Online Appendix

A Power calculations

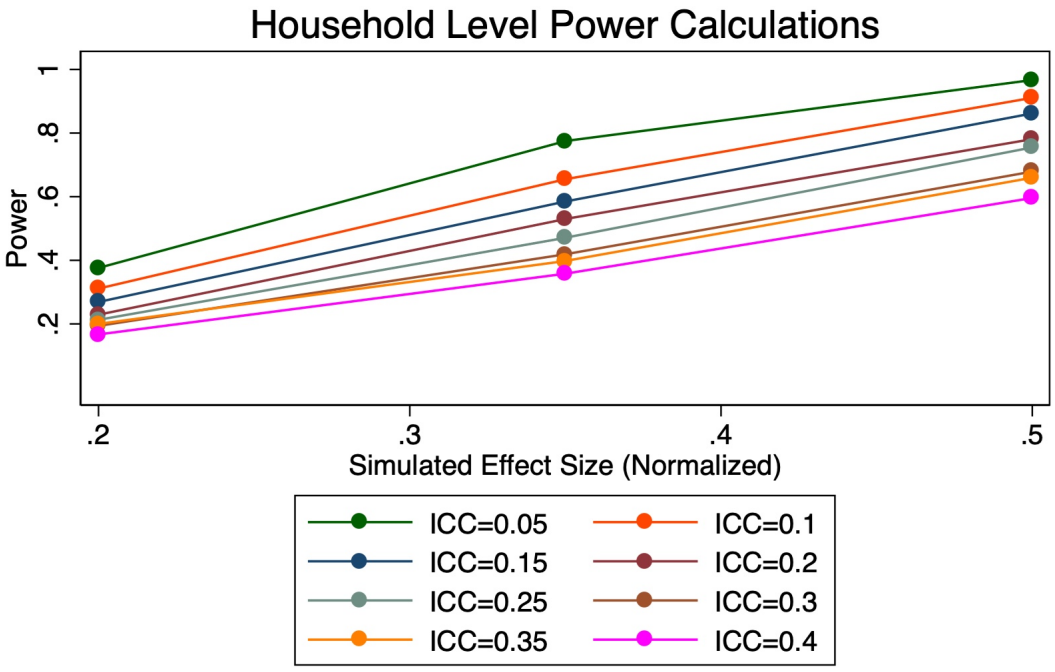
Power calculations are carried out using an ANCOVA specification. Calculations assume a sample size of 50 control villages, and 10 villages which receive both the community monitoring and the SMS reminder treatment. Within these villages, actual realised sample sizes in terms of numbers of households are used, averaging 9 for SMS reminder villages and 9.6 for control villages. These actual realised sample sizes are lower than those used in the pre-analysis plan due to attrition, and missing control data for some observations. For households in the SMS reminder treatment communities, the sample size is substantially lower than the pre-analysis plan, in which a sample of 20 was expected. Power is calculated for a range of normalized effect sizes, and for a range of Intra Cluster Correlations (ICC) at randomization block level. A conservative estimate of inter-temporal correlation (0.15) is also included in each calculation. This inter-temporal correlation was selected based on an assessment of auto-correlation in forest coverage in the years prior to the study, using Hansen et al. (2013) forest cover loss data in a sub-set of project villages from Masindi and Hoima districts. Although there are limited theoretical links between this outcome and many of the outcomes used in this study, this provides a conservative estimate (one might expect many household level outcomes to be highly autocorrelated). An estimate for ICC is harder to compute without pilot data. Hence power to standardized effect sizes are calculated for a range of ICCs.

Results are provided in Figure A.1. Results are highly sensitive to the choice of ICC, and only studies with a large effect size (0.5) and/or a low ICC (less than 0.2) are powered at the conventional level of 80%.

B Results for pre-specified outcomes not in the main text

This section presents results on the impact of the SMS reminder treatment on those outcome variables that were pre-specified in this study's pre-analysis plan (PAP), but that were not presented in the main text. The SMS reminder treatment did not statistically significantly affect these outcomes.

Figure A.1: Power calculations



Note: This figure denotes the share of simulated regressions able to detect a given simulated effect size (x-axis, measured in standard deviation units), at different levels of Intra-Cluster Correlation (ICC).

Table A.1: Components of scrutiny and sanctions indices

Outcome variable	Component household survey questions	
Scrutiny of others		In the past 12 months, have you or members of your household voluntarily patrolled the common pool forest? [Yes=1]
		Imagine your neighbour broke a rule relating to forest use. How likely is it that you would notice that your neighbour did this? (Likert scale, very likely=5)
Sanction others	Hypothetical	Imagine your neighbour broke a rule relating to forest use. How likely is it you would scold your neighbour? (Likert scale, very likely=5)
	Hypothetical	Imagine your neighbour broke a rule relating to forest use. How likely is it you would report your neighbour to (i) the local government (ii) a community-based organisation responsible for common pool forest (iii) the NFA? (Likert scale, very likely=5)
	Actual	At times, people in this village may break the rules relating to forest use. In the past year, have you personally (i) scolded someone in the villages for breaking the rules? [Yes=1] (ii) reported someone in the village to the LC1, the CFM or CLA or the NFA, for breaking the rules? [Yes=1]
Perceived scrutiny by others		Imagine you broke a rule relating to forest use. How likely is it that your neighbour would notice that you did this? (Likert scale, very likely=5)
Perceived likelihood of sanctioning	Hypothetical	If a household in this village breaks a rule about forest use, how likely is it that they will receive a penalty? (Likert scale, very likely=5)
	Actual	In the past 12 months, has the [authority] exacted any penalties on you or members of your households for violating the rules regarding the use of the forest? [Yes=1]
	Actual	In the past 12 months, have you or members of your household been scolded/told off for violating the rules regarding the use of the forest? [Yes=1]

The PAP hypothesized that the SMS reminder treatment might increase participation in forest governing bodies and satisfaction with forest use rules. The reasoning behind this is the following. It follows from hypotheses H1 and H2 in the main text that SMS reminders may increase users' knowledge of or attentiveness to forest use rules they disagree with. This may provide an incentive to attempt to change these rules through participation in forest governing bodies. If successful, such participation may increase users' satisfaction with forest use rules.

Columns 1 and 2 of Table B.2 present the effect of the SMS reminder treatment on satisfaction with rules and participation in forest governing bodies. This effect is not statistically significant in either case. The signs on the coefficients of interest are furthermore the opposite of those hypothesized.

The PAP stated that the effect of the SMS reminder treatment on household material welfare is indeterminate. In the short term, the treatment might reduce in household material welfare due to decreased forest use. In the medium and long term, forest conservation might benefit household material welfare due to the continuous availability of forest products for all households in the village. Note that the measure of welfare employed, a household level asset index, captures private household level wealth only, and does not reflect any benefits of forest conservation at the village level that are not reflected by an increase in household assets.[17] As is evident from column 3 of Table B.2, the SMS reminder treatment did not significantly affect this measure of household welfare: the coefficient of interest is insignificant and the effect size is small, about 0.05 standard deviations.

The net treatment's net welfare effect is likely to be negligible. Given that the SMS reminders did not systematically reduce forest use, there is little evidence that they affected local or global externalities associated with forest use and deforestation. Moreover, the cost of sending the SMS reminders is small as well.

Lastly, column 4 of Table B.2 presents estimates for the effect of the SMS reminder treatment on the pre-specified index for the salience of forest use rules. This index combines the variable capturing attentiveness to forest use rules used in the main paper with an additional variable resulting from the following household survey question: "Would you agree with the following statement: It is important that people stick to the communal forest use rules even if they suffer a shock?".

[17] The treatment's overall welfare effect is likely to be negligible. Given that the SMS reminders did not systematically reduce forest use, there is little evidence that they affected local or global externalities associated with forest use and deforestation. Moreover, the treatment did not affect material welfare and cost of sending the SMS reminders is small.

As with attentiveness, the SMS reminder treatment does not have a statistically significant impact on this measure of the salience of forest use rules. As with the result in the main text, this could be due to a ceiling effect. The community monitoring treatment, which was administered contemporaneously with the SMS reminder treatment, significantly increased the salience of forest use rules, and there may have been little space for a second treatment to increase this further.

Table B.2: Other pre-specified outcomes

	(1) Satisfaction with rules	(2) Participation governance	(3) Welfare	(4) Salience of rules
Monitoring	0.012	0.013	0.053***	0.154***
	(0.067)	(0.055)	(0.012)	(0.040)
Monitoring × Rules	-0.070	-0.157	-0.050	-0.049
	(0.115)	(0.117)	(0.033)	(0.089)
$\beta_1 + \beta_2$	-0.058	-0.144	0.002	0.104
$\beta_1 + \beta_2$ p-value	0.622	0.210	0.947	0.249
Control mean	0.031	-0.016	-0.012	-0.029
Lag dep. var.	No	No	Yes	No
Controls	Yes	Yes	Yes	Yes
Observations	913	731	963	916

Standard errors (clustered at the village level) in parentheses
* p<0.1, ** p<0.05, *** p<0.01

C Results without control variables

This section re-estimates all household-level results presented in the main text, omitting all control variables. This has the advantage of allowing more households from SMS reminder treatment villages into the sample. The number of households that was administered the household survey in these villages was doubled at endline, but these households are dropped from the pre-specified analysis as no baseline control variables were collected for them. Omitting control variables, which were selected for their ability to predict the main dependent variable, does lead to a loss in precision.

Results are broadly similar to those presented in the main text. When omitting control variables, we find additional evidence that the SMS reminder treatment improved knowledge of forest use rules. Table C.3 presents estimates of the impact of the SMS reminder treatment on knowledge. The estimated effect on the knowledge index (column 1) is of a very similar magnitude to that presented in the

main text, and unlike in the main text, this result is statistically significant, albeit at the 10% level. Contrary to what is presented in the main text however, this result is driven by objective knowledge, not self-reported knowledge, although neither result is statistically significant. Omitting control variables does not qualitatively affect the estimates of the impact of the SMS reminder treatment on attentiveness to forest use rules.

Table C.3: Knowledge of and attentiveness to forest use rules

	(1) Knowledge	(2) Self-reported	(3) Objective	(4) Attentiveness
Monitoring	0.110*	0.098	0.134	0.034
	(0.062)	(0.063)	(0.083)	(0.065)
Monitoring × Rules	0.256*	0.074	0.285	0.059
	(0.151)	(0.120)	(0.195)	(0.120)
$\beta_1 + \beta_2$	0.366	0.172	0.419	0.093
$\beta_1 + \beta_2$ p-value	0.011	0.192	0.027	0.470
Control mean	-0.031	0.000	-0.053	-0.000
Lag dep. var.	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	1205	1184	1205	1206

Standard errors (clustered at the village level) in parentheses
* p<0.1, ** p<0.05, *** p<0.01

Omitting control variables does not meaningfully affect the estimates of the impact of the SMS reminder treatment on scrutiny and sanctioning of others (Table C.4). As in the main text, none of the estimates are statistically significant. Furthermore the size of the coefficient is small, even smaller than the one presented in the main text.

Omitting control variables does weaken the result on perceived scrutiny and sanctioning by others. In the main text, we find a statistically significant impact of the SMS reminder treatment on an index capturing sanctioning by others and hypothetical sanctioning by others. The corresponding coefficients are no longer statistically significant when the control variables are omitted (Table C.5, columns 2 and 3 respectively). The estimated effect on hypothetical sanctioning remains substantial in size, at 0.2 standard deviations compared to 0.39 standard deviations in the main text.

Finally, Table C.6 presents estimates when taking non-compliance with forest use rules and forest use as dependent variables. Estimates are comparable to those

Table C.4: Scrutiny and sanctioning of others

	(1) Scrutiny of others	(2) Sanctioning others	(3) Hypothetical	(4) Actual
Monitoring	0.015	-0.010	-0.015	-0.001
	(0.048)	(0.046)	(0.055)	(0.047)
Monitoring × Rules	-0.023	-0.006	-0.002	-0.013
	(0.077)	(0.068)	(0.084)	(0.083)
$\beta_1 + \beta_2$	-0.007	-0.016	-0.017	-0.014
$\beta_1 + \beta_2$ p-value	0.919	0.824	0.840	0.881
Control mean	-0.029	-0.002	-0.001	-0.000
Lag dep. var.	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	1204	1206	1205	1206

Standard errors (clustered at the village level) in parentheses
* p<0.1, ** p<0.05, *** p<0.01

Table C.5: Scrutiny and sanctioning by others

	(1) Scrutiny by others	(2) Sanctioning by others	(3) Hypothetical	(4) Actual
Monitoring	0.070	-0.029	-0.051	-0.017
	(0.060)	(0.042)	(0.064)	(0.044)
Monitoring × Rules	0.037	0.015	0.199	-0.072
	(0.133)	(0.071)	(0.134)	(0.063)
$\beta_1 + \beta_2$	0.106	-0.014	0.148	-0.089
$\beta_1 + \beta_2$ p-value	0.398	0.856	0.270	0.211
Control mean	0.000	-0.001	4.388	-0.005
Lag dep. var.	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	1195	1206	1193	1206

Standard errors (clustered at the village level) in parentheses
* p<0.1, ** p<0.05, *** p<0.01

presented in the main text in terms of size. The estimated effect of SMS reminders on forest use is now statistically significant, albeit only at the 10% level.

Table C.6: H4: Compliance and forest use

	(1) Non-compliance	(2) Non-compliance 2	(3) Forest use
Monitoring	0.033*	0.034	-0.011
	(0.019)	(0.037)	(0.028)
Monitoring × Rules	0.067*	0.058	-0.098*
	(0.035)	(0.085)	(0.058)
$\beta_1 + \beta_2$	0.100	0.091	-0.110
$\beta_1 + \beta_2$ p-value	0.007	0.331	0.093
Control mean	-0.000	0.017	0.018
Lag dep. var.	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	1205	1265	1266

Standard errors (clustered at the village level) in parentheses
* p<0.1, ** p<0.05, *** p<0.01

D Results differentiating between SMS recipient and non-recipient households

This section presents estimates for the effect of the SMS reminder treatment, distinguishing between households in treatment villages that did and did not consent to receiving the treatment. As households could self-select into receiving the treatment, these results cannot be interpreted as causal. However, they do provide suggestive evidence on how effective an SMS reminder might be if the saturation was higher, or if the treatment was targeted explicitly at conditional cooperators.

We hypothesize that the effect of the SMS reminder treatment is stronger for those households in treatment villages that consented to receive the SMS messages. To test this, we estimate a second model which adds $\beta_3 \text{Monitoring}_j * \text{Rules}_j * \text{SMS recipient}_{ij}$ to estimating Equation 1. $\text{SMS recipient}_{ij}$ is a dummy variable equaling one if a household consented to receive SMS reminders and β_3 captures the effect of the treatment on SMS recipients compared to other households in the SMS reminder treatment villages. The linear combination of $\beta_1 + \beta_2 + \beta_3$ captures the joint effect of the community monitoring treatment on SMS recipients compared to pure control households. This linear combination and the corresponding p-values at the bottom of all results tables in this section. To maximize the number of

SMS recipients in the sample, all results are from a specification without control variables.

Knowledge of forest use rules is slightly higher among SMS recipients compared to non-recipients in the same village, but the coefficient is not statistically significant (Table D.7, column 1). Echoing the main results, SMS recipients do self-report significantly better knowledge of forest use rules (column 2). This different is quite large, amounting to 0.7 standard deviations. Objective knowledge of forest use rules does not increase more among SMS recipients (column 3).

Attentiveness to forest use rules, as measured by discussions, is higher among SMS recipients compared to their fellow village inhabitants (column 4 of Table D.7). The implied effect size, although it cannot be interpreted as causal, is large, at 0.6 standard deviations.

Table D.7: Knowledge and attentiveness - SMS recipients

	(1) Knowledge	(2) Self-reported	(3) Objective	(4) Attentiveness
Monitoring	0.110*	0.097	0.134	0.034
	(0.062)	(0.062)	(0.083)	(0.065)
Monitoring × Rules	0.239	-0.070	0.302	-0.066
	(0.148)	(0.112)	(0.190)	(0.126)
SMS recipient	0.086	0.700***	-0.083	0.623***
	(0.087)	(0.180)	(0.123)	(0.198)
$\beta_1 + \beta_2 + \beta_3$	0.434	0.728	0.353	0.591
$\beta_1 + \beta_2 + \beta_3$ p-value	0.011	0.001	0.131	0.004
Control mean	-0.031	0.000	-0.053	-0.000
Lag dep. var.	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	1205	1184	1205	1206

Standard errors (clustered at the village level) in parentheses
* p<0.1, ** p<0.05, *** p<0.01

SMS recipients are around 0.5 standard deviations more likely than non-recipients in the same village to report that they scrutinize or sanction others for breaking forest use rules (Table D.8). This difference holds across indicators for hypothetical and actual sanctioning. These results could imply that the SMS reminder treatment did induce recipients to intensify scrutiny and sanctioning of others. Alternatively, it may mean that households who consented to receive SMS messages are more likely to scrutinize and sanction others, even in absence of the treatment. This alternative explanation could hold if conditional cooperators are both more likely

to consent to the treatment and more likely to incur costs of sanctioning others at little direct benefit to themselves.

Table D.8: Scrutiny and sanctioning of others - SMS recipients

	(1) Scrutiny of others	(2) Sanctioning others	(3) Hypothetical	(4) Actual
Monitoring	0.015	-0.010	-0.015	-0.001
	(0.048)	(0.046)	(0.055)	(0.047)
Monitoring × Rules	-0.127	-0.108	-0.096	-0.129*
	(0.081)	(0.068)	(0.092)	(0.075)
SMS recipient	0.520***	0.507***	0.470***	0.579***
	(0.150)	(0.103)	(0.107)	(0.161)
$\beta_1 + \beta_2 + \beta_3$	0.408	0.390	0.359	0.449
$\beta_1 + \beta_2 + \beta_3$ p-value	0.004	0.000	0.000	0.011
Control mean	-0.029	-0.002	-0.001	-0.000
Lag dep. var.	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	1204	1206	1205	1206

Standard errors (clustered at the village level) in parentheses
* p<0.1, ** p<0.05, *** p<0.01

Table D.9: Scrutiny and sanctioning by others - SMS recipients

	(1) Scrutiny by others	(2) Sanctioning by others	(3) Hypothetical	(4) Actual
Monitoring	0.070	-0.029	-0.051	-0.017
	(0.060)	(0.042)	(0.064)	(0.044)
Monitoring × Rules	-0.034	0.005	0.165	-0.072
	(0.134)	(0.070)	(0.130)	(0.065)
SMS recipient	0.348***	0.050	0.166	-0.002
	(0.100)	(0.078)	(0.129)	(0.090)
$\beta_1 + \beta_2 + \beta_3$	0.384	0.026	0.280	-0.090
$\beta_1 + \beta_2 + \beta_3$ p-value	0.012	0.805	0.132	0.377
Control mean	0.000	-0.001	4.388	-0.005
Lag dep. var.	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	1195	1206	1193	1206

Standard errors (clustered at the village level) in parentheses
* p<0.1, ** p<0.05, *** p<0.01

SMS recipients furthermore report that their forest use is more closely scrutinized by others in the village (Table D.9, as compared to non-recipients. The implied effect size, although this is not causal, is around 0.35 standard deviations. SMS recipients to not perceive sanctioning to be statistically significantly more likely than non-recipients. This may be due to the omission of control variables, which weakens the results on perceived sanctioning by others also in absence of adding the interaction term (see section C). Alternatively, if those who already adhere to forest use rules self-select into the treatment, their fear of sanctioning might be low and not easily increased by SMS messaging.

There is no evidence that SMS recipients complied more with forest use rules, or that they use the forest less than their fellow village inhabitants. Recipients of SMS reminders are, if anything, marginally more likely break forest use rules, and their forest use is slightly higher (Table D.10). However, the respective coefficient estimates are not statistically significant and very small, in the range of 0.01 to 0.07 standard deviations.

Table D.10: Compliance and household forest use - SMS recipients

	(1)	(2)	(3)
	Non-compliance	Non-compliance 2	Forest use
Monitoring	0.033*	0.034	-0.011
	(0.019)	(0.037)	(0.028)
Monitoring × Rules	0.061*	0.044	-0.101
	(0.035)	(0.084)	(0.062)
SMS recipient	0.027	0.070	0.013
	(0.040)	(0.064)	(0.045)
$\beta_1 + \beta_2 + \beta_3$	0.122	0.148	-0.099
$\beta_1 + \beta_2 + \beta_3$ p-value	0.023	0.188	0.116
Control mean	-0.000	0.017	0.018
Lag dep. var.	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	1205	1265	1266

Standard errors (clustered at the village level) in parentheses
* p<0.1, ** p<0.05, *** p<0.01

E Deviations from the pre-analysis plan

Objective knowledge of forest use rules. The household survey asked open-ended questions on forest use rules. ("What are the rules or limits for harvesting a

product? To whom and when do the rules apply?" "What are the rules banning entry into the forest? To whom and when do they apply?"). It was not possible to operationalize these questions in a way that would be useful for the analysis, and hence they are not use in the knowledge index.

Attentiveness is measured through the normalized frequency of discussions about forest use rules. This measure was pre-specified as part of a more broadly defined salience index. The pre-specified index makes use of an additional survey question "Would you agree with the following statement: It is important that people stick to the communal forest use rules even if they suffer a shock?". Since the latter question measures the extent to which norms are internalized rather than attentiveness, the second questions is not used in the paper. However, the results for the salience index are presented in Column 4 of table B.2 in the appendix.

The paper distinguishes between hypothetical *sanctioning* and actual sanctioning for violations of forest use rules. This distinction was not pre-specified, but is important on conceptual grounds, since actual sanctioning can only happen once rules have been broken.

Forest use index: Several components of the forest use index were included in a slightly different form than was pre-specified. Specifically:

- The count variables measuring self-reported harvesting of forest product were highly skewed towards zero with long tails. In order to reduce the weight on outliers, the variables were log-transformed ($\ln(1 + x)$).

- We use a dummy variable to measure whether a household cleared part of the forest (Question Q111). Since very few households reported that they cleared any forest, questions Q112-Q114, measuring the land area that was cleared, were not used.

- Household level forest loss from satellite data, defined as forest loss around the point on the forest border closest to the household, was calculated using the closest point on the imputed forest border, not the actual forest border as pre-specified. It proved prohibitively expensive in practice to record the entire actual forest border by walking along it with a GPS device. Instead, the research team recorded 2 kilometers of the actual forest border and the remaining forest border was imputed using satellite data as follows. A pixel is considered to be part of a forest if within a radius of 25 pixels, at least

50% of pixels is forested. The forest border is the boundary of such a forest between the two outermost surveyed households that intersects with the recorded forest border.

Forest use index (village level): Several components of the forest use index at the village level were included in a slightly different form than was pre-specified. Specifically:

- Some count variables measuring measuring forest use along transects were highly skewed towards zero. To reduce the weight on outliers, these variables were log-transformed (ln(1+x)). This applies to the variables: number of kilns, number of animals grazing and number of branches cut per 100 meter of transect.

- Village level forest loss from satellite data was calculated in two rather than one way, as was pre-specified. It proved prohibitively expensive to record the entire village-forest border, to calculate forest loss in a 500 meter buffer around it, as originally envisioned. Instead, we calculate forest cover loss for: (a) a 500 meter buffer around the imputed village forest border (see above for imputation procedure) and; (b) forest loss in the area around the recorded forest border and transects. Both measures of forest loss are included in the village level forest loss index.

Control variables: The pre-analysis plan lists a number of variables which will be included as control variables if they are unbalanced between treatment and control group at the 10% level. Based on these rules, household cash income should have been included as a control variable. However, due to a large number of potentially non-random missing observations, this variable was not included.

Treatment implementation: Implementation of the treatment deviated from how it was pre-specified in the PAP in two ways. Firstly, while the PAP specifies that the implementing partner would recruit study participants (i.e. SMS recipients) from a list of the 10 households surveyed at baseline, and 10 further households, in practice all households within the community were invited to sign up for SMSs during the first community meeting. Households not on the original lists within the communities were not excluded from signing up to the treatment. Secondly, as sign-ups to receive the SMSs were lower than desired in the initial community meeting, community members were offered the choice to sign up to the treatment

during the implementing partner's midline visit (5-7 months after the beginning of the intervention, depending on the village). Hence, a proportion of the sample within each community received a weaker version of the intervention, as it covered fewer than 12 months' of SMSs.

Data collection: Data collection was different from pre-specified in the PAP in the following ways:

- Endline measurements were taken in November and December 2018, not August 2018 as pre-specified, to keep the time between baseline and endline data collection as close to 12 months as possible.

- In 6 villages no endline data could be collected in person, as local authorities refused the research team permission to enter. Instead, survey data was collected over the phone, but no on-the-ground measurements of forest use could be taken.

- In addition, 10 household surveys and two community surveys at endline were collected over the phone, as these were erroneously not collected in person due to a logistical error.

- The household survey differed between baseline and endline to remove questions that proved to have a high non-response rate and introduce some new questions. Were no baseline data was collected, this is indicated in Table 1 in the main text.