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Gender quotas increase the equality and effectiveness of climate policy interventions

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Interventions to reduce greenhouse gas emissions strive to promote gender balance so that men and women have equal rights to participate in, and benefit from, decision-making about such interventions. One conventional way to achieve gender balance is to introduce gender quotas. Here we show that gender quotas make interventions more effective and lead to more equal sharing of intervention benefits. We conducted a randomized 'lab'-in-the-field experiment in which 440 forest users from Indonesia, Peru and Tanzania made decisions about extraction and conservation in a forest common. We randomly assigned a gender quota to half of the participating groups, requiring that at least 50% of group members were women. Groups with the gender quota conserved more trees as a response to a 'payment for ecosystem services' intervention and shared the payment more equally. We attribute this effect to the gender composition of the group, not the presence of female leaders.

Human responses to climate change that ignore gender issues may miss important opportunities to increase intervention effectiveness and equality. In many societies, the governance of shared natural resources is traditionally a male-dominated decision-making process. The village groups that govern common-pool resources, such as forests, are often collectives in which women have limited voice and influence[1]. To address such inequities, governmental and nongovernmental organizations alike have begun to condition the implementation of conservation interventions on the villages' agreement to accept gender quotas that include more women in decision-making processes[2]. With the goal of enhancing gender equality, these quotas often fall short of establishing completely gender-balanced representation in decision-making processes. In many instances, quotas only require village groups to give women a minimal amount of representation; it is rare that intervention organizations ask local groups to go beyond a minimalist quota approach to require a majority of women in the decision-making group. Here we investigate what one can expect of more progressive measures. We specifically examine how a progressive gender quota—requiring at least 50% of group members to be women—affects the local group's response to a collective payment for ecosystem services (PES) intervention.

For policymakers who seek to reduce greenhouse gas emissions from tropical deforestation, collective PES programmes have come to represent an increasingly popular intervention[3–7]. Collective PES programmes, which target collectively owned forestlands as opposed to individually owned properties[6,8], are also receiving increased attention from scholars. There are at least two reasons for this shift. First, more and more of the world's remaining forest is owned collectively by communities of local forest users[4,6,9,10], making collective PES a viable climate policy instrument for this growing category of forestland owners[3]. Second, there is a widespread perception among implementing organizations that large collective PES contracts may incur lower transaction costs compared to the establishment of a large number of small, individual PES agreements[6,11].

The existing research on collective PES focuses largely on the collective-action problem that is created when a group of resource users must coordinate their conservation behaviour to comply with the PES agreement[6]. Users may face strong temptations to free-ride on the conservation efforts of others. There are some recent experimental studies of collective PES that focus on conservation outcomes and the likelihood of users overcoming this collective-action problem[3,4,7]. However, existing studies have paid less attention to the problem created when local user groups are in charge of distributing the payments to group members: PES benefits, when distributed to community members through a local governance institution, may be shared unequally, and little is known about the local institutional factors that either allow or alleviate inequalities in PES benefit sharing. Inequality in benefit sharing is one of the most widely discussed institutional failures of communal-resource management by local user groups[12–15], and it is unlikely that collective PES programmes will be immune to this problem. Whereas local institutions for collective action are seen as a necessary condition for the success of collective PES[6], little is known about how specific institutional characteristics condition PES outcomes, especially with regard to inequality. The main contribution of our research is to examine how a gender quota—an institutional feature of local governance arrangements that is popular in many developing countries[2,16,17]—affects equality and effectiveness of collective PES arrangements.

Empirical studies confirm that women and men often have distinct policy preferences[18–21] and that gender quotas lead to a shift in focus toward the policy issues that are favoured by women[19,22,23]. Work in behavioural economics suggests that women have a stronger preference for equality than men do. Specifically, the dictator game, which asks a subject to divide a pot of money between him- or herself and another participant, is commonly used as a behavioural measure of inequality aversion[24]. Results from such games show that women are willing to share up to twice as much of the pot as men[25–28]. A meta-analytic study of dictator games confirms that, controlling for other factors, women tend to be more averse to inequality than men[28]. Thus, if a gender quota leads to decisions more in line with women's policy preferences, we expect that it will result in a more equal distribution of PES programme benefits.

Previous evidence about the effect of gender composition on environmental conservation has been inconclusive[29–31]. For example, public-good games in the laboratory have shown that women

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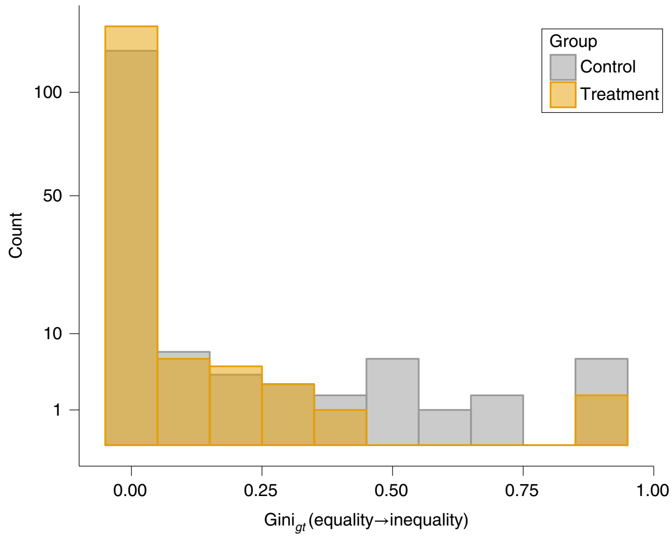

Fig. 1 | Distribution of Gini coefficients for the PES payment. Each observation represents the inequality in the distribution of an individual payment among eight participants in the treatment ($n=28$ groups) and control ($n=27$ groups) conditions. Higher values represent higher inequality, with 0 representing perfect equality.

Table 1 | Estimated effects of gender quota on time-varying group-level outcomes

Dependent variable	Model 1	Model 2
	Gini$_{gt}$	Harvest$_{gt}$
Gender quota$_g$	−0.049 (−0.081, −0.016)	4.897 (3.580, 6.214)
	$z=-2.947$	$z=7.288$
	$P=0.004$	$P<0.001$
PES$_t$		−9.519 (−11.045, −7.992)
		$z=-12.221$
		$P<0.001$
Gender quota$_g$ × PES$_t$		−5.717 (−7.811, −3.624)
		$z=-5.353$
		$P<0.001$
Constant	0.082 (0.054, 0.111)	24.630 (23.749, 25.510)
	$z=5.736$	$z=54.828$
	$P<0.001$	$P<0.001$
n	304	879

Estimates from linear mixed-effects models with heteroscedasticity-robust 95% confidence intervals in parentheses. Unit of analysis is group round for both models, with the dependent variables measured for each group (g) at each round (t). Model 1 includes observations from game rounds 9–16. Model 2 includes observations from rounds 1–16. Both models include random intercepts at the group level. Further details on the modelling approach are given in the Methods.

behave differently in groups with particular gender compositions, contributing more to public goods in groups with more female participants[32,33]. Similarly, in a common-resources game, gender composition affects results, not through individual gender differences, but rather through group dynamics[34]. The behaviour of women in such games is also more likely to vary because of differences in experimental context[35,36]. In field settings, some studies have reported that female-majority groups manage to achieve better resource-conservation outcomes[37,38], while other studies working on similar outcomes have reported the opposite, attributing this negative conservation outcome to insufficient economic resources to allow women to be effective[39,40].

A compelling explanation for these seemingly contradictory findings is that while women do have different preferences and behaviours than men in common-pool resource scenarios, these differences do not always affect conservation outcomes, especially when existing conditions are disempowering to women[2]. If resource users face harsh constraints, preferences may not translate into behaviour. This suggests that gender quotas alone may be insufficient to improve conservation outcomes, but when progressive gender quotas are combined with other supportive interventions, we can expect a positive relationship between female-majority groups and conservation outcomes. In that sense, PES programmes may provide a supportive financial incentive that helps to overcome opportunity costs that may otherwise prevent women from being effective in some forest-conservation settings. On the basis of this logic, we expect that when progressive gender quotas—quotas that reserve at least half of the seats at the decision-making table for women—are combined with other supportive interventions such as a collective PES programme, this combination will produce increased conservation behaviour relative to either intervention operating in isolation.

To test our hypotheses, we conducted a framed field experiment with 55 groups of 8 participants each, in 31 villages near collectively managed forests in Indonesia, Peru and Tanzania (Methods). The experiment was a modified version of the common-pool resource-appropriation game[41] and it framed the resource as a shared forest common. We told participants that an external organization had offered to make a payment to the group if they did not cut any trees from the forest and that they would be imperfectly monitored based on the number of trees cut in each round. Participants were told that the group would need to elect a group member at the beginning of each round to serve as the 'group leader' for that round and that the leader would decide how much of the PES payment would be given to each group member. Groups of participants from each village were randomly assigned to either the treatment group ($n=28$ groups), which had a gender quota ensuring that at least 50% of the final list of eight participants were women, or the control group ($n=27$ groups), for which our field protocol did not manipulate the gender composition of each group of eight volunteers (leading to an average of 33% women in the groups assigned to control). The game lasted for 24 rounds for each group and the collective PES intervention was presented to the participants during rounds 9–16. See Methods for a more detailed discussion of the experiment, Supplementary Note 1 for further details on the gender differences between the treatment and control groups and the Supplementary Methods for the field protocol.

Although we present the results using linear mixed-effects regression models for ease of interpretation (Table 1), our inferences are robust to a generalized linear modelling approach that is appropriate for the way the two dependent variables are distributed (Methods, Supplementary Note 4 and Supplementary Table 5). We also present easily interpretable, nonparametric within-village comparisons as descriptive summaries of the differences between treated and control groups. See Methods for details of these estimations.

Effect of the gender quota on PES distribution

We calculated a Gini coefficient representing inequality in the distribution of each PES across the eight participants in each group. Figure 1 shows these raw data. Groups randomly assigned to the gender-quota treatment distributed the PES more equally than groups assigned to control, as evidenced by the lower Gini coefficients of the treated groups. A clear majority of payments that were distributed with Gini coefficients greater than 0.25 belong to the control group. Additionally, Fig. 1 shows that most payments were distributed perfectly equally. We also computed inequality in the

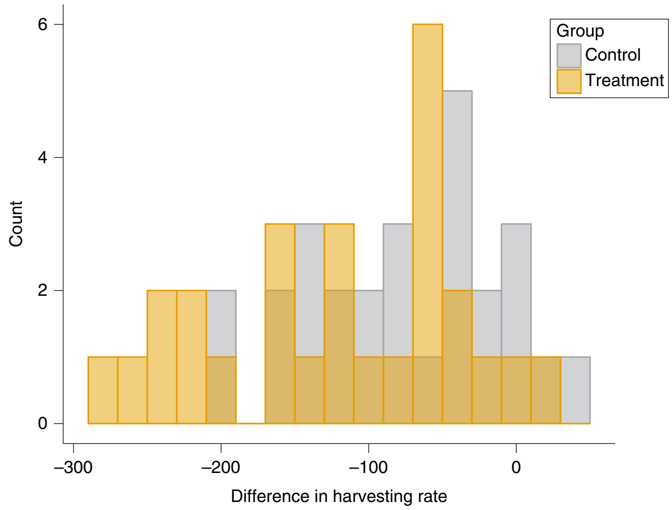

Fig. 2 | Difference in total group harvests between PES and pre-PES rounds. Each observation represents the difference between the total harvest in rounds 9–16 (PES rounds) and the total harvest in rounds 1–8 (pre-PES rounds) for a single group of eight participants in the treatment ($n = 28$ groups) and control ($n = 26$ groups) conditions.

total distribution of the PES in each group throughout the entire course of the game and compare pairs of groups within villages, each of which contains one group assigned to the gender-quota treatment and one group assigned to control. Groups randomly assigned to the gender-quota treatment had overall Gini coefficients that were 0.041 lower on average than the corresponding group without the gender quota in the same village (95% bias-corrected and accelerated (BCA) confidence interval = −0.0100, −0.0045). Table 1 shows the estimated treatment effect of the gender-quota treatment on the Gini coefficient of group g at round t, estimated using a linear mixed-effects regression model. These results suggest that groups treated with a gender quota had an average time-varying Gini coefficient that is estimated to be just under half of that in the control group (estimated treatment effect coefficient = −0.049; 95% confidence interval = −0.081, −0.016; $z = -2.947$; $P = 0.004$).

Effect of the gender quota on conservation

Figure 2 shows, for each group, the difference between the total harvest in the PES rounds and total harvest in the pre-PES rounds. The vast majority of groups harvested fewer trees once the PES was offered, but reductions were larger in the groups with the randomly assigned gender-quota treatment. All of the groups that harvested at least 200 fewer trees in the PES rounds than in the pre-PES rounds belonged to the treatment group. Comparing the magnitude of this reduction between groups assigned to treatment and those assigned to control in the same village, we found that groups assigned to the gender-quota treatment reduced their harvests in the PES rounds by an average of about 42 more trees than the corresponding group without the gender quota in the same village (95% BCA confidence interval = −77.59, −7.62). A linear mixed-effects model of the time-varying group harvesting rate also suggested that the groups with the gender quota changed their harvesting patterns more substantially in response to the PES, compared to the control groups (Table 1). Although the groups without the randomly assigned gender quota harvested about 9.5 fewer trees per round on average during the PES rounds compared to the pre-PES rounds (a reduction of 39%), the estimate on the interaction term suggests that the groups with the quota reduced their harvests by an additional 5.7 trees per round beyond the baseline reduction found in the groups assigned to control (95% confidence interval = −7.811, −3.624; $z = -5.353$; $P < 0.001$). In other words, the groups with the gender quota harvested more than 15 fewer trees per round during the PES rounds compared to the pre-PES rounds (a reduction of about 51%).

The estimated coefficient of the treatment indicator in a model that included observations from all PES and pre-PES rounds (model 2) is positive and statistically significant. While this initially suggests that groups with the gender quota harvested more than the control groups during the pre-PES rounds, this result is not robust to additional tests. We reran the same analysis using an overdispersed Poisson regression and found that the difference in harvesting rates between treatment and control groups in the pre-PES rounds is not statistically significant (Supplementary Note 4 and Supplementary Table 5).

Figure 3 shows time-varying Gini coefficients and group harvests, averaged at each round across groups. During the PES rounds, Gini coefficients for the distribution of the PES payments start at roughly the same average for groups with and without the randomly assigned gender-quota treatment (Fig. 3a). The average Gini coefficient shows an upward trend over time for the groups assigned to control, while it stays relatively flat for the groups assigned to treatment. Although groups assigned to treatment appeared to harvest more than groups assigned to control in the pre-PES rounds, treated groups also reduced their harvests more once the PES was offered, achieving lower average harvesting rates than the control groups in several of the PES rounds (Fig. 3b).

Mediation by group gender composition

Groups receiving the gender-quota treatment awarded more leadership positions to women (Supplementary Note 1 and Supplementary Fig. 2), and causal mediation analysis (Methods) allowed us to investigate whether female leadership mediated the effect of the treatment on the time-varying Gini coefficients. Results of causal mediation analysis show no evidence for a mediating effect of female leadership and suggest that the treatment from model 1 is probably mediated only by the overall gender composition of the group. The estimated average causal mediation effect (ACME) for the gender of the leader of group g at round t is not statistically significant at conventional levels (ACME = 0.012; 95% confidence interval = −0.032, 0.002; $P = 0.119$). By contrast, the estimated ACME for the proportion of women in the group (ACME = −0.067; 95% confidence interval = −0.120, −0.016; $P = 0.008$) is negative, statistically significant, and roughly equal in size to the estimated total effect of the treatment from the mediation model (estimate = −0.062; 95% confidence interval = −0.129, −0.0006; $P = 0.048$). The average direct effect (ADE), which can be interpreted as the share of the treatment effect that is not accounted for by the proportion of women in the group, is substantively small and does not reach statistical significance (ADE = 0.005, 95% confidence interval = −0.046, 0.054; $P = 0.848$).

Given that gender composition appears to mediate the effect of the gender-quota treatment on inequality in the distribution of the PES (whereas leader gender does not), it would make sense that female participants—who tend to have stronger preferences for equality[24–28]—should be signalling their preferences to the leader to influence the leader's distribution decisions. Perhaps the most obvious way that this could be occurring is through open communication between rounds, as allowed by our game design. We find that women were indeed responsible for a much larger share of communication in the groups randomly assigned to the gender-quota treatment (Supplementary Note 1, Supplementary Fig. 3 and Supplementary Table 1).

Conclusion

In the search for climate interventions that can achieve the greatest improvements in outcomes at the lowest cost, analysts and policymakers face difficult choices. Policy analyses often assume

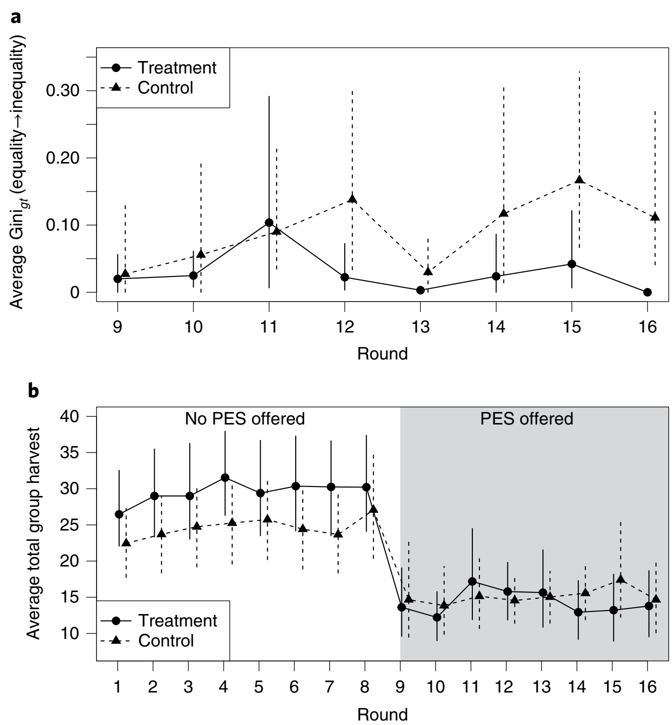

Fig. 3 | Average $Gini_{gt}$ and harvesting rate at each round. **a**, Average $Gini_{gt}$ for the PES payment distribution across PES rounds for the treatment ($n = 28$ groups) and control ($n = 27$ groups) conditions. Higher values of the $Gini_{gt}$ coefficient represent higher inequality. **b**, Average harvesting rate in pre-PES (rounds 1–8) and PES (rounds 9–16) rounds in the treatment and control conditions. Observations are averaged at each round across groups included in the main analysis. 95% BCA confidence intervals are shown. Confidence intervals were omitted for $Gini_{gt}$ in the treatment condition in round 16 as they did not vary.

that gender quotas can make policy interventions more equitable and just, but not necessarily more effective. As a result, policymakers may overlook progressive gender quotas as a viable means to increase both the equality and effectiveness of climate policy interventions.

Our preliminary finding that gender quotas can help climate policy interventions to bring about increases not only in equality, but also in programme effectiveness raises questions about the reasons behind such effects. One possibility is that the stronger environmental preferences of women are more easily achieved under the additional support of PES, or it could be because women are more disposed to rule compliance than men[42]. Either way, it is clear that groups with the gender quota changed their harvesting behaviour in response to the PES more strongly than the groups without the quota. In other words, a progressive gender quota may be an institution that helps local user groups to resolve the problems of free-riding and skewed distribution of programme benefits that plague many forest conservation programmes.

Although our randomized experiment allows us to overcome the selection bias problems that hamper many existing observational studies of PES and other policy instruments[43], one limitation of our study is the potential lack of external validity afforded by a behavioural experiment. We took several measures when designing the experiment to augment its external validity, such as conducting the experiment with actual forest users who are potential targets of collective PES interventions (rather than undergraduate students)[44,45], framing the experiment to resemble the situations that participants face on a regular basis (Supplementary Methods) and using cash incentives to mimic the incentive structures inherent to collective PES arrangements. These efforts notwithstanding, it is important to keep in mind that a framed experiment such as the one described here can never fully capture the complexity of the decisions that forest users face in real-world situations. Furthermore, although we performed this experiment in multple different countries with users in villages that resemble typical targets for PES, our sample is not adequate to rigorously test how gender quotas might have stronger (or weaker) effects in the context of different countries.

Even with the extensive discretionary powers given to leaders in this experiment, the overall gender composition of a local forest governance institution may be more important than the promotion of women into executive leadership positions, at least for the distributional outcome that we examine in this study. Studies such as the one described here are especially policy-relevant during a time when governments are experimenting with policy interventions that mandate female representation on local governance councils as well as executive female leadership within those councils, both within and beyond the forestry sector[46,47]. Thirty per cent or one-third have been common cut-offs for gender quota policies in legislatures, for example in India[46], Argentina[48] and Rwanda[22]. The 30% minimum emerged as a prescriptive norm in 1995 at the Fourth World Conference for Women in Beijing[49]. Our results demonstrate that there is potentially even greater benefit when quotas are progressively set at full gender parity. There is an emerging trend in this direction in some Latin American countries[50], and another notable example is community forestry in Nepal, which has stipulated since 2009 that at least 50% of local forest user committee members should be women[51].

In addition to the potential benefits to climate interventions outlined in this study, there are important normative reasons to seek greater inclusion of women in the local institutions that have historically reflected or even reinforced local gender inequalities[2]. At the same time, a quota does not necessarily ensure that the right people are in the decision-making group, and other measures may be necessary to ensure that a quota is not met by, for example, stacking the group with female relatives of pre-existing leaders. Furthermore, for any of the supposed benefits of policy interventions, such as gender quotas, to be fully realized, we must reckon with the fact that rural women often face discrimination even when formal institutions give them a seat at the table. Deeply entrenched gender disparities mean that the active participation of women in actual decision-making is often limited, even if they are allowed to formally take part in the decision-making process[37]. While we provide preliminary evidence that suggests that quotas do indeed matter for two important outcomes related to collective PES programmes, understanding how to include women in local climate policy interventions meaningfully and effectively is one of the most important directions for future research.

Online content

Any methods, additional references, Nature Research reporting summaries, source data, statements of data availability and associated accession codes are available at https://doi.org/10.1038/s41558-019-0438-4.

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Author contributions
K.P.A. conceived the project, N.J.C. and T.G. developed the analytical approach, N.J.C. analysed the data, and K.P.A., T.G. and N.J.C. wrote the paper.

Competing interests
The authors declare no competing interests.

Additional information
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Methods

Framed field experiment. We used a framed field experiment with local forest users ($n=440$) from 31 rural villages in Indonesia, Peru and Tanzania[3]. Our experimental design used a modified version of the traditional common-pool resource appropriation game[41], which captures how the users of common-pool resources behave in a collective-action scenario related to the use of the shared resource. Participants played the game in groups of eight, and our framing described the experiment as a decision-making activity in which participants would make decisions about harvesting from a shared local forest. The treatment and control groups in each village played the game sequentially; however, in each village our field teams randomized the order of implementation. Across the treatment and control conditions, the basic structure of the game was designed so that participants faced trade-offs between collectively conserving the resource in pursuit of a shared PES incentive and free-riding to profit from the resource at the expense of other group members who contributed to its conservation.

The experiment proceeded in three stages of eight rounds each. During the first and third stages (the pre-PES and post-PES stages), participants decided anonymously how many trees they wanted to harvest from the resource, knowing that they would earn five tokens for each tree that they harvested. To simulate the ecosystem services that a conserved forest provides, which are typically shared by forest users as a public good, we also awarded participants one token for each tree left standing in the forest at the end of each round. Although participants were encouraged to ask questions to the moderator about the activity, they were not allowed to communicate with one another during the pre-PES and post-PES stages.

During the second stage (the PES stage), we implemented a payment scheme that was meant to simulate a collective PES programme, and participants were allowed to communicate openly with one another between each round. We retained the payoff structure described above for the pre-PES and post-PES stages, but also told participants that an external organization had offered to make an additional payment to the group if members of the group did not cut any trees from the forest. We told participants that because the external organization could not perfectly monitor whether they had conserved the forest, they would be monitored with some probability based on the number of trees cut at each round. If the group did not cut any trees, the payment would automatically be awarded in that round. The probability of receiving the payment would decrease linearly as harvesting increased beyond zero trees, and the external organization would not give the payment at all in a given round if harvesting exceeded half the number of trees in the forest in that round. Participants were told that the group would need to choose a group member at the beginning of each round to serve as the 'leader' for that round, and that the leader would decide how much of the PES payment (160 tokens for the entire group) should be given to each group member. In other words, it would be entirely up to the leader to choose how the PES payment would be distributed if it was paid by the external organization in a given round. In this way, the leader distributing the bonus faced a choice that mirrored that of the 'multiple-recipient dictator game'[52], in which the behaviour of the dictator was found to be similar to that in a two-player dictator game.

Across all stages of the game, the Nash strategy was for a participant to harvest the maximum of ten trees at each round whereas the Pareto-optimal strategy was to harvest nothing. Full game equilibria have been described previously[3]. Participants were given monetary compensation at the end of the experiment in local currency. These payoffs were proportional to the number of tokens earned by the participant, and were offered so that game decisions had tangible financial implications for the participants. See Supplementary Methods for the field protocol.

We performed the experiment in 31 villages in Indonesia, Peru and Tanzania. Because we wanted the participants to resemble the typical target populations of collective PES programmes, we selected these villages from the existing study sites of the Center for International Forestry Research. Our sampling strategy, participant recruitment methods and randomization procedures have been described previously[3]. Each of the villages in our sample had a pair of groups, with one group randomly assigned to the treatment condition and one assigned to the control condition. Out of the 31 villages, seven villages did not have a full pair due to either time constraints in the field or a lack of enough volunteers for the experiment. However, the assignment of treatment or control status was still done randomly for the single groups in these seven villages. Although the game design was exactly the same across the treatment and control groups, the treatment groups had a gender quota ensuring that at least 50% of the participants in the group were women, whereas our field protocol did not manipulate the gender composition of each group of eight volunteers under the control condition (see Supplementary Note 1 and Supplementary Fig. 1 for details of the gender composition under the treatment and control conditions). The randomly assigned treatment in this experiment is therefore the gender quota, with all other features of the game—leadership, communication and the PES offer—administered uniformly across all groups in the experiment during rounds 9–16, regardless of whether the group was assigned to treatment or control status. The quota were implemented by the field team during the recruitment and randomization process, and the gender quota were not discussed with the participants. Our main reason for not announcing to participants of the treatment groups that they were in a gender-quota treatment group was to reduce the risk for inviting biased behavioural adjustments by the participants, referred to by cognitive psychologists as 'demand characteristics'[53], as a response to our presence during the experiment. This is a situation in which we, by making such an announcement, might signal to the participants what we consider to be appropriate participant behaviour and thus create an implicit demand for the participants to be 'good participants' and behave in the way they believe that we expect them to. By minimizing the use of labels and the amount of information conveyed with regards to this treatment, we sought to reduce such bias[53].

Supplementary Table 6 shows basic descriptive statistics for the participants in our experiment. Tests of participant-level balance on pre-experiment characteristics, such as age, education and wealth do not suggest any statistical differences between participants in groups assigned to treatment and those assigned to control (other than gender; Supplementary Note 6 and Supplementary Table 7). Furthermore, we replicate the results of the main analyses presented in Table 1 while controlling for group-level averages on a number of key pre-activity participant characteristics (Supplementary Table 8). Although our analysis presented in Supplementary Table 1 suggests that groups assigned to the gender-quota treatment were more likely to choose female leaders, we neither encouraged nor required them to do so. Supplementary Notes 2, 3, Supplementary Figs. 4–6 and Supplementary Tables 2–4 describe additional dynamics of the game, beyond those presented in the results.

Analytic methods. *Nonparametric descriptive comparisons.* We hypothesized that groups randomly assigned to the gender-quota treatment would distribute the PES more equally than groups assigned to control. We also hypothesized that although a group assigned to either treatment or control should reduce its harvesting rate when the PES is offered, groups randomly assigned to treatment should reduce their harvesting rates more strongly than groups assigned to control. Although we use regression modelling to present our results, we first showed the differences between treated and control groups using aggregate-level nonparametric comparisons. This method accounts for the paired nature of the data by making comparisons within village pairs. The seven 'unpaired' groups were therefore omitted from the comparisons, in addition to a within-village pair that was omitted because of missing data.

For each group of eight participants, we calculated a Gini coefficient that represented the inequality in the distribution of the PES among the eight participants in the group during all rounds in which a payment was made to the group. We then calculated the absolute difference in the Gini coefficient between each group assigned to treatment and the corresponding group assigned to control in the same village. The mean of this within-village difference has a simple, intuitive interpretation, as it represents the average difference in inequality between a group with a gender quota and a group in the same village without a gender quota. We present this mean in the results, along with 95% BCA confidence intervals, as a nonparametric and easily interpretable summary of the differences between treated and control groups in the same village with respect to the inequality outcome.

We present a similar summary of differences in harvesting responses to the PES between treated and control groups. For each group of eight participants, we calculated the absolute difference between the total number of trees harvested during rounds 9–16 (when the PES was offered) and the total number of trees harvested during rounds 1–8 (before the PES was offered). This is a measure of the group's response to the PES. We then calculated the absolute difference in this response between each group assigned to treatment and the corresponding group assigned to control in the same village, presenting the mean of this absolute difference (with 95% BCA confidence intervals) in the results.

Repeated-measures approach and mixed-effects modelling. Because our experiment generated multiple observations per group (one at each round), we also present the results using a repeated-measures approach based on regression modelling[54]. The unit of analysis for both of our models presented in Table 1 is group round. In other words, we calculated inequality in benefit sharing as well as total harvesting rates for each group at each round of the game, treating each round as an individual observation. Both models account for dependencies among observations with random intercepts at the level of the group (represented by v_g in the equations below). Additionally, our results are robust to the inclusion of country fixed effects. Although we present linear mixed-effects models for ease of interpretation, Poisson models are more theoretically justified for our dependent variables (the harvesting variable is a count and the Gini variable approximates a Poisson distribution, as shown in Fig. 1). We present the same analysis in Supplementary Table 5 (Supplementary Note 4) using overdispersed Poisson regression models and find the same results.

Model 1 only uses observations from the PES rounds (rounds 9–16) for all groups. The dependent variable in model 1 is a Gini coefficient of the distribution of the PES among the eight participants in group g at round t. Groups that did not receive the monetary payment in one or more rounds are omitted from the model in those rounds. The regression model is represented by equation (1). The estimate of the dichotomous treatment indicator (β_1) in this model can be interpreted as the estimated effect of the gender-quota treatment on the time-varying Gini coefficient.

$$\mathrm{Gini}_{gt} = \alpha + \beta_1 \mathrm{quota}_g + v_g + e_{gt} \quad (1)$$

Model 2 uses observations from the PES rounds (rounds 9–16) as well as the pre-PES rounds (rounds 1–8) for all groups. The dependent variable in model 2 is the total number of trees cut by participants in group g at time t. The estimate of the PES indicator (β_2) can be interpreted as the average difference in total per-round harvests between the PES rounds and the pre-PES rounds for groups assigned to the control condition. The estimate of the interaction term (β_3) can be interpreted as the difference in the effects of the PES between groups with the gender quota and those assigned to control. The estimate of the treatment indicator (β_1) can be interpreted as the effect of the gender-quota treatment on pre-PES harvesting rates.

$$\begin{aligned}\mathrm{harvest}_{gt} &= \alpha + \beta_1 \mathrm{quota}_g + \beta_2 \mathrm{PES}_t \\ &\quad + \beta_3 \mathrm{quota}_g \times \mathrm{PES}_t + v_g + e_{gt}\end{aligned} \quad (2)$$

Mediation analysis. Because the results of model 1 suggests that the gender quota reduced inequality, we propose two possible mediating pathways through which this difference could arise: the proportion of women in group g (which was designed to be higher under the treatment condition compared to the baseline control condition) and the gender of the leader selected by group g at round i (which was entirely under the control of the participants, but which tended to be less skewed toward males under the gender quota (Supplementary Table 1 and Supplementary Fig. 2).

We used causal mediation analysis[55] to separately estimate the mediating effects of both of these variables on the differences in inequality between groups assigned to the treatment and control conditions. We regressed the two mediators on a treatment indicator (using logistic regression for the gender of the leader and linear regression for the proportion of women in the group), regressed the Gini coefficient on each mediator separately while controlling for treatment assignment (using ordinary least squares analyses) and computed the ACME for each mediator through the method described previously[55]. Whereas the gender of the leader does not appear to significantly mediate the effect of treatment assignment on the Gini coefficient, the proportion of women in the group does (as explained in the results). We present the estimated ACME for the proportion of women in the group, the ADE and the total effect, with confidence intervals computed to account for clustering at the level of the group.

Equations (3) and (4) show the regression models used to estimate the ACME for the proportion of women in the group.

$$\text{Proportion of women}_g = \alpha + \beta_1 \mathrm{quota}_g + e_{gt} \quad (3)$$

$$\mathrm{Gini}_{gt} = \alpha + \text{Proportion of women}_g + \beta_1 \mathrm{quota}_g + e_{gt} \quad (4)$$

Equations (5) and (6) show the regression models used to estimate the ACME for the gender of the leader at time t.

$$\Pr(\text{Female leader}_{gt} = 1) = \mathrm{logit}^{-1}(\alpha + \beta_1 \mathrm{quota}_g) \quad (5)$$

$$\mathrm{Gini}_{gt} = \alpha + \text{Female leader}_{gt} + \beta_1 \mathrm{quota}_g + e_{gt} \quad (6)$$

Although we use linear models to estimate equations (4) and (6) for ease of interpretation, the causal mediation analysis produces the same results when overdispersed Poisson error models are used.

Ethics statement. The protocol for this study was reviewed and approved by the Institutional Review Board at the University of Colorado at Boulder, approval number 13-0198.

Reporting Summary. Further information on research design is available in the Nature Research Reporting Summary linked to this article.

Data availability

The datasets generated and analysed during the current study are available from the corresponding author on reasonable request.

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Software and code

Policy information about availability of computer code

Data collection	N/A
Data analysis	Data analysis was performed in R (version 3.4.4) using the standard base R packages and the following packages: lme4, mediate.

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Behavioural & social sciences study design

All studies must disclose on these points even when the disclosure is negative.

Study description	This is a quantitative study analyzing the results of a lab-in-the-field experiment related to forest use decisions.
Research sample	The research sample includes 224 rural villagers in Indonesia, Tanzania, and Peru. Participants were 55% female with an average age of 39 years. The sampled villages were selected to resemble typical villages that would receive payments for ecosystem services (PES) interventions.
Sampling strategy	We recruited our sample from study sites associated with the Global Comparative Study on REDD+ led by the Center for International Forestry Research (CIFOR) in Indonesia, Peru, and Tanzania. By choosing our study sites from these samples of villages, we were also able to utilize existing relationships with local partner organizations through CIFOR in order to ensure good working relationships between our local partners and research subjects. We purposively sampled villages from these sites to achieve reasonable variation with respect to two community-level variables: poverty and distance to markets. We enlisted the help of community leaders to announce our research activity ahead of time, and encouraged our local research partners to ensure as broad participation as was possible with respect to gender and ethnicity. On the day of the activity in each village, participants self-selected into the experiment by arriving at our pre-arranged research location. We subsequently randomly assigned these participants into treatment and control groups.
Data collection	All data used in the paper were collected on paper. Forest user decisions were first recorded on decision cards by the participants themselves. The field staff checked the cards to make sure they were filled out correctly in each round of the experiment. Our field staff then entered all decision data into an excel spreadsheet. In addition, two independent observers documented participant behavior in each round of the experiment. Field staff also interviewed each participant individually before and after the experiment. The interview responses were entered on a paper questionnaire and later entered into an excel spreadsheet.
Timing	All field data used in the paper were collected between January 2014 and November 2014.
Data exclusions	One group of eight participants was excluded from this analysis because, due to errors in data entry, treatment assignment cannot be determined for this group.
Non-participation	Participation in the experiment was voluntary. None of the participants who enrolled in the experiment decided to drop out.
Randomization	We divided volunteers from each village into two groups, and randomly assigned one of two conditions to each of the groups involved in our experiment. Under the treatment condition, the group had a gender quota ensuring that at least 50% of the final list of participants were women.

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Materials & experimental systems

n/a	Involved in the study
☒	☐ Antibodies
☒	☐ Eukaryotic cell lines
☒	☐ Palaeontology
☒	☐ Animals and other organisms
☐	☒ Human research participants
☒	☐ Clinical data

Methods

n/a	Involved in the study
☒	☐ ChIP-seq
☒	☐ Flow cytometry
☒	☐ MRI-based neuroimaging

Human research participants

Policy information about studies involving human research participants

Population characteristics	We recruited our sample from study sites associated with the Global Comparative Study on REDD+ led by the Center for International Forestry Research (CIFOR) in Indonesia, Peru, and Tanzania. By choosing our study sites from these samples of villages, we were also able to utilize existing relationships with local partner organizations through CIFOR in order to ensure good working relationships between our local partners and research subjects. We purposively sampled villages from these sites to achieve reasonable variation with respect to two community-level variables: poverty and distance to markets. We enlisted the help of community leaders to announce our research activity ahead of time, and encouraged our local research partners to ensure as broad participation as was possible with respect to gender and ethnicity. On the day of the activity in each village, participants self-selected into the experiment by arriving at our pre-arranged research location. We subsequently randomly assigned these participants into treatment and control groups.
Recruitment	We enlisted the help of community leaders to announce our research activity ahead of time, and encouraged our local research

Recruitment	partners to ensure as broad participation as was possible with respect to gender and ethnicity. On the day of the activity in each village, participants self-selected into the experiment by arriving at our pre-arranged research location. Participants were organized into groups of eight individuals based on the time of their arrival at the research location and family membership (we made sure individuals from the same family did not participate in the same group).
Ethics oversight	The field-based data collection activities for this study followed a research protocol for human subjects that was reviewed and approved by the Institutional Review Board (IRB) at the University of Colorado at Boulder (Study# 13-0198).

Note that full information on the approval of the study protocol must also be provided in the manuscript.