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Experimental evidence on payments for forest commons conservation

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Payments for Ecosystem Services (PES) represent a popular strategy for environmental protection, and tropical forest conservation in particular. Little is known, however, about their effectiveness. Many argue that even if PES increase conservation while payments last, they may adversely affect other motivations for pro-environmental behaviour in the longer term. We test whether conditional payments also encourage forest users to conserve shared forest resources after payments end. Using a framed field experiment with 1,200 tropical forest users in five countries, we show that (1) during the intervention, conditional payments increased conservation behaviour; (2) after payments stopped, users continued to conserve more on average than they did before the intervention, especially when they were able to communicate with each other; and (3) trust amplified the lasting conservation effects of the interventions. PES effectiveness may increase when interventions facilitate interpersonal communication and when implemented in contexts where forest users enjoy high levels of trust.

In the search for more effective environmental policies to prevent loss of tropical forests and to enhance the conservation of biological diversity, PES have become an increasingly popular instrument among policymakers[1–3]. Applied to tropical forests, PES rely on conditional payments that seek to induce increases in forest conservation behaviour among forest users and are often promoted as a complement to more traditional conservation interventions, such as the establishment of protected areas and community-based conservation programmes[4,5].

Despite the increased popularity of PES programmes, there is limited scientific evidence on the extent to which, and under what circumstances, these interventions prompt changes in conservation behaviour[2,6,7]. Even less is known about how PES affect forest users' collective actions to conserve their forest commons[8–10]. This is a serious limitation since an increasing proportion of the world's remaining forests are owned and managed collectively by rural communities[11].

Recent studies warn that PES interventions may do more harm than good in the long term—there is the potential for such interventions to undermine and even destroy people's pre-existing motivations to conserve forests[12–14]. Several studies have shown that monetary incentives can, in some contexts, discourage the very behaviour they are intended to promote[15–17]. The observed displacement of non-monetary motivations in these studies carries a worrisome implication for long-term PES outcomes: when monetary incentives are removed there may be a net decrease in conservation behaviour (for a review of this literature, see the Supplementary Information, Section A).

Empirical studies that have assessed the effects of PES interventions are hampered by several limitations (Supplementary Information, Section A). First, few studies have produced experimental evidence and fewer still have looked at the behavioural responses of the actual forest users in developing countries. Second, most studies have been based on small sample sizes ($n < 250$ individuals), in a small number of sites ($n < 10$ villages), in just one or two countries. Third, the analysis of postintervention behaviour appears to be absent from most of the PES literature. Finally, few studies have analysed empirically how particular contextual conditions, such as user characteristics, might increase the prospects for PES success[18].

This paper makes two novel contributions to the literature on PES. Our findings add robust new evidence on how PES interventions affect forest user decision-making about the conservation of forest commons in a wide variety of tropical forest contexts, including how behaviour may change after a PES intervention stops. This study also explores the ways in which trust among local forest users affects the prospects for successful PES programme implementation in common-property forests.

Specifically, we develop and test three hypotheses (H1, H2 and H3) about the behavioural effects of conditional payments (see the Supplementary Information, Section A, for a more extensive discussion of these hypotheses).

H1. Payments conditional on group performance will increase cooperative conservation of common pool resources

If one assumes that resource users always seek to maximize their individual, short-term economic pay-offs, one would expect these users to overharvest the forest commons—no matter how big the PES incentive is when paid to the group as a whole[10]. However, an increasing body of evidence demonstrates that actual resource users are usually much more cooperative than that and are sometimes able to self-organize to solve common pool resource (CPR) dilemmas[19–21]. We build on earlier experimental work investigating how PES affects self-governance of CPR systems[10,22,23] to propose that forest users—many of whom live in communities that have

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self-organized governance systems for their forest commons—will perceive the conditional PES payment as an additional positive incentive to find cooperative solutions to the CPR dilemma. We expect this positive conservation effect to be particularly strong when PES interventions enable forest users to communicate as this allows them to exercise their self-organization skills.

H2. Payments will increase the motivation of forest users for conservation, especially when users' livelihoods depend on forests

There is evidence that the introduction of a 'market framing' into what was previously viewed as a 'non-market situation' will cause the crowding out of pre-existing, non-monetary motivations[24,25]. However, some of the most widely cited evidence on the crowding out of non-monetary motivations by financial compensation comes from settings in which pre-existing motivations were entirely intrinsic, with no pre-existing economic motivations[16,17]. These studies say nothing about situations where a market logic may already prevail or where monetary incentives are already salient for individual decisions. Large numbers of forest users, especially in rural areas of developing countries, depend on forests to sustain their livelihoods[4,8,11], and are therefore likely to be motivated, at least partially, by economic considerations in decisions regarding their use of forest resources. These economically dependent forest users are a common target for PES programmes[2,3]. In this context, PES would not introduce an economic logic where previously none existed. Following this logic, we expect users to conserve at least as much forest post-PES as they did in the period before the intervention.

H3. High-trusting individuals are more likely to sustain cooperative conservation behaviour after the PES incentive is removed

Trust is widely cited as an important factor contributing to cooperation in social dilemma games. Less well understood is what happens to the effect of trust on cooperation when payments are involved. We define trust as an individual's beliefs about the expected behaviour of others[26–30]. Trust determines a person's baseline beliefs about how cooperative others will be[31]. Faced with a collective-action problem—in this case conserving a CPR—an individual's beliefs about the expected behaviour of others will influence that individual's decision to cooperate (conserve the forest) or not (harvest trees). Since individuals tend to interpret new information in ways that confirm their pre-existing beliefs[32–34], we propose that participants in PES programmes will interpret the observed behaviour of others differently depending on how much they trust others. High-trusting forest users will interpret the cooperation they observe, induced by the PES intervention, as confirmation of their belief that other users are also generally inclined towards cooperation. As a result, they will expect others to continue to conserve resources even after the payments end.

Results

We tested these ideas with a framed field experiment (FFE) that enrolled 1,200 forest users living in 54 different rural villages near tropical forests in five developing countries: Bolivia, Indonesia, Peru, Tanzania and Uganda. As active users who collectively own and manage forested lands, these individuals and their villages are all plausible targets of PES interventions. The FFE is an appropriation game in which players harvest from a CPR, framed as a forest. The game is divided into three stages with the treatments (described below) applied only during the second stage, allowing us to measure the impact during and after interventions relative to a baseline established in the first stage. Each stage consisted of eight decision rounds, during which participants were asked to make individual decisions about how many resource units each participant would like to appropriate from the shared forest.

We started the experiment by dividing the participants from each village into three groups of eight individuals. In total, we had 150 groups of forest users who participated in the experiment. Each group was assigned to one of three treatments: Treatment B, an imperfectly monitored, conditional monetary incentive (hereafter called a 'bonus' or 'PES'); Treatment C, an opportunity for communication between participants; or Treatment BC, which used both the bonus and communication features. Each of these treatments was assigned to 50 groups of forest users ($n = 400$ individuals). Participants earned payoffs in the game in the form of tokens (which we converted into local currency at the end of the experiment) so that all participants' decisions in the experiment had personal financial implications.

To measure trust among our participants, we conducted a pre-experiment survey that asked participants to indicate the degree to which they trust other people (in their community). See the Supplementary Information for a detailed description of the field experiment, including the field protocol and analytical methods.

Our simulated PES interventions prompted an increase in pro-conservation behaviour: for the two groups that received a PES treatment, there was an abrupt drop in harvesting when the bonus payment was first introduced. While harvesting rates increased somewhat after the bonus payment was removed, they still remained below pretreatment levels on average during the post-treatment stage of the experiment. More specifically, we found that (1) during the intervention, PES increased forest conservation behaviour; (2) after payments stopped, users continued to harvest less on average than they did before the PES intervention, especially when they were able to communicate; and (3) trust amplified the lasting conservation effects of PES interventions in the post-treatment stage.

During the intervention, payments increased forest conservation behaviour. The PES intervention caused significantly lower harvesting levels ($P < 0.001$), relative to the average rate in the pretreatment stage, which was roughly 3.5 trees per round across all three treatment groups (Table 1). On average, individuals harvested roughly 0.7 fewer trees per round when the conditional incentive was offered (Treatment B) compared with the pretreatment rounds, a decrease of 19%. Participants who received the communication-only treatment (Treatment C) harvested roughly 0.9 fewer trees per round on average during the treatment rounds (compared with the pretreatment rounds), a decrease of 25%. The most effective conservation intervention of the three was the one that combined a monetary bonus payment with opportunities for communication (Treatment BC), during which participants harvested 1.7 fewer trees per round on average compared with the pretreatment rounds, a decrease of 48%.

After payments stopped, users continued to conserve more on average than they did before the PES intervention, especially when they were able to communicate. The two groups that received a PES treatment abruptly reduced harvesting when the bonus payment was first introduced, but showed no equivalent rebound in harvesting after the bonus payment had been removed (see Fig. 1). In fact, average harvesting was 4.3% lower in the post-treatment stage when the bonus payment was no longer offered, relative to the pretreatment stage (Table 1). This result indicates that PES not only swayed users to harvest significantly less while payments were being offered, but also that increases in conservation behaviour induced by PES were still measurable after the monetary incentive was discontinued ($P = 0.003$). The communication treatment also had a sustained effect that appeared substantially stronger than the effect of the PES bonus alone (with harvesting rates 19% lower than pretreatment levels), but we saw the strongest lasting effect in the treatment that combined the bonus with communication (a reduction of 23% below pretreatment levels). This result suggests that PES can

Table 1 | Treatment and post-treatment effects on individual harvest decisions

	Treatment effects on individual harvest decisions		
	Treatment B (bonus)	Treatment BC (bonus with communication)	Treatment C (communication)
Treatment	−0.672	−1.660	−0.874
	(−0.774, −0.570)	(−1.759, −1.560)	(−0.972, −0.776)
	$t=-12.903$	$t=-32.756$	$t=-17.466$
	$P<0.001$	$P<0.001$	$P<0.001$
Constant	3.602	3.452	3.450
	(3.527, 3.677)	(3.375, 3.528)	(3.377, 3.523)
	$t=93.709$	$t=88.552$	$t=92.368$
	$P<0.001$	$P<0.001$	$P<0.001$
	Post-treatment effects on individual harvest decisions		
	Treatment B (bonus)	Treatment BC (bonus with communication)	Treatment C (communication)
Treatment	−0.155	−0.803	−0.666
	(−0.255, −0.055)	(−0.900, −0.707)	(−0.762, −0.570)
	$t=-3.026$	$t=-16.314$	$t=-13.591$
	$P=0.003$	$P<0.001$	$P<0.001$
Constant	3.602	3.452	3.450
	(3.529, 3.675)	(3.377, 3.526)	(3.376, 3.524)
	$t=96.356$	$t=90.640$	$t=91.301$
	$P<0.001$	$P<0.001$	$P<0.001$
	Post-treatment effects on individual harvest decisions (interacted with trust)		
	Treatment B (bonus)	Treatment BC (bonus with communication)	Treatment C (communication)
Treatment	−0.149	−0.807	−0.665
	(−0.250, −0.049)	(−0.904, −0.711)	(−0.761, −0.569)
	$t=-2.912$	$t=-16.411$	$t=-13.574$
	$P=0.004$	$P<0.001$	$P<0.001$
Trust	0.016	−0.011	−0.108
	(−0.057, 0.089)	(−0.091, 0.069)	(−0.182, −0.034)
	$t=0.436$	$t=-0.264$	$t=-2.850$
	$P=0.663$	$P=0.792$	$P=0.005$
Treatment × trust	−0.176	−0.198	0.103
	(−0.278, −0.075)	(−0.301, −0.094)	(0.006, 0.200)
	$t=-3.406$	$t=-3.737$	$t=2.089$
	$P=0.001$	$P<0.001$	$P=0.037$
Constant	3.601	3.451	3.449
	(3.528, 3.675)	(3.377, 3.526)	(3.375, 3.523)
	$t=96.240$	$t=90.717$	$t=91.344$
	$P<0.001$	$P<0.001$	$P<0.001$

Coefficients from linear mixed-effects estimation listed with heteroscedasticity-robust 95% confidence intervals in parentheses. Unit of analysis is individual-round. All estimations were performed with random intercepts at the level of the group and the individual participant. Treatment effects were estimated by comparing individual decisions from the pretreatment rounds (1–8) with the treatment rounds (9–16). Post-treatment effects were estimated by comparing individual decisions from the pretreatment rounds (1–8) with the post-treatment rounds (17–24). $N=6,400$ for each estimation.

promote conservation behaviour over the longer term, especially when users can communicate with one another. This also suggests that, at least in the context of our experiment, the effect of the bonus in the absence of communication was comparatively weaker than the other two interventions.

Although the main focus of our analysis is the change in users' average harvesting decisions as a result of the treatments, it is interesting to note that out of the 1,200 participants, 801 of them (66.75%) decided to harvest less during the stage of the three treatments, and 675 of them (56%) decided to harvest less during the post-treatment stage compared with the pretreatment stage (even though we offered no external incentives during either pre- or post-treatment stages). Table A1 in the Supplementary Information presents a more detailed discussion of how many participants reduced harvesting rates across the stages of the experiment.

The trend lines in Fig. 1 show that cooperative behaviour did decline gradually from round to round during all three stages. This decay of cooperation is common in cooperation games such as ours[35].

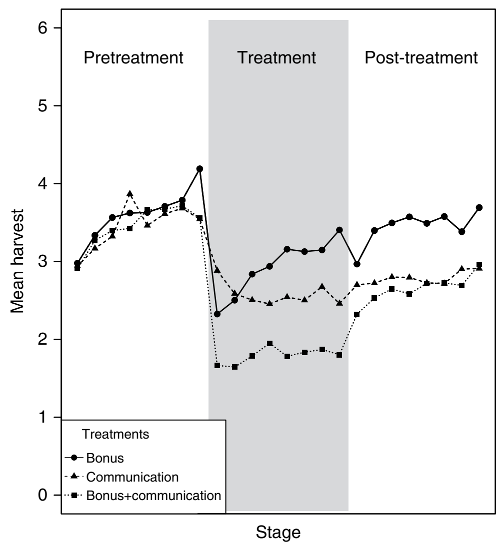

Fig. 1 | Aggregate-level harvesting patterns across stages of the game. The figure shows mean harvesting levels at each round of the game for all participants assigned to each treatment.

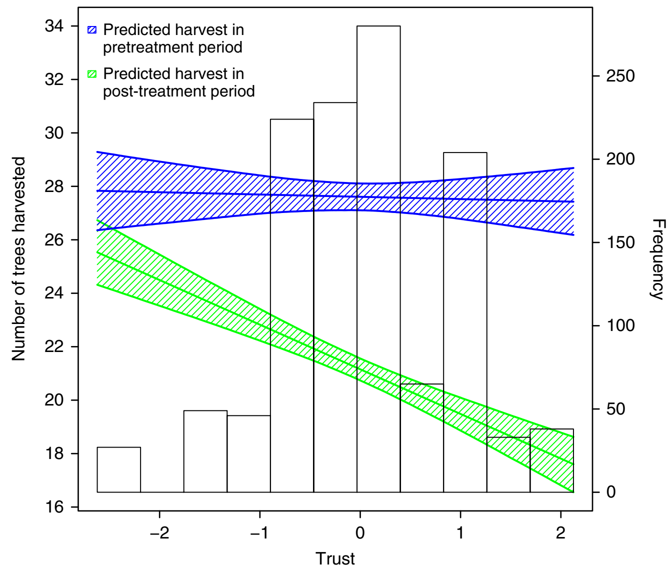

Fig. 2 | Predicted individual-level harvest in pretreatment and post-treatment stages for individuals with varying levels of trust who received the bonus-with-communication treatment, with 95% confidence intervals. The green prediction line represents the predicted number of trees harvested as a function of individual trust after the bonus-with-communication treatment has ended (rounds 17–24), and the blue line is the predicted harvest as a function of trust during the pretreatment period (rounds 1–8). The histogram overlay shows the distribution of trust scores for all individual participants.

To check for treatment effects on the round-to-round change in harvesting levels in the post-treatment stage of the game, we transformed the dependent variable to its first difference, but failed to find any statistically significant treatment effects on the rate of change in harvesting. In other words, the post-treatment effects are apparent as changes in average harvesting levels, rather than as changes in temporal trends in harvesting.

The simple aggregate-level comparisons presented in Table 2 show that the experimental treatments were strong enough for all treatment and post-treatment effects to be detectable and statistically significant ($P<0.001$) even after aggregating the unit of analysis to the group (substantially reducing the number of observations to $N=50$), except for the post-treatment effect of Treatment B. This is probably due to the small magnitude of this effect, the loss of power caused by aggregation, and the temporal decay of cooperative behaviour, which biases the estimated effect size downwards in a simple paired test (see Methods and Supplementary Information, Section B4).

Table 2 | Changes in harvesting patterns across game stages at the aggregate group level, $N=50$

Differences between treatment rounds and pretreatment rounds		
Treatment B (bonus)	Treatment BC (bonus with communication)	Treatment C (communication)
$W=173$	$W=30$	$W=132$
$P<0.001$	$P<0.001$	$P<0.001$
Differences between post-treatment rounds and pretreatment rounds		
Treatment B (bonus)	Treatment BC (bonus with communication)	Treatment C (communication)
$W=459$	$W=179.5$	$W=248$
$P=0.128$	$P<0.001$	$P<0.001$

Test statistics and two-tailed P values are shown from paired Wilcoxon signed-rank tests (W) with continuity correction. For these aggregate group-level comparisons, summed harvests in each group of eight participants in the pretreatment period (rounds 1–8) are compared with those of the treatment (rounds 9–16) and post-treatment (rounds 17–24) periods. Aggregate-level descriptive comparisons are discussed in further detail in the Supplementary Information, Section B7, and additional comparisons are also presented. Each test was performed on $N=50$ groups.

Trust amplified the lasting conservation effect of PES interventions. Our results also suggest that the way users responded to the PES treatment depended on their individual trust scores: the estimated interaction terms for the PES treatments in Table 1 are negative and significant ($P<0.01$), suggesting that the estimated difference between post-treatment harvesting rates and pretreatment baseline rates depended substantially upon individuals' levels of trust for these treatments. The effect of the BC treatment is negative and statistically significant for all participants except for those reporting the lowest possible value for the trust variable (Fig. 2). For those with the maximum observed trust score, the model predicts an extraction in the post-treatment stage that is more than 35% lower than in the pretreatment stage. While the theory about the supportive role of individual-level trust did not lead to a specific expectation about the interaction between trust and the communication-only treatment (which was included in the analysis for the methodological reasons explained in the Methods section), the results show a substantively small interaction for this treatment that functions in the opposite direction to the other two interaction terms, suggesting that the estimated effect of the communication treatment is strongest for participants with the lowest trust scores ($P=0.037$).

Discussion

Our results challenge several common claims about PES and how such interventions affect conservation behaviour. First, conventional economic theory predicts that individuals facing a CPR dilemma such as this one should harvest the maximum possible amount (ten trees per round) in all stages of the experiment and that the treatments should have no effect. Consistent with prior research[19,20], our results reject this prediction, as participants harvested approximately 3.5 trees per round on average during the pretreatment rounds, and harvesting rates were statistically lower than baseline levels in the treatment period across all three treatment groups.

Second, drawing on motivation crowding theory, several reports have raised concerns about the possibility that PES programmes may reduce conservation behaviour by way of crowding out non-monetary motivations for resource conservation (see, for example, refs [14,39,40]). The implication of these reports for our experiment is that, if such crowding occurred, one would observe harvesting levels that were higher in the post-treatment stage compared with the pretreatment stage. That is not how users in our experiment behaved. In fact, forest users in all three treatments harvested on average significantly less in the post-treatment stage compared with the pretreatment stage. These findings are consistent with the results of two recent FFEs in Tanzania, using dictator games[41] and between-subject comparisons of CPR games[42]. Both studies failed to find evidence for motivation crowding from PES-like treatments.

Most forest users in developing countries have an economic stake in their interactions with forests. That they are already extrinsically motivated in their decisions about forest use may explain why our simulated intervention with extrinsic incentives did not undermine their forest conservation behaviour. While the number of forest-dependent people in the world depends on the definition used[43]—ranging from hundreds of millions[44] to 1.6 billion[45]—an indisputably large number of rural dwellers depend on forests for their economic livelihoods. For this large group of people, forests provide fuel, construction materials, food and medicine, and help users earn income to meet other basic needs. For example, one study that analysed data from 8,000 households living in or near forests in 24 developing countries found that more than 20% of their total income come from forests[46]. Where economic considerations already drive forest-use decisions, the risk that PES interventions crowd out intrinsic motivations is probably small. In our sample of forest users, fewer than 5% claimed to visit the forest for reasons only associated with personal enjoyment; the vast majority instead cited factors related to accessing products and services for their households or villages. This reality helps explain why users in our experiment responded to PES incentives by increasing and sustaining cooperative conservation behaviour.

It is important to note that this interpretation is consistent with crowding theory, which proposes that motivational crowding may occur only when individuals hold primarily intrinsic motivations to perform a task (people do things because it gives them personal enjoyment)[17]. Since the vast majority of our sample's participants are not primarily intrinsically motivated in their use of forest resources, crowding theory does not predict that PES would reduce their conservation behaviour. The result that PES did not crowd out conservation behaviour may be explained by PES incentives being compatible with pre-existing motivations (which are often a mix of intrinsic and extrinsic motivations).

It is possible that the particular PES design used in these experiments, which offered individual economic benefits contingent on the group's cooperative behaviour, signalled to participants that other-regarding behaviour is an appropriate and effective response to such collective-action dilemmas. By extension, designing real-world PES interventions in this way may help to reinforce forest users' cooperative sentiments.

The finding that our PES interventions increased conservation behaviour significantly does not mean that these interventions are always the most cost-effective approach to conservation. The strong effect of the communication treatment, which was substantially larger in the post-treatment stage than that of the bonus treatment without communication, suggests that non-monetary interventions can be at least as effective. Such interventions may include community-based interventions that promote inclusive governance practices, deliberative decision-making and face-to-face communication among local resource users. The strongest treatment during all stages of our experiment combined a monetary incentive with opportunities to communicate, which suggests that the design features of a PES intervention may matter a great deal for its effectiveness in shaping resource user behaviour.

Our results also suggest that trust is an important moderating factor in determining the post-treatment impact of PES programmes. Previous research has shown that individual trust is a relatively stable characteristic that is quite difficult for public policies to alter in the short term, but there is one particular contextual factor that exhibits a strong correlation with trust within groups: economic equality[47]. Other research suggests that policies that reduce inequalities within and between forest user groups may help users to sustain their communally managed forests[48]. Combining these insights with our own results implies that PES interventions may be more effective in the long run when supported by efforts to reduce economic inequality.

It is important to bear in mind that these results are based on an FFE, which has both advantages and disadvantages when it comes to evaluating the effects of PES. A big advantage is that PES programmes are so recent that experimental post-treatment data for these programmes are rarely available[23]. Relative to conducting conventional surveys, FFEs with actual forest users are likely to yield more reliable behavioural data. Conducting experiments with forest users helped improve the external validity of the findings, especially compared with more traditional laboratory-style experiments whose subjects (often undergraduate students) have little in common with rural residents of developing countries[49]. In addition, we incorporated two design features to ensure the games were perceived by participants as reasonable, albeit simplified, representations of everyday decision-making about their use of forest commons. First, the participants earned real monetary payoffs, in local currency, based on their decisions in the game, and these payoffs were structured to represent economic trade-offs associated with forest use and conservation decisions that characterize CPR dilemmas. Second, the PES treatments included a monitoring and enforcement feature with 'imperfect information' (see the Supplementary Information, Section B1) that mimicked the existence of transaction costs in the implementation of PES programmes[50].

These design features notwithstanding, the stakes and contexts in which forest users made decisions in our experiment are similar, but not identical, to decisions they make about their interactions with actual forest resources. For example, in cases where the PES intervention seeks to provide monetary compensation for not harvesting from the forest, once such compensation is removed forest users may have no other option than to return to harvesting products from the forests to support their livelihoods. Even though our results show that the PES interventions produced sustained increases in cooperative conservation behaviour (in turn, leading to improvements in the provision of ecosystem services), it is important to recognize that these benefits may not be enough for local people to refrain from extractive forest use once payments stop.

Even though external validity can never be fully assured in FFEs such as ours, FFE studies can constitute an important step along a pathway of policy evolution. This process allows decisionmakers and researchers to explore the probable behavioural responses to proposed interventions[23]. As such, our findings may be useful to policymakers and conservation programme managers as they consider options for designing PES interventions, including criteria for targeting (for example, levels of trust) and training (for example, opportunities to communicate and collaborate).

In conclusion, our findings bring greater nuance to the understanding of how PES interventions may affect user behaviour in forests managed as common property. Contrary to claims that PES interventions may reduce forest users' pre-existing motivations to conserve forests, we find the opposite relationship: cash payments increased forest users' ability to cooperate for the purposes of conserving forests, even after the PES intervention was discontinued. The findings also suggest that there may be social conditions—such as high levels of trust—that make PES programmes more likely to produce sustained improvements in the conservation of forest commons.

Not all PES programmes are designed and implemented in ways that will always reproduce our experimental results. There may be situations in which PES programme implementation could undermine group cohesion and trust among participants and consequently harm local collective action to protect forests. While our simulated PES intervention distributes payments equally among all participants, in reality there may be corrupt local leaders who pocket most of the payment for themselves. In other cases, PES programmes may have difficulties in defining user groups and determining which should be included in the programme, potentially causing conflicts between members of different villages or user groups.

PES is no panacea for forest conservation, and behavioural responses are likely to depend not only on the incentive itself or the management decisions about implementation, but also on the characteristics of the local forest users. Our findings do suggest, however, that policy actors may be able to increase PES programme effectiveness on forest commons by promoting interventions that facilitate interpersonal communication among forest users, and by prioritizing implementation in contexts where users enjoy high levels of trust.

Methods

Framed Field Experiment (FFE). We used an experiment that is an adaptation of the traditional appropriation or CPR game[36]. It models how forest users behave when facing collective-action problems associated with the use and protection of a CPR, which we framed as a forest. We described the experiment to participants as a series of decisions about harvesting from the local forest. The underlying structure of the experiment is characterized by a tension between monetary incentives promoting cooperation to maximize group-level earnings and free-riding to maximize individual earnings while others contribute to the protection of ecosystem services. This design complements traditional CPR games in that it seeks to capture the ecosystem services that accrue when the group is able to conserve some or all of the CPRs. These ecosystem services include protection from wind and soil erosion, the purification and storage of water and pollination, among other services. Most scholars and policy actors now recognize that these ecosystem services contribute an enormous value to rural livelihoods—by improving agricultural productivity, stabilizing soil and ensuring a reliable supply of water—and that it is fundamental to capture the value of these public goods in the modelling of real-life decisions about common-pool forest resources[37,38].

The experiment was divided into three stages. In the first and third stages, only the underlying structure described above applied, and participants were not allowed to communicate. Any attempt to talk or otherwise exchange information, particularly about the game, resulted in a calm but firm intervention by the moderator (in practice, this rarely happened). Participants were, however, encouraged to ask clarifying questions (but not requests for strategic advice) about the game, which were repeated and the answers given to the group. In the second stage, we introduced one of three treatments: a conditional conservation payment or 'bonus' (Treatment B), open communication among participants (Treatment C) or a combination of bonus and communication (Treatment BC). Treatment B was designed to be similar to a PES intervention—to study the extent to which the payment alone altered the participants' conservation behaviour.

We explained to participants that a bonus would be paid directly and equally to each individual by an external group. We also explained that this group could not monitor the forest perfectly; if no harvesting took place, the bonus was awarded with a probability of 100%. This probability decreased linearly to 0% as harvesting increased to half of the forest; the bonus was never rewarded in any round when more than half of the forest was harvested. In Treatment C, participants were allowed to discuss and coordinate their decisions for a few minutes before each round. These time periods were announced as lasting 3–5 min, but this limit was not strictly enforced if the players were on topic; in practice, discussions tended to be much shorter in later rounds of the stage. The final treatment (BC) combined both the conditional payment and communication.

All treatments were designed so that they had identical social optima (harvesting nothing) and Nash equilibria (harvesting the maximum allowed). For treatments including a bonus, the conditional payment was equivalent to a 25% subsidy on top of the payout for forest non-use, assuming socially optimal behaviour.

We included the communication treatment because it has been a common experimental treatment in both CPR decision-making games[51] and public goods games[52] following early experimental findings that interpersonal communication among participants can be as effective as top-down enforcement or incentives[53]. This allowed us to compare the strength of our PES treatments with the conservation-enhancing effect of communication alone. Furthermore, utilizing a communication treatment with no monetary incentive allowed us to potentially rule out alternative causal mechanisms for the effects of the bonus treatments. For instance, if we observed higher harvesting rates in the post-treatment stage compared with the pretreatment stage, this would be consistent with a motivational crowding effect. However, if we observed the same pattern with the communication treatment, this would indicate that the apparent crowding effect had nothing to do with the monetary incentive at all.

Each treatment was played with a different group of eight participants in each village, for a total of 24 participants per community. We recruited participants along with local leaders who helped us to meet our goals of having groups with balanced gender composition and representing the ethnic, age (only including players 18 years and older) and other types of diversity present in the community. We also balanced these traits among the three groups in each community as well as possible. Section B5 in the Supplementary Information presents the results of a balance test, which suggests that our experimental treatment groups were balanced with respect to a number of individual-level covariates as well as the pretreatment behaviour for our activity. The order of treatments within each community was randomized to prevent spurious results due to potential discussions of the game between people who had participated and those who would participate in future games. Supplementary Table 6 presents descriptive statistics for all participants in each country.

The three-stage design of the experiment allowed us to assess both the immediate effects and the lasting effects[23] of the interventions. The immediate effects were estimated from the difference between the treatment stage (stage 2) and the baseline stage (stage 1); the lasting effects were estimated from the difference between the post-treatment stage (stage 3) and baseline (stage 1) stage. This design has two advantages. First, it allowed us to assess both immediate and post-treatment impacts of incentives, which is something of particular importance in policy choices as incentive programmes tend to have a finite time horizon (because of funding constraints). Second, it allowed each group to be its own control, allowing more robust statistical comparison and thus stronger conclusions.

At the end of each game the token earnings accrued by each player were summed, and these were converted into local currency at a country-specific rate. These rates were determined as a function of local rates for unskilled wage labour so that cooperative play would result in a payout of between one and two days' wage, and uncooperative play a bit over half that amount. The payouts were made individually to players in private, typically in another room or outside the building, and no announcements or information were circulated about other players' earnings. They were, however, free to discuss this with one another after the activity ended.

Further details of the experimental design, including the precise value attached to harvesting decisions and the bonus, the expected earnings under cooperative and selfish decision- making and full field protocols for the game are available in the Supplementary Information, Sections A and B.

Analytic methods. In analysing and presenting the results of our experiment, we faced four key challenges: (1) disentangling the during-treatment direct effects and post-treatment lasting effects of PES on harvesting behaviour; (2) utilizing variation in decision-making between individuals as well as within-subject variation over the course of the game; (3) dealing with the dependence of observations at multiple levels—the country, the experimental group and the individual participant; and (4) testing the interaction between individual-level trust and the PES treatment. In order to address these challenges, we utilized a linear mixed-effects approach that takes advantage of the repeated observations for each participant while accounting for the dependence of observations at different levels. We utilized linear models to simplify the interpretation of our treatment and interaction effects, but we re-ran our analyses using a generalized linear modelling approach (presented in the Supplementary Information, Section B6), which produces the same inferences as the linear models that we present. The tests shown at the top of Table 1 compare harvesting behaviour in the treatment rounds for each treatment with harvesting behaviour in the pretreatment stage, allowing us to look at the direct effects of each treatment. The tests shown in the middle of Table 1 compare harvesting behaviour in the post-treatment rounds (after the treatment was taken away) with harvesting behaviour in the pretreatment rounds, allowing us to look at the lasting effects of each treatment.The tests shown at the bottom of Table 1 perform the same pre-to-post comparison but interact each treatment indicator with trust, allowing us to test the hypothesis that individual-level trust moderates those post-treatment effects.

Analysing harvesting decisions after payments stop is a stronger test of motivational crowding effects—that is, it does not limit the analysis of immediate, direct effects to the period when payments flow. All three models treat the individual-round as the unit of analysis to leverage temporal variation, and all models include random intercepts to account for non-independence at the levels of the user group and individual participant. We explore differences between countries in the Supplementary Information, Section B9.

Direct treatment effects. In order to estimate the immediate effect of the bonus and communication treatments on individual harvests, we fit three linear mixed-effects models (one for each treatment) on data from two stages. The first stage comprises rounds 1–8 of the game, before any treatment had been administered. With the unit of analysis as the individual-round, the observations from the first stage function as the baseline or control observations. The second stage comprises rounds 9–16 of the game, during which the PES was being offered for groups assigned to Treatment B and Treatment BC (and during which opportunities for communication, without any PES payment, were being offered for Treatment C). These models, presented at the top of Table 1, compare average levels of harvesting during the treatment stage with average levels of harvesting in the pretreatment stage for the individuals assigned to each treatment. The parameter estimate on the treatment indicator can be interpreted as the estimated average difference in harvesting levels during the treatment stage compared with the pretreatment baseline rounds. One common analytical approach is to use differencing to examine trends in the data over time, but our interest is in comparing average levels of harvesting decisions before and after the intervention, and differencing by round would not allow us to detect this effect. In Section B4 of the Supplementary Information, we replicate all of our results with a linear time trend, a common method for dealing with linear over-time trends in longitudinal data[54]. This is because there is a commonly observed decay of cooperative behaviour over time in public goods experiments[35] and our treatments are correlated with time.

Lasting effects. To estimate the lasting effects of the treatments, the three models presented in Table 1 compare harvesting decisions in the pretreatment stage with decisions in the post-treatment stage for participants assigned to each treatment. As with models estimating the treatment effects, this is a within-subject design in which the first eight rounds function as the baseline control observations. The post-treatment stage comprises rounds 17–24 of the game. Because the bonus and communication opportunities were eliminated after round 16, this specification allowed us to estimate the lasting effect of the treatments—the patterns of harvest that persist after the treatments are taken away. In other words, the PES 'treatment' in this set of models is the experience of having the payment offered for eight rounds and subsequently taken away. We similarly considered the post-treatment effects of Treatments C and BC. For Treatment C, this means that the effect we estimate in this model is the lasting effect of having opportunities for communication offered and then subsequently taken away. The parameter estimate for each treatment indicator is the estimated difference in average harvesting levels between the post-treatment stage and pretreatment baseline rounds for individuals assigned to each treatment. If the PES treatment 'crowds out' motivation to conserve forest resources, the estimates should be positive for Treatment B and Treatment BC but not for Treatment C.

Lasting effects and the role of trust. Our linear mixed-effects approach also allowed us to test our theory regarding the moderating effect of trust on the lasting effects of the PES treatments. In accordance with our theory about trust, we employed an individual-level measure of generalized trust. This variable, which is based on a pre-experiment survey question and is expressed in standard deviation units with higher scores indicating higher reported levels of generalized trust, is explained in the Supplementary Information, Section B2. At the bottom of Table 1, we present the same pre-to-post comparisons for each treatment presented above, but interact the treatment indicator with the trust variable. We expect a negative coefficient on the interaction terms between trust and the two bonus treatments because we expect high-trusters to be better able to sustain any cooperation achieved during the bonus rounds. Our theory regarding the role of trust in moderating the effects of payments leads to no specific prediction with respect to an interaction between trust and the communication-only treatment.

The results from our linear mixed-effects models presented in Table 1 are robust to the inclusion of country fixed effects. We also present the same results with a linear time trend (Supplementary Information, Section B4), covariates (Supplementary Information, Section B5) and a generalized linear mixed-effects approach (Supplementary Information, Section B6). These alternative tests produce the same inferences as the original results, with the exception that the inclusion of a linear time trend leads to larger estimated effect sizes for each treatment. This is probably due to the decay of cooperative behaviour over time in cooperation experiments[9], which probably attenuates our effect sizes since the treatments are correlated with time. The only result that does not appear as robust to these alternative specifications is the estimated interaction between trust and the Treatment C. While our theory did not lead us to formulate any particular hypothesis regarding an interaction between trust and the communication-only treatment, this interaction term was estimated to be statistically significant ($P < 0.05$) in the comparison presented in Table 1. The interaction effect is substantively small and in the opposite direction to the other two interactions and, unlike the other interactions, it is not robust to the alternative modelling strategy in the Supplementary Information, Section B6.

Data availability. The authors declare that the main data supporting the findings of this study are available within the article and its Supplementary Information files.

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Author contributions

K.P.A., C.F.S. and G.D.W. conceived of the project, M.C.L., K.P.A., C.F.S. and E.M. designed the experiments, M.C.L. and E.M. conducted the experiments, N.J.C. and T.G. developed the analysis 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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