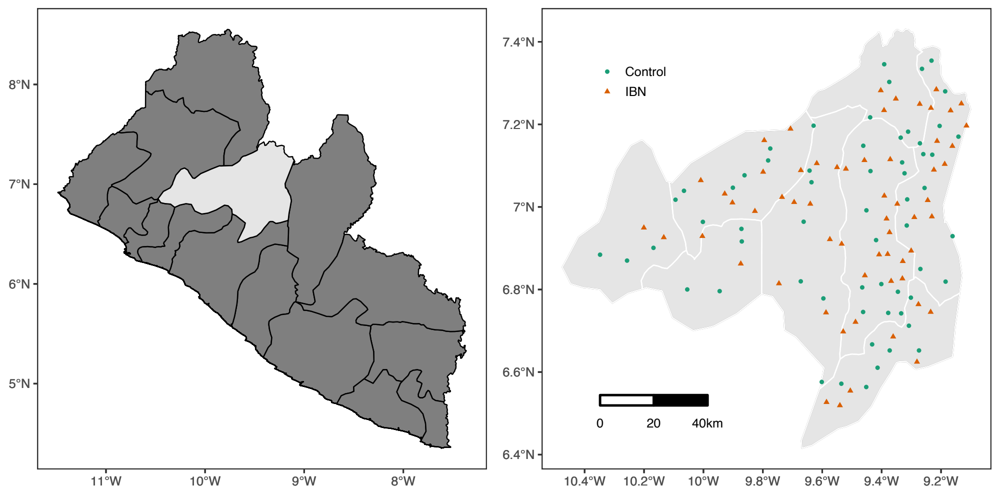


Figure A.1: Treatment assignment for the 120 communities in the evaluation sample. Communities were organized into blocks and then randomized into one of four groups: (1) Control and (2) IBN training.

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.3 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.4 Primary 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.[23]

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 negotiation training, averaging over communities 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 coefficients 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.5 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 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:

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

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

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

C.6 Interacted-factorial Specification

In our sample, we also had an orthogonal, cross-randomization for a community monitoring (CM) treatment that was the basis of a separate study. The CM treatment is not the subject of the current paper; we study the CM treatment in Christensen, Hartman, and Samii (2021). The CM treatment operates as a nuisance factor for the current paper. For reasons of statistical power, we choose to estimate an average treatment effect for the IBN treatment that marginalizes over the sample's distribution of the CM treatment (as well as the other covariates). As such our quantity of interest is a sample average treatment effect in a setting where the CM treatment is present as a background nuisance factor. Following Lin (2013), our centered-interaction specification ensures that the coefficient on the negotiation treatment variable captures this quantity of interest. (See also Goldsmith-Pinkham, Hull, and Kolesár (Forthcoming) for a similar recommendation to use an appropriately centered interaction specification.) We recognize that our choice to target an effect that marginalizes over the CM treatment means that our effect is defined against something other than a "no intervention status quo" (as discussed in Muralidharan, Romero, and Wüthrich 2023). We think this is justified because our specification and target effect allow us to use the full sample, meaning more statistical power.

Table A.6: Interacted Factorial Specification for Selected Behavioral Outcomes

	Positive Surplus	Total Surplus	Forest Use by External Actors	Benefits from External Forest Use
IBN	0.0561*	3.104*	-0.1069	0.2491
	(0.0296)	(1.776)	(0.1380)	(0.1626)
Community Monitoring (CM)	-0.0068	0.2060	0.3320	0.3541**
	(0.0309)	(1.975)	(0.2520)	(0.1785)
IBN $\times$ CM	0.0084	-0.7463	-0.3230	-0.3927
	(0.0436)	(2.815)	(0.2740)	(0.2652)
N	2,115	2,115	705	705

Table A.6: Treatment effects for selected simulation-based (columns 1-2) and real-world (columns 3-4) outcomes using an interacted factorial specification that does not mean-center the cross-randomized community monitoring (CM) treatment. Standard errors clustered at the community-level. Statistical significance: **: 0.05, *: 0.1.

Nonetheless, for completeness, we also fit the interacted factorial specification suggested by Muralidharan, Romero, and Wüthrich (2023), which includes the indicators for the IBN treatment, the CM treatment, and their interaction, without the CM indicator centered. This allows us to read the coefficient on the IBN treatment variable as an estimate of its effect absent the CM intervention (see Appendix Table A.6). For our preferred specification to be justified, we would want to see that the coefficient is qualitatively similar to the estimate with the centered CM interaction, but with a larger standard error. We focus attention on key behavioral outcomes: positive surplus in the simulations, total surplus in the simulations, external forest use index, and benefits from external forest use. For the simulation outcomes, this is precisely what we see. For the forest use and benefits outcomes, the signs are consistent across specifications, although the results suggest that the CM intervention may have amplified the effect of the Negotiations treatment on reducing forest use and (perhaps as a consequence) dampened potential benefits from forest use caused by the Negotiations treatment. Given the power loss however, it is unsurprising to see that these results are not statistically significant.

D. Additional Analysis

D.1 Control-group levels

Table A.7: 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.7: 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.

Table A.8: Control-group Levels for Real-world Forest Use

Outcome	Mean	SD	Min	Max	N
Forest Use by External Actors	0.00	1.00	-0.61	6.09	184
Benefits from External Forest Use	0.00	1.00	-0.20	5.01	184
Rule cutting trees	0.84	0.37	0.00	1.00	182
Talked about community forest	2.15	7.00	0.00	60.00	175
Does Not Want to Reduce Logging Activity	0.93	0.26	0.00	1.00	186

Table A.8: Summary statistics for real-world forest use from respondents who were not assigned to either the negotiation training or the other cross-randomized intervention. Indexes are normalized to have mean 0 and standard deviation 1 in the control group.

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. We present this in the top panel of Appendix Figure A.2.

Figure A.2: Probability of Agreement Conditional on the Negotiated Surplus

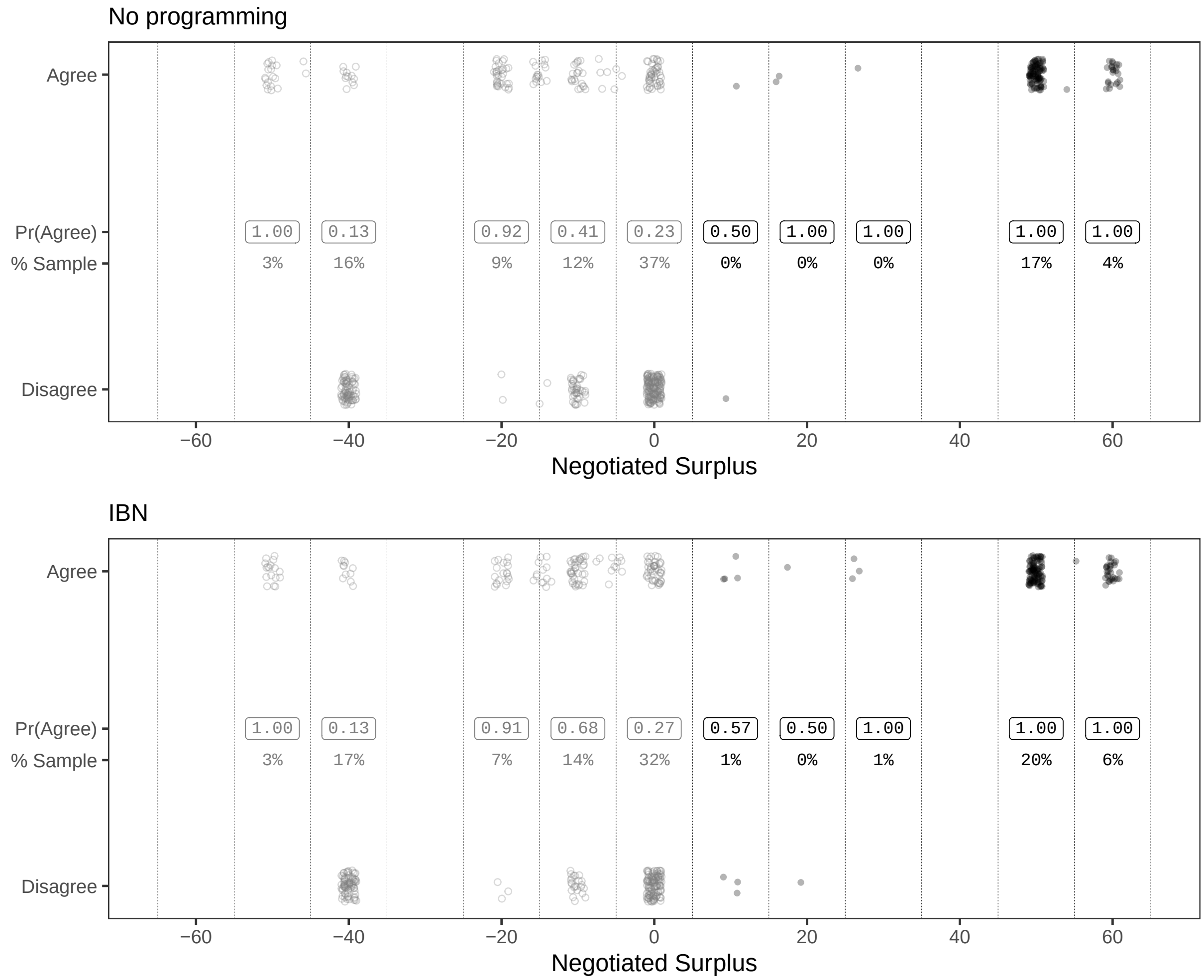


Figure A.2: Using data from the groups with no programming (top panel) and IBN only (bottom panel), we calculate the surplus that a respondent could have earned in a negotiation, which is the price they negotiate minus the value of the outside option from the simulation script. We plot this value along 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. Below this probability, we report the share in each bin.

For completeness, we show the rates of agreement by negotiated surplus in the group that only receives IBN training in the bottom panel of Appendix Figure A.2. Consistent with structural estimates, we find that the treatment improves respondents' capacity to negotiate a higher price: more of the sample falls in the higher bins. Yet, conditional on negotiating a negative surplus, trainees are not much more likely to walk away.

D.2 Full PAP Analysis

Tables A.9 and A.10 present the pre-specified analysis with and without covariate adjustment discussed in Christensen et al. (2021).

Table A.9: 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.9: Pre-specified Outcomes with Covariate Adjustment defined in Equation (1). Standard errors in parentheses are clustered at the community level.

Table A.10: Pre-specified Outcomes without Covariate Adjustment

Outcome	ATE	Std. Error	*p*-value	N
MNP: Manipulation Checks				
Mean-effects Index	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				
Mean-effects Index	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				
Mean-effects Index	-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				
Mean-effects Index	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				
Does Not Display Anger or Frustration	0.032	(0.014)	0.02	2139
H5: Positive Surplus				
Achieves Surplus Greater than Zero	0.068	(0.023)	0.00	2139
H6: Total Surplus				
Surplus Achieved	3.166	(1.472)	0.03	2139
H7: Moderated-Mediator				
Differential Effect of Agreement on Surplus for Trainees	4.578	(2.283)	0.05	2139

Table A.10: Pre-specified Outcomes without Covariate Adjustment. Standard errors in parentheses are clustered at the community level.

D.3 Pre-specified heterogeneous treatment effects

Tables A.11 and A.12 present the pre-specified heterogeneous treatment effects discussed in Christensen et al. (2021).

Table A.11: Heterogeneous Treatment Effects for Above Primary Education

Outcome	ATE	HTE	SE	*p*-value	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

Table A.11: Pre-specified heterogeneous treatment effects by education. † stands for mean-effects index. Standard errors in parentheses are clustered at the community level.

Table A.12: Heterogeneous Treatment Effects for Women

Outcome	ATE	HTE	SE	p-value	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.12: Pre-specified heterogeneous treatment effects by gender. † stands for mean-effects index. Standard errors in parentheses are clustered at the community level.

D.4 Effects on Real-World Forest Use

Table A.13: ATE of IBN on Sub-components of Community Forest (CF) Use

Outcome	ATE	Std. Error	*p*-value	Romano-Wolf *p*-value	N
Forest Use by External Actors†	-0.265	(0.135)	0.052		705
External Forest Use in CF (EA)$^{\circ}$	-0.455	(0.284)	0.115	0.158	693
External Forest Use in CF (SVY)$^{\circ}$	-0.015	(0.059)	0.798	0.789	703
Active Pitsawing in CF$^{\circ}$	-0.172	(0.123)	0.165	0.210	703
External Forest Use beyond CF$^{\circ}$	-0.110	(0.067)	0.104	0.180	695
Benefits from External Forest Use†	0.054	(0.136)	0.691		705
Benefits from Pitsawing$^{\circ}$	0.010	(0.026)	0.691	0.917	705
Benefits from Investment$^{\circ}$	-0.004	(0.016)	0.795	0.917	705

Table A.13: Average treatment effect estimates on real-world outcomes using Equation 1. Standard errors in parentheses are clustered at the community level. † stands for mean-effects index; ○ stands for sub-components of mean-effects index. We also adjust p-values across sub-components of each index per Romano and Wolf (2005).

D.4.1 Heterogeneous Effects

While we did not conduct a baseline survey (due to budget constraints), we can extract pre-treatment measures of forest loss from satellite imagery captured before our IBN training. We use a circular buffer (0.79 sq km) to approximate the community forest in each village and, within that buffer, compute the proportion of primary or secondary forest cleared in 2016 and the first half of 2017. The median community lost 1.5 percent of the forested area within their buffer in the 18 months prior to any programming. We then look at whether the IBN training generated heterogeneous effects for communities above and below the median level of pre-treatment forest loss. We expect the IBN training would have a larger effect on forest use where a community's forest is under more pressure, presumably due in part to deals that permit pitsawing or other forms of external forest use.

Looking first at the raw data, in new Appendix Figure A.3 we see that the IBN training generates a steeper decline in the number of external forest-use sites (i.e., pitsawing, mining, or agribusiness) uncovered in our independent environmental assessments (EAs) in communities experiencing greater forest loss prior to the intervention. As expected, the control level is lower in communities experiencing less deforestation prior to the intervention, and we see a minor reduction in this level when the IBN training is conducted in such communities. By contrast, in communities with above median forest loss prior to treatment, the number of external forest use sites uncovered in the EAs falls from 1.7 to 1.1.

We then incorporate this indicator for above-median deforestation into our covariate adjustment. Our Equation 1 includes this demeaned indicator and its interaction with our treatment variable.

Figure A.3: Average External Forest Use by IBN and Pre-Treatment Forest Loss

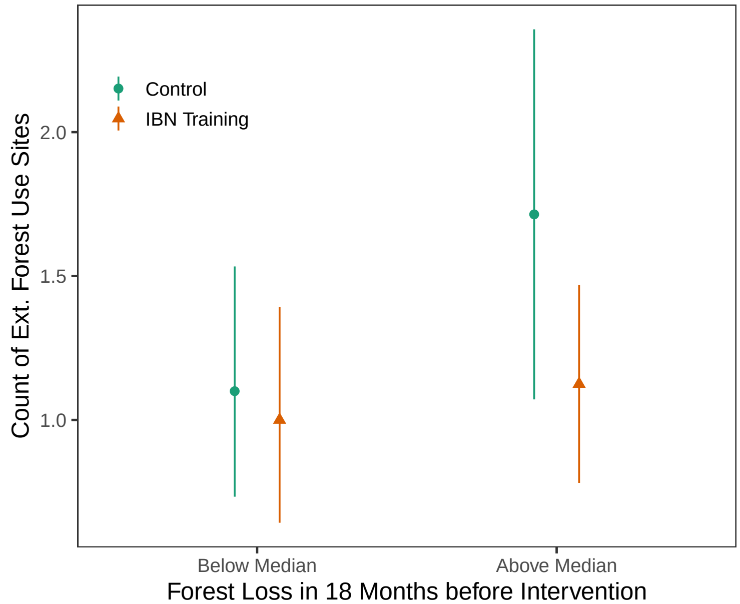


Figure A.3: We compute the average number of external forest use sites (pitsawing, mining, agribusiness) detected in the EAs, stratifying on whether a community is above or below the median of pre-intervention forest loss and whether a community received the IBN training. We remotely sense forest loss in 2016 and 2017 to measure deforestation at the community-level in the 18 months prior to any intervention. We focus attention on a circular buffer (0.79 sq km) that is centered on sites geotagged during the independent environmental assessments (EAs), which provides a rough approximation of the community forest.

Table A.14: Heterogeneous Effects on External Forest Use by Pre-treatment Deforestation

			Abv. Med. Forest Loss		$(H_0 : HTE = 0)$	
	ATE	Std. Error	HTE	Std. Error	p-value	N
Forest Use by External Actors†	-0.291	(0.133)	-0.392	(0.248)	0.119	705
External Forest Use in CF (EA)$^{\circ}$	-0.466	(0.282)	-0.429	(0.602)	0.479	693
External Forest Use in CF (SVY)$^{\circ}$	-0.022	(0.061)	-0.065	(0.140)	0.643	703
Active Pitsawing in CF$^{\circ}$	-0.189	(0.125)	-0.318	(0.223)	0.160	703
External Forest Use beyond CF$^{\circ}$	-0.116	(0.067)	-0.155	(0.130)	0.237	695

Table A.14: Average treatment effect estimates on real-world outcomes using Equation 1. We include in the covariate adjustment an indicator for whether a community had above-median forest loss prior to the IBN training. The HTE reported above is the coefficient on the interaction between this demeaned indicator and our treatment variable. The p-value relates to the null hypothesis that this HTE estimate equals zero. Standard errors in parentheses are clustered at the community level. † stands for mean-effects index; ∘ stands for sub-components of mean-effects index.

Appendix Table A.14 reports the sample ATE (when the indicator is at its mean), as well as the interactions between the treatment and the demeaned indicator, which we label the HTE. In communities with above-median forest loss prior to treatment, the treatment effect is then ATE + HTE

$\times$ 0.5, where 0.5 is the value of the demeaned indicator in such communities. While we cannot reject the null that this HTE differs from zero at conventional levels, across the index and all sub-components the HTE takes the same sign and is sizeable in magnitude. The IBN training is more pertinent and, thus, has a larger impact in communities facing more pressure to exploit their forestland.

D.5 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.15 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.15: 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.15: 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.

D.6 Spatial Spillovers

Table A.16 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.16: Spatial spillovers

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)	0.28	1,065
H5: Positive Surplus	0.002	(0.004)	0.60	1,065
H6: Total Surplus	0.070	(0.218)	0.76	1,065
Expl: Forest Use by External Actors	-0.011	(0.028)	0.71	351

Table A.16: Estimates from the spatial spillover. Standard errors in parentheses are clustered at the community level. † stands for mean-effects index. * indicates that sample is restricted to control communities.

D.7 Analysis of Remotely Sensed Deforestation

Table A.17 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.17: Analysis of remotely sensed deforestation

Outcome	ATE	Std. Error	*p*-value	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.17: 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 Knowledge Indexes

We construct mean-effects indexes a la Kling, Liebman, and Katz (2007) to measure the knowledge of possible deals and the knowledge of outside options.

The index of “knowledge of possible deals" combines answers from: (i) correctly defining IBN, (ii) understating the importance of agreements that work for both parties, (iii) importance of developing strategies to improve relationship, (iv) preparation to understand the interest of the other party, and (v) 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 index of “knowledge of the outside option" combines answers from (i) correctly defining IBN, (ii) understanding the difference between interest and position and (iii) how to appraise their outside option.

We note that the indexes share a common first item, (i) correctly defining IBN. Appendix Table A.18 re-estimates the indirect effects of these mediators excluding this item from one or both of the indexes, and our conclusions are the same in all cases.

Table A.18: Mediation Analysis Excluding Item (i) Correctly Defining IBN

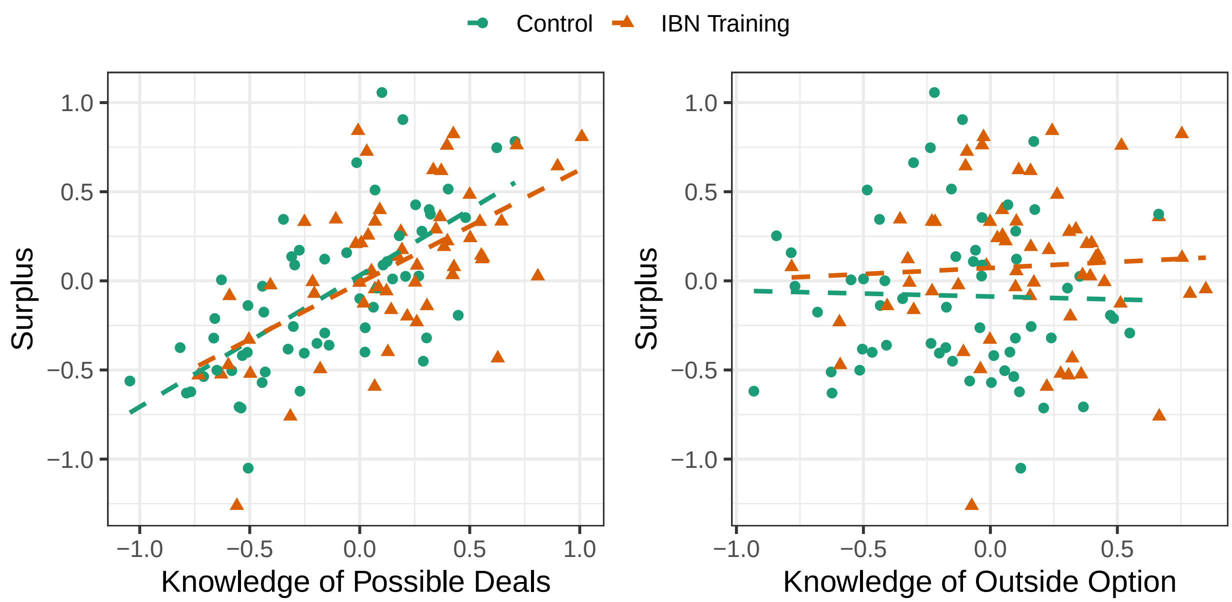
	Indirect Effects of Knowledge Index on Surplus		
	Possible Deals	Outside Option	Direct Effect
Estimates in Table 2	0.15	0.02	-0.01
	(0.04)	(0.01)	(0.07)
Excluding (i) from Possible Deals	0.13	0.05	-0.01
	(0.04)	(0.01)	(0.06)
Excluding (i) from Outside Option	0.16	0.02	-0.02
	(0.04)	(0.01)	(0.06)
Excluding (i) from both	0.13	0.03	0.01
	(0.04)	(0.01)	(0.06)

Table A.18 presents the mediation results using alternative knowledge indexes that exclude item (i) correctly defining IBN.

E.2 Robustness Check

Appendix Figure A.4 plots the bivariate relationship between the knowledge indices and surplus (after both are residualized) with separate best-fit lines included for the treatment and control communities. We cannot reject the null hypothesis that these slopes are the same in both groups for either knowledge index.

Figure A.4: Relationship between Mediators and Surplus at Community-level



Appendix Figure A.4 is created using the same steps as Figure 2, but we separately plot the linear relationships between the knowledge indices and surplus for treatment and control communities.

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

We also conduct our mediation analysis using principal component analysis to construct the intermediate knowledge variables (PCA). Using PCA, the first component loads more on items we related to the knowledge of possible deals; while the second component loads on items we related to the knowledge of outside option: cor(PC_2, knowledge of outside option) = 0.999 and cor(PC_1, knowledge of possible deals) = 0.669. In Appendix Table A.19, we estimate a larger indirect effect for PC_2, which is consistent with our finding in Table 2 that knowledge gains related to identifying possible deals account for more of the total reduced-form effect.

PCA allows us to be agnostic about how to combine and weight the items in our knowledge indexes. The PCA reveals, first, that our preferred knowledge indexes largely separate two distinct dimensions of knowledge. The first principal component (PC_1) is highly correlated ($r = 0.99$) with knowledge of outside option; PC_2, with knowledge of possible deals ($r = 0.67$). Second, when we perform the mediation analysis, our indirect-effect estimates suggest that PC_2 accounts for more of the total, reduced-form effect. This robustness check indicates that our mediation analysis does not depend on arbitrary decisions about how to group or weight items.

Table A.19: Mediation Analysis using Principal Components

Panel A: First-Stage Estimates			
	PC_1	PC_2	**Surplus**
Treatment	0.237 (0.071)	0.268 (0.104)	0.069 (0.079)
PC_1			0.005 (0.126)
PC_2			0.368 (0.243)
Panel B: Decomposition of the Total Effect of IBN on Std. Surplus			
Total	0.169 (0.085)	**Indirect:** PC_1	0.001 (0.027)
Direct	0.069 (0.079)	**Indirect:** PC_2	0.099 (0.082)

Table A.19: Mediation analysis using first two principle components. 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 with 200 repetitions in parentheses are clustered at the community level.

F. Structural Model

F.1 Estimation of Lee Bounds for Negotiation Capacity

We estimate Lee (2009) bounds as follows:

- Assume that treatment increases the rate of agreement (monotonicity).
- Estimate effect of treatment on the probability of agreement. Call this share the induced to agree q.
- Remove share q from top and bottom of treatment group distribution and re-estimate k.

This procedure produces a lower bound of 0.62 and and upper bound of 5.88.

F.2 Structural Estimates by Subgroup

Our sample of community leaders includes village and quarter chiefs, as well as individuals who tend to have less say in forest use given their position, gender, or education (e.g., school teachers, midwives). In new analysis presented in Appendix Section F.2, we split our sample of leaders by position, gender, and education and look, first, at the self-reported engagement and influence of these subgroups in control communities; and second, at the effect of the IBN training on these subgroups' appraisal of their outside option (δ_1) (see Table A.20, reproduced below). Among the sub-groups that we would expect to be most involved in decisions related to the community forest, we find that the IBN training tends to improve their ability to appraise their outside option. The negative and insignificant structural estimate we report in Table 2 is driven by sub-groups that tend to be relatively disengaged and/or powerless. While this subgroup analysis is admittedly underpowered, it provides a way of reconciling our lab-in-the-field and real-world results: among those with sway over the community forest, IBN training made them more discerning, which could have led to the reduction in external forest use reported in Table 3.

First, using only observations from control communities, we summarize the relative engagement and influence in each of these subgroups (see Table A.20). (Only 15 women in our sample (6%) have primary education, so we do not split women by education.) Engagement is the number of people the respondent has spoken with about the community forest in the last week, top-coded to 20 to limit the influence of 4 outliers. Influence indicates whether a respondent believes they can influence rule-makers in their community. We demean these variables by community to capture each respondent's engagement or influence relative to other (surveyed) leaders in their community. Table A.20 shows the number of observations in each subgroup across control communities, as well as their average engagement and influence. Absent treatment, we find that chiefs and educated youth leaders tend to be relatively influential and engaged. By contrast, uneducated youth leaders and women serving as the women's leader, teacher, and midwife self-report little engagement and influence. Elders and landlords are a mixed bag: the educated are engaged but not influential, and the uneducated are not particularly engaged but claim to have some sway.

Table A.20: Sub-group Analysis of Influence and Appraisal

	Control Communities			$\widehat{\delta}_1$	
	N	Engagement	Influence		
Chiefs					
Men with Education	6	0.80	0.00	0.01	(0.31)
Men without Education	24	0.90	0.17	0.12	(0.23)
Women	3	-0.06	0.19	0.47	(1.35)
Youth Leaders					
Men with Education	19	1.74	0.04	0.12	(0.24)
Men without Education	11	-0.58	-0.16	-0.45	(0.40)
Elders/Landlords					
Men with Education	16	0.75	-0.01	-0.12	(0.28)
Men without Education	42	0.23	0.05	-0.36	(0.16)
Women	2	-0.42	0.17		
Womens Leaders					
Women	29	-1.18	-0.16	-0.15	(0.20)
Other					
Men without Education	2	-0.17	0.08		
Women	30	-1.31	-0.04	-0.14	(0.19)
All	184	0.00	0.00	-0.11	(0.08)

Table A.20: We create subgroups based on position, gender, and education. We do not split women by education, as only 6% of females have primary education. Using only observations from control communities, we first compute the average engagement and influence of the respondents in these subgroups. These variables have been de-meaned by community, so values above zero correspond to above-average engagement or influence relative to the other (surveyed) leaders in one's community. We then structurally estimate the effect of the IBN training on appraisal for each of these subgroups ($\widehat{\delta}_1$). Standard errors, reported in parentheses, are clustered on community, which is the unit of assignment. We do not have sufficient observations in treatment and control for all sub-groups to estimate treatment effects.

Our structural estimates of the effect of IBN training on appraisal ($\widehat{\delta}_1$) are positive for all types of chiefs, as well as for educated youth leaders. While the standard errors are large, the point estimates suggest that these types of trainees, conditional on the price they were able to negotiate, are more inclined to walk away and take their outside option. For the other subgroups, we estimate a negative effect of IBN on their ability to appraise their outside option. These other subgroups make up a majority of our sample and, thus, generate the negative (if insignificant) estimate that we report in Table 2. However, this analysis shows that they also tend to be less engaged and influential and, thus, may not influence real-world outcomes related to forest use.

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.

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.21: 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
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
Forest Use by External actors	EA/SVY	Index combining information from External Forest Use in CF (EA), External Forest Use in CF (SVY), Active Pitsawing in CF (SVY), External Forest Use beyond CF (SVY) (defined below)
External Forest Use in community forest	EA	Count of external forest-use activities (concessions, mining, pitsawing) in the community forest (detected)
External Forest Use in community forest	SVY	Count of external forest-use activities (concessions, mining, pitsawing) in the community forest over the previous 3 months (self-reported)
Active Pitsawing in community forest	SVY	Index that incorporates whether there is active pitsawing (i.e., manually sawing logs into boards using a large saw operated by two people) and how many trees are being logged (self-reported)
External Forest Use beyond community forest	SVY	Count of external forest-use activities (concessions, mining, pitsawing) *outside* of the community forest over the previous 3 months (self-reported)
Benefits from External Forest Use	SVY	Index combining information from Benefits from Pitsawing and Benefits from Investment (defined below)
Benefits from Pitsawing	SVY	Benefits received from Pitsawing (money, building materials, roads or bridges, other tokens, other services)
Benefits from Investment	SVY	Benefits received from external investment in forest-use (money, building materials, roads or bridges, other tokens, other services)

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

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