ONLINE APPENDIX TO "INTEREST-BASED NEGOTIATION OVER NATURAL RESOURCES: EXPERIMENTAL EVIDENCE FROM LIBERIA"

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Appendix 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, cannot 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	299.06	127.75	437.29	12.50	2639.00	0	120
Urban	0.05	0.00	0.21	0.00	1.00	0	120
Under 18	0.47	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	0.26	0.34	0.00	1.00	0	120

Notes: 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%

Notes: 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

Notes: 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

Notes: 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.

Appendix 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. 2021b).

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. 2021b). .

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 et al. 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 et al. (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.

Appendix 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 C.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. 2021b). As such, we analyze the data as if we used complete block random assignment.

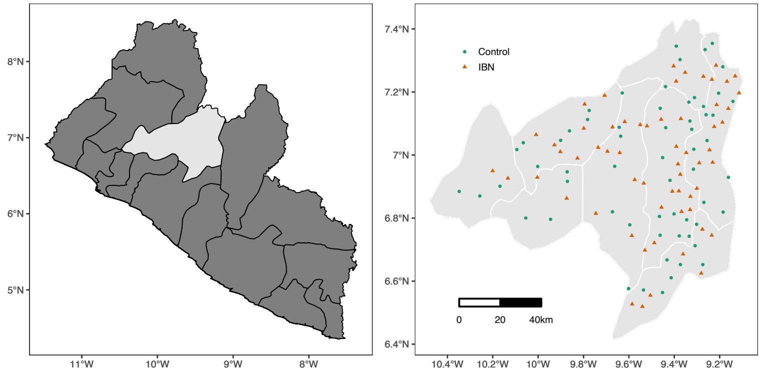

FIGURE C.1. Treatment Assignment. Treatment assignment for the 120 communities in the evaluation sample. Communities were organized into blocks and then randomized into one of four groups: (1) Control and (2) IBN training.

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 C.1 presents the balance tests.

TABLE C.1. 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

Notes: 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):

$$
\begin{aligned}
Y_{sibc} = {} & \alpha + \beta\, \mathbb{1}(\mathrm{NEG})_{bc} \\
& + \varphi_1\, \tilde{\mathbb{1}}(\mathrm{CM})_{bc} + \varphi_2\, \mathbb{1}(\mathrm{NEG})_{bc} \times \tilde{\mathbb{1}}(\mathrm{CM})_{bc} \\
& + \sum_{b=1}^{B-1} \left[\varphi_{3b}\, \tilde{\mathbb{1}}_b + \varphi_{4b}\, \mathbb{1}(\mathrm{NEG})_{bc} \times \tilde{\mathbb{1}}_b \right] \\
& + \sum_{s=1}^{2} \left[\varphi_{5s}\, \tilde{\mathbb{1}}_s + \varphi_{6s}\, \mathbb{1}(\mathrm{NEG})_{bc} \times \tilde{\mathbb{1}}_s \right] \\
& + \sum_{k=1}^{K} \left[\varphi_{7k}\, \tilde{X}_{k,ibc} + \varphi_{8k}\, \mathbb{1}(\mathrm{NEG})_{bc} \times \tilde{X}_{k,ibc} \right] + \varepsilon_{sibc}
\end{aligned}
\tag{C.1}
$$

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 $\tilde{.}$ 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.[1]

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.

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

To test this, we work with a specification that takes the same form as Equation C.1, except that we also include an indicator for agreement as well as the interaction between agreement and the treatment:

$$\text{Surplus}_{sibc} = \alpha \ + \ \beta_1 \, \mathbb{1}(\text{NEG})_{bc} + \beta_2 \, \mathbb{1}(\text{Agree})_{sibc} + \beta_3 \, \mathbb{1}(\text{NEG})_{bc} \times \mathbb{1}(\text{Agree})_{sibc} \tag{C.2}$$

$$+ \ \varphi_1 \, \tilde{\mathbb{1}}(\text{CM})_{bc} + \varphi_2 \, \mathbb{1}(\text{NEG})_{bc} \times \tilde{\mathbb{1}}(\text{CM})_{bc}$$

$$+ \ \sum_{b=1}^{B-1} \left[\varphi_{3b} \, \tilde{\mathbb{1}}_b + \varphi_{4b} \, \mathbb{1}(\text{NEG})_{bc} \times \tilde{\mathbb{1}}_b \right]$$

$$+ \ \sum_{s=1}^{2} \left[\varphi_{5s} \, \tilde{\mathbb{1}}_s + \varphi_{6s} \, \mathbb{1}(\text{NEG})_{bc} \times \tilde{\mathbb{1}}_s \right]$$

$$+ \ \sum_{k=1}^{K} \left[\varphi_{7k} \, \tilde{X}_{k,ibc} + \varphi_{8k} \, \mathbb{1}(\text{NEG})_{bc} \times \tilde{X}_{k,ibc} \right] + \varepsilon_{sibc}$$

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 et al. (2021a). 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 et al. (2024) 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 et al. 2023). We think this is justified because our specification and target effect allow us to use the full sample, meaning more statistical power.

TABLE C.2. 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)	(1.7778)	(0.1380)	(0.1626)
Community Monitoring (CM)	-0.0068	0.2060	0.3320	0.3541**
	(0.0309)	(1.9771)	(0.2520)	(0.1785)
IBN × CM	0.0084	-0.7463	-0.3230	-0.3927
	(0.0436)	(2.8181)	(0.2740)	(0.2652)
N	2,115	2,115	705	705

Notes: 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 et al. (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 C.2). 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.

Appendix D: Additional Analysis

D.1. Control-group levels

TABLE D.1. 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

Notes: 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 D.2. Control-group Levels for Real-world Forest Use.

Outcome	Mean	SD	Min	Max	N
Forest Use by External Actors	0.00	1.00	-0.77	5.48	184
Benefits from External Forest Use	0.00	1.00	-0.20	5.01	184
Rule in Community against Logging w/o Permission	0.84	0.37	0.00	1.00	182
Neighbors Consulted about Forest in Last Week	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
Price Demanded to Clear Forest (logged)	8.54	1.99	0.00	13.82	184

Notes: 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 D.1.

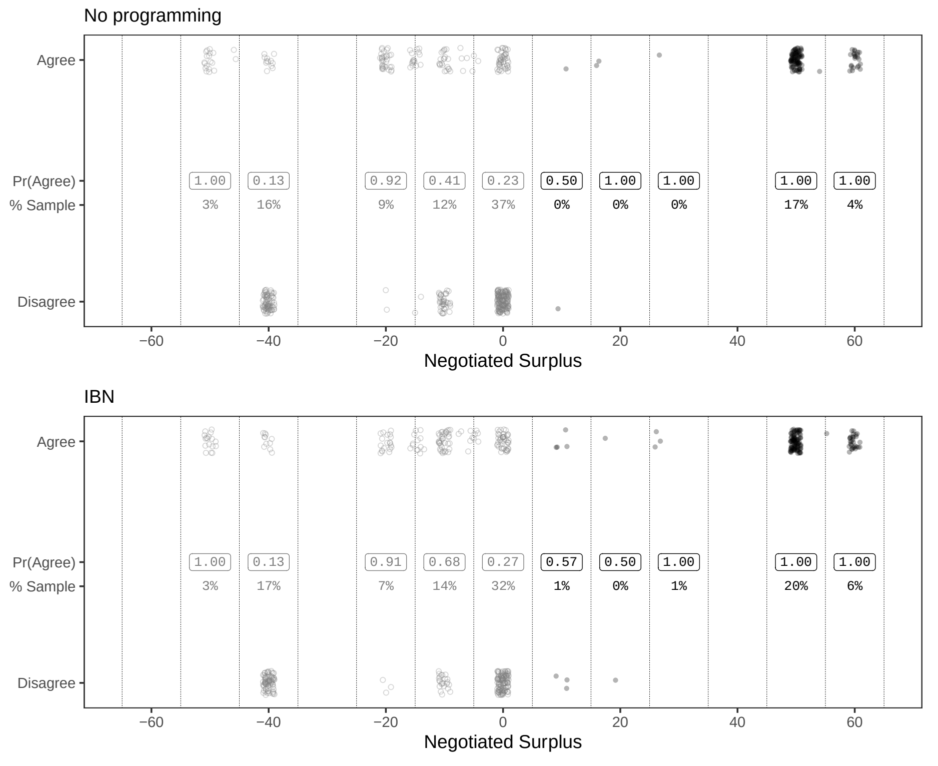

FIGURE D.1. Probability of Agreement Conditional on the Negotiated Surplus. Using data from the groups with no programming (top panel) and IBN only (bottom panel), we calculate the surplus that a respondent could have earned in a negotiation, which is the price they negotiate minus the value of the outside 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 D.1. 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 D.3 and D.4 present the pre-specified analysis with and without covariate adjustment discussed in Christensen et al. (2021b).

TABLE D.3. 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

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

TABLE D.4. 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

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

D.3. *Pre-specified heterogeneous treatment effects*

Tables D.5 and D.6 present the pre-specified heterogeneous treatment effects discussed in Christensen et al. (2021b).

Table D.5. 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

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

TABLE D.6. 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

Notes: 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 D.7. 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	0.201	693
External Forest Use in CF (SVY)$^{\circ}$	-0.015	(0.059)	0.798	0.460	703
Active Pitsawing in CF$^{\circ}$	-0.172	(0.123)	0.165	0.204	703
External Forest Use beyond CF$^{\circ}$	-0.110	(0.067)	0.104	0.219	695
Benefits from External Forest Use†	0.054	(0.136)	0.691		705
Benefits from Pitsawing$^{\circ}$	0.010	(0.026)	0.691	0.950	705
Benefits from Investment$^{\circ}$	-0.004	(0.016)	0.795	0.950	705

Notes: 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 D.2 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

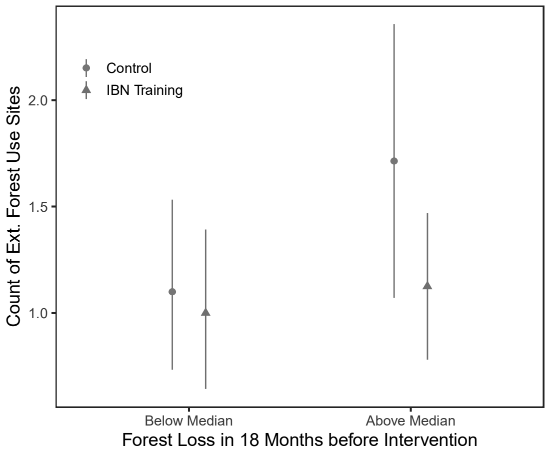

FIGURE D.2. Average External Forest Use by IBN and Pre-Treatment Forest Loss. We compute the average number of external forest use sites (pitsawing, mining, agribusiness) detected in the EAs, stratifying on whether a community is above or below the median of pre-intervention forest loss and whether a 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.

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.

TABLE D.8. 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)°	-0.466	(0.282)	-0.429	(0.602)	0.479	693
External Forest Use in CF (SVY)°	-0.022	(0.061)	-0.065	(0.140)	0.643	703
Active Pitsawing in CF°	-0.189	(0.125)	-0.318	(0.223)	0.160	703
External Forest Use beyond CF°	-0.116	(0.067)	-0.155	(0.130)	0.237	695

Notes: 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.

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. Appendix Table D.8 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 sizable 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. Analysis of Remotely Sensed Deforestation

Table D.9 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 D.9. 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

Notes: 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.

D.6. Spatial Spillovers

Table D.10 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 = 6.1 km).

TABLE D.10. Spatial spillovers

Outcome	Estimate ($\widehat{\beta}$)	Std. Error	p-value	N*
H1: Knowledge of IBN†	-0.006	(0.021)	0.77	355
H2: Knowledge of Inter-personal Skills†	-0.003	(0.024)	0.90	355
H3: Deployment of IBN Skills†	0.011	(0.027)	0.68	355
H4: Deployment of Inter-personal Skills	0.006	(0.004)	0.10	1,065
H5: Positive Surplus	-0.004	(0.004)	0.41	1,065
H6: Total Surplus	-0.108	(0.299)	0.72	1,065
Expl: Forest Use by External Actors	-0.012	(0.043)	0.78	351

Notes: 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. 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 within-community spillovers. Table D.11 shows that the changes in material benefits from external forest use are similar to trainees. In addition, we do not observe a 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 D.11. Within-Community Spillovers

Outcome	ATE	Std. Error	p-value	N
Benefits from External Forest Use†	0.073	(0.167)	0.662	476
Satisfaction with Leadership				
Overall satisfaction	-0.013	(0.033)	0.690	476
Satisfaction related to the community forest	-0.028	(0.040)	0.484	476

Notes: 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.

Appendix E: Mediation Analysis

E.1. Knowledge Indexes

We construct mean-effects indexes a la Kling et al. (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) behavior 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 E.1 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 E.1. 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)	(0.06)
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 E.1 presents the mediation results using alternative knowledge indexes that exclude item (i) correctly defining IBN.

E.2. Robustness Check

Appendix Figure E.1 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.

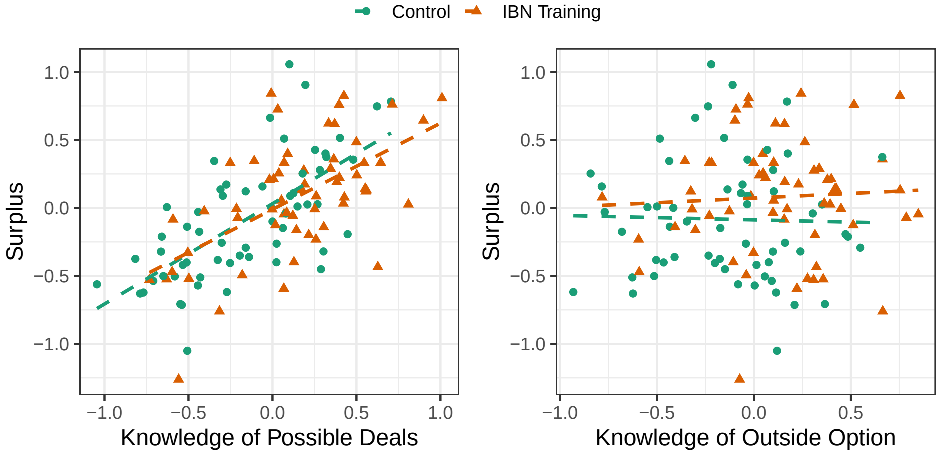

FIGURE E.1. Relationship between Mediators and Surplus at Community-level. Appendix Figure E.1 is created using the same steps as Figure 2, but we separately plot the linear relationships between the knowledge indices and surplus for treatment and control communities.

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 principal component (PC_1) loads more on items we related to the knowledge of possible deals; while the second principal component (PC_2) 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 E.2, 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 E.2. Mediation Analysis using Principal Components.

Panel A: First-Stage Estimates			
	PC_1	PC_2	**Surplus**
Treatment	0.237 (0.073)	0.268 (0.110)	0.069 (0.079)
PC_1			0.005 (0.125)
PC_2			0.368 (0.255)
Panel B: Decomposition of the Total Effect of IBN on Std. Surplus			
Total	0.169 (0.070)	**Indirect:** PC_1	0.001 (0.028)
Direct	0.069 (0.079)	**Indirect:** PC_2	0.099 (0.087)

Notes: 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.

Appendix 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 F.1, 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 under-powered, 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 F.1). (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 F.1 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 F.1. Sub-group Analysis of Influence and Appraisal.

	Control Communities				
	N	Engagement	Influence	$\widehat{\delta_1}$	
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)

Notes: 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.

Using respondents in these sub-groups assigned to treatment and control, we then compute our structural estimates of the effect of IBN training on appraisal ($\widehat{\delta_1}$), which 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.

Appendix G: Analysis Plan

G.1. Deviation from the PAP

In this section, we report the deviation from the PAP (Christensen et al. 2021b). 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 G.1. 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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