Economic games can be used to promote cooperation in the field

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We present experimental evidence of the impact of playing a game on real-life cooperation. The game was framed as a pest-management activity, the effectiveness of which depends on the decisions of others. Playing the game changes behavior in the field, increasing the participation in all collective activities directed at reducing pest pressure. The economic impact of those activities is important, leading to losses that are ∼20% lower than in the control group. Increased cooperation reflects changes in the understanding of others' willingness to cooperate, not changes in the understanding of underlying technological interdependencies.

cooperation | pluralistic ignorance | framed field experiment

Cooperation with nonkin is a distinctive aspect of human life (1), maintained by norms of conditional behavior (2), where norms have the accepted definition of shared views of how individuals ought to behave in a given situation (3). Promoting and sustaining cooperation in a context where it hitherto did not exist may require the elimination of inaccurate beliefs about others' actions and beliefs, i.e., overcoming pluralistic ignorance (4).

A variety of approaches have been used to create belief shocks that may lead to changes in behavioral norms (5–7). Changing the law (8) may provide an indication of the acceptability of some practices and foster changes in behavior (9) or perceptions (10). Similarly, "edutainment " through radio shows (11), soap operas (12), or movies (13) can provide new role models that motivate the adoption of behaviors that were privately desired, but perceived as publicly shunned. At a local scale, information about what others do or value has been shown to promote environmental conservation (14), reduce alcohol consumption among teenagers (15), or increase female labor participation (16).

In this paper, we evaluate the feasibility of using a framed game as an approach to promote cooperation. Similar to social scientists (17) or students in a classroom (18), participants in these games may learn about others' behavior in conditions that were hitherto-unobserved, including about others' willingness to cooperate (thereby reducing pluralistic ignorance), without having to incur the costs of promoting it themselves. This possibility was first noticed by ref. 19, and, building on that suggestion, recent work has used economic games as a teaching tool about the potential benefits of cooperation, usually in contexts where such problems are relatively new, such as the overexploitation of aquifers due to expanded access to water pumps (20–22). We build on this work in two ways: by experimentally evaluating the impact of playing a game on behavior outside the laboratory (improving on earlier evidence that mostly relies on a before–after comparison) and by quantifying the relative importance of reducing ignorance about others' willingness to cooperate.

The game was framed as a way of reducing rodent damage to rice. In Asia, such damage has important impacts on food security (23), which are particularly noticeable during outbreaks, some of which have even been followed by famine and civil unrest (24). Given the long history of association between rodents and humans (25, 26), the importance and persistence of such large losses is usually traced to rodents' reproductive behavior, in particular, high litter size, short gestation period, and early sexual maturity (27, 28). In the absence of coordinated control (either by limiting rodents' access to food and/or culling rodents), rodents quickly multiply, rendering any individual effort almost useless. This is certainly the case of the black rat (*Rattus rattus*), one of the main rodent pest species in the world (29), including in northern Lao People's Democratic Republic (PDR), the setting of our analysis (30). The challenge, then, is how to coordinate multiple farmers into practicing rodent control over an extended period of time, when there are incentives to free-ride on others' efforts to reduce pest pressure (27, 28), a problem common to the management of other pests and invasive species (31, 32).

The game simulates this coordination problem in a simplified way. Players have to decide whether to contribute to reduce pest pressure by deciding how much time to allocate to the production of a private good (rice), knowing that the payoff (shown in Table 1 and corresponding to real money) also depends on how much time the group, as a whole, allocates to rodent control. Focusing on symmetric equilibria, the payoff table exhibits two Nash equilibria (own contribution = {0,1}), with the social optimum being achieved when everyone contributes one time unit to rodent control. As such, the payoff matrix parallels the decision to participate in tasks such as collective hunting of rodents [an effective, but unused, approach to rodent control in our setting (28)]. Still focusing on symmetric strategies, the social optimum can also be achieved with higher contributions (own contribution = {2, 3}), although these strategies do not correspond to a Nash equilibrium. Details about the rules of the game, as well as a discussion of players' behavior, are presented in *Methods*.

The game was played as part of a project aimed at evaluating effective ways to promote food security implemented in 36 villages in Viengkham and Pakxeng, two districts in the province of Luang Prabang in northern Lao PDR. Data were collected among 12 households per village randomly selected from village rosters in November 2017 (baseline data) and December 2018 (when endline data were collected), with one additional

Significance

We experimentally evaluate the effect of playing a framed economic game on real-life cooperation. We frame the game within the context of reducing rodent damage to rice. Playing the game increases cooperative control of this pest and significantly reduces losses. The effect is driven by learning about others' willingness to cooperate.

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Table 1. Time allocation and payoff table (excerpt)

OT	Your time – rodent control							
	0	1	2	3	4	5	6	7
0	7,000	7,000	7,000	6,500	5,500	4,500	3,500	2,500
1	7,500	7,000	7,000	6,500	5,500	5,000	3,500	2,500
2	7,500	7,500	7,000	6,500	6,000	5,000	4,000	2,500
3	7,500	7,500	7,000	6,500	6,000	5,000	4,000	2,500
4	7,500	7,500	7,000	6,500	6,000	5,000	4,000	2,500
5	7,500	7,500	7,500	7,000	6,000	5,000	4,000	3,000
6	8,000	7,500	7,500	7,000	6,000	5,500	4,000	3,000
7	8,000	8,000	7,500	7,000	6,500	5,500	4,500	3,000
8	8,000	8,000	7,500	7,000	6,500	5,500	4,500	3,000
9	8,000	8,000	7,500	7,000	6,500	5,500	4,500	3,000
10	8,000	8,000	8,000	7,500	6,500	5,500	4,500	3,500
11	8,500	8,000	8,000	7,500	6,500	6,000	4,500	3,500
12	8,500	8,500	8,000	7,500	7,000	6,000	5,000	3,500
13	8,500	8,500	8,000	7,500	7,000	6,000	5,000	3,500
⋮				⋮				
26	10,000	9,500	9,500	9,000	8,000	7,500	6,000	5,000
27	10,000	10,000	9,500	9,000	8,500	7,500	6,500	5,000
28	10,000	10,000	9,500	9,000	8,500	7,500	6,500	5,000

Note: Values are in LAK (1 USD = 8,650 LAK in May 2018). OT, others' time contribution. Number of players = 5.

survey collected in May 2018 (at the start of the main rice-producing season and immediately before the game was played). The baseline data confirm the importance of rodent damage in the region, a generalized awareness of interdependency in terms of the effect of individual decisions regarding rodent control has on neighbors' production, and an absence of collective activities directed at reducing this problem. Almost all respondents (95%) reported that rodent damage is one of the main reasons that they harvested an area smaller than planted. Estimates of the damage are large and similar to earlier studies (30): On average, 20% of the planted area during the rainy season is not harvested due to rodent damage. Almost 90% of the respondents agree that they benefit from neighbors' efforts at controlling rodents, but control mostly relies on individual use of traps (90% of the respondents) with no experience of coordinated control of this pest.

Half of the villages participating in this study were randomly allocated to treatment (play the game), while the remaining were used as control. In each of the treatment villages, five groups of five players participated in the game. We first invited members of households who were previously interviewed (at baseline and then reinterviewed in May 2018, up to 12 participants per village, but usually less due to attrition), after which we invited members of other households randomly selected from the village roster to complete the desired number of participants per village. In total, 450 people participated in these games, of which 175 had been interviewed previously.

In June 2018, all 36 villages received a short training on rodent control, delivered by local extension officers and focused on the implementation of collective rodent hunts (28), allowing them to overcome any limitation in the capacity to implement this approach. The training included a demonstration of how to implement collective hunting, with extension officers organizing one in each village in July 2018, after which villagers were encouraged to further organize these activities by themselves. Extension officers also pointed out that August and September would be the ideal times for further activities.

The hypothesis we want to test is whether playing the game leads to changes in cooperation in the field during the forthcoming production season and, ultimately, to a reduction in area damaged by rodents. In addition, we want to understand the mechanisms that drive any change in behavior, distinguishing between learning about the benefits of collective action [usually emphasized in previous work (20, 21)] and learning about other players' willingness to cooperate.

Our identification strategy relies on the random allocation of treatment across villages. Randomization of treatment status was largely successful in creating two groups that are statistically identical at baseline. Similarly, selective attrition does not seem to be important. Table 2 presents ordinary least squares (OLS) estimates of intention-to-treat (ITT), which reflect changes due to living in a treated village on our main outcomes of interest: participation in training and in collective activities, reductions in damage due to rodents, and participation in other collective activities. Our preferred specification is presented in section B, where the inclusion of additional controls improves the precision of the estimates and provides slightly more conservative estimates of the effect of this intervention on rodent damage in the rice plot. Accounting for the relatively small number of clusters in this experiment through the estimation of wild-bootstrap P values does not significantly change our conclusions.

Households in villages where the game was played were significantly more likely to participate in rodent-control training and in collective rodent-control activities (hunting), either promoted by the extension officers or organized at the village level, with participation rates that were ∼9%, 10%, and 11% higher than in the control villages, respectively. These differences in behavior translate into reductions in rodent damage in the rice plot, with households in treated villages reporting damage (expressed as share of the total area of the main rice plot) that is, on average, 20% lower than in control villages. This effect is precisely estimated and economically important: Given that the average household in control villages harvested 1,780 kg of unmilled rice, our preferred estimate of damage reduction translates to 76.7 kg of unmilled rice per household, roughly equivalent to 2 wk of rice consumption by the average household. However, it does not seem that an increase in cooperation in one domain (rodent control) translates into increases in participation in other community activities, which could indicate some general increase in willingness to cooperate.

These conclusions did not change when we focus on the subsample of compliers (those who were invited to the game and accepted the invitation), although Local Average Treatment Effects (LATEs), estimated by using instrumental variables, were slightly larger than the ITT estimates. Although outcomes were

Table 2. **Effect of playing the game on cooperative behavior and damage: ITT estimates**

	Training (0/1)	Hunting (training) (0/1)	Hunting (village) (0/1)	Damage (% area)	Community activities (d)
A: ITT estimates, no covariates					
ITT	0.088	0.095*	0.098	−3.632**	−0.149
SE	(0.063)	(0.050)	(0.069)	(1.689)	(0.687)
Wild-BS	[0.181]	[0.072]	[0.174]	[0.039]	[0.824]
B: ITT estimates, with covariates					
ITT	0.090*	0.097**	0.106*	−3.542**	0.057
SE	(0.053)	(0.042)	(0.061)	(1.735)	(0.637)
Wild-BS	[0.115]	[0.042]	[0.123]	[0.056]	[0.937]
Control mean	0.662	0.508	0.344	18.772	5.651
N	399	399	399	344	346

Note: SEs, clustered at the village level, are presented in parentheses. Wild-bootstrap (wild-BS) *P* values are presented within brackets. See *Methods* for a discussion of control variables included in section B.
$*P < 0.1$; $**P < 0.05$.

self-reported, we did not find any evidence that Socially Desirable Response (SDR; a possible source of bias in our estimates) is an important concern in this study. Finally, ITT estimates are notably homogeneous, although we found some evidence of smaller reduction in damage in plots of female-headed households. See *Methods* for details regarding these different robustness tests.

These results allow us to estimate the cost–benefit ratio associated with this intervention. Using the ITT estimates from Table 2, section B, and the prices directly after the harvest (in December 2018, the lowest in the year) and accounting for the costs associated with the facilitation of the game, as well as the training on collective rodent hunting, we estimate a Benefit–Cost Ratio (BCR) of this intervention of 10.1 (*Methods*). However, the scaling up of this type of intervention may require the training of additional extension officers, which we are unable to cost.

We hypothesize two mechanisms through which playing the game may lead to changes in collective action. The first is a better understanding of the benefits of collective action on rodent control. The second is a better understanding of others' willingness to participate in collective action, a mechanism that has been largely ignored. In the endline survey, we asked those who participated in the game, and we were able to reinterview (133 out of 175 participants) what were the main lessons from their participation, focusing on those two mechanisms. Almost 30% of the respondents stated that they learned about benefits of a large participation in rodent control (i.e., of collective action, such as the rodent-hunting activities and as something distinct from spillovers between neighbors, which were largely known at baseline), while over 50% of the respondents mentioned that they learned that others were more willing to participate in rodent-control activities than they expected.

We use causal mediation analysis (33) to quantify the relative importance of these two mechanisms and decompose the estimates of the total effect in two components: the effect of the mechanism (and estimate the Average Causal Mediation Effect [ACME]) and a residual that includes the effect of other potential pathways (the Average Direct Effect [ADE]) (*Methods*). Table 3 presents these estimates for the two competing explanations examined here.

For the first mechanism (learning about the benefits of collective action), the estimates of ACME on participation in collective activities are generally positive, but always small. The results are substantially different when we examine the effect of learning about others' willingness to cooperate. All ACME estimates on participation in collective activities directed at rodent activity are larger and significantly different from zero, allowing us to conclude that this is the most important pathway leading to cooperative behavior outside the game. In both cases, the ACME estimates on damage in the rice plot are not significant, reflecting the fact that such reduction is achieved not because participants learned about the problem, but because something (collective hunting) was done about it. Results of an analysis of the sensitivity of these estimates to the effect of unobserved confounders that may bias the effect of the mechanisms on outcome and of the extension of this analysis to the consideration of multiple mediators do not substantively change these conclusions (*Methods*).

We can link the reduction in ignorance about others' willingness to cooperate with behavior in the game. Fig. 1 presents the partial linear regression of respondents' conclusion that others are more cooperative than expected as a nonlinear function of other players' average contribution in the game (all seven rounds), controlling for the effect of players' characteristics

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Table 3. **Effect of playing the game: Quantifying mechanisms**

	Training (0/1)	Hunting (project) (0/1)	Hunting (village) (0/1)	Damage (% area)
Mediator: Learning about the benefits of collective action				
ACME	0.042	0.052	0.033	−0.305
	[0.010 to 0.078]	[0.015 to 0.093]	[−0.003 to 0.071]	[−1.359 to 0.810]
ADE	0.027	0.021	0.058	−3.166
	[−0.067 to 0.129]	[−0.085 to 0.135]	[−0.047 to 0.171]	[−6.586 to 0.289]
ATE	0.069	0.072	0.091	−3.471
	[−0.025 to 0.170]	[−0.034 to 0.185]	[−0.014 to 0.201]	[−6.712 to −0.196]
Mediator: Learning about others' willingness to cooperate				
ACME	0.117	0.164	0.164	−0.417
	[0.063 to 0.171]	[0.103 to 0.226]	[0.104 to 0.227]	[−2.075 to 1.302]
ADE	−0.047	−0.091	−0.073	−3.074
	[−0.146 to 0.059]	[−0.200 to 0.026]	[−0.180 to 0.042]	[−6.747 to 0.637]
ATE	0.069	0.072	0.091	−3.491
	[−0.025 to 0.169]	[−0.033 to 0.186]	[−0.013 to 0.203]	[−6.777 to −0.180]

Note: The 95% CIs are in brackets.

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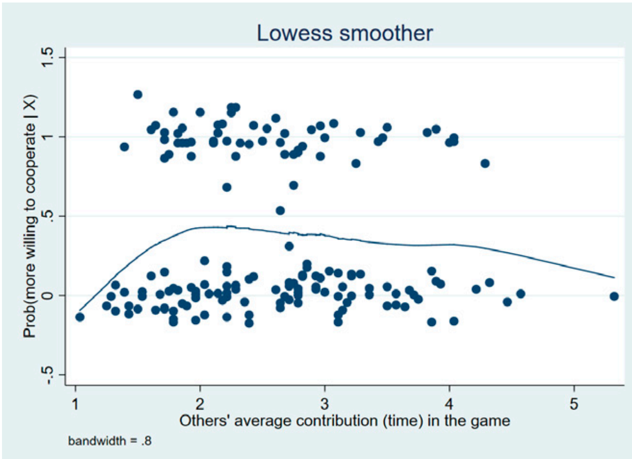

Fig. 1. Learning about others' willingness to cooperate as a nonlinear function of others' contribution in the game.

(*Methods*). There is a clearly nonlinear relation between the two variables: the probability of learning that others are willing to cooperate is zero at the Nash equilibrium (when contributions can be interpreted as reflecting individual rationality) and increases rapidly as average contributions increase to other levels of social optima (which correspond to average contributions of two and three time units, which, to be achieved, requires players to not use their best response strategies, i.e., to engage in costly cooperation).

In addition to providing evidence that economic games can be used to promote collective action at local level, the results in this article offer two main conclusions of practical importance. They both build on a better understanding of the mechanisms that drive cooperative behavior and, in particular, that games can be used to reduce pluralistic ignorance. The first is that it may be possible to use this approach to promote cooperation, even in the absence of social or technical change that creates new management problems (the most frequent motivation for its previous use in the literature). The second is whether other forms of reducing ignorance about what others do (via information campaigns or edutainment, for example) are equally as effective as the framed games we played and evaluated. However, and as the discussion of the external validity of field experiments emphasizes (34), a preliminary conclusion about the adequacy of such possibilities needs to be based on an understanding of the mechanisms underlying the failure to cooperate (including, possibly, the degree of ignorance about others' willingness to cooperate).

Methods

Description of Treatment.

Description of the game. The game was played in May 2018 in 18 villages randomly selected from a list of 36 villages in Viengkham and Pakxeng, two districts in the province of Luang Prabang, in northern Lao PDR. The set of 36 villages (treatment + control) was randomly selected from the list of villages surveyed for the Agricultural Census 2012 to be included in a larger project aimed at designing strategies to improve food security in the region. Participants were randomly selected from village rosters, with priority given to those who had been interviewed in November 2017 and reinterviewed in May 2018 (the day before the game was played). A total of 450 participants (of which 175 had previously been part of the study) were randomly allocated to five groups of five players per village.

The payoff table (*SI Appendix*, Table S1) and the nature of the payments were explained to all participants, with opportunity for several practice rounds to confirm that instructions were understood. The game proceeded through seven rounds, with the total amount of time allocated to rodent control by the group announced at the end of each round, allowing each participant to know how much they earned and how much time others had contributed. After seven rounds, each participant was privately informed of how much money he or she earned (equal to the sum of the payoffs of each round). After this announcement, participants had the opportunity to briefly discuss the game and what they could do to increase their earnings, after which they were allowed to decide (via majority decision in a secret vote) whether to play the game again or not, knowing that only one session would be paid at the end. Detailed instructions are presented in *SI Appendix*.

The main statistics of the game (contributions and payoffs) are summarized in *SI Appendix*, Table S2. We did not detect any difference in behavior between participants who were part of the study from its start and those who were recruited to play the game only. Approximately 50% of the participants played a second session of the game. On average, participants allocated 2.5 and 2.2 time units to rodent control in sessions 1 and 2. Average payment was 51,400 Lao kip (LAK), an amount equivalent to the wage of ∼1.5 d of casual farm labor.

Although not contributing time to rodent control was always a best response, this strategy was scarcely used. Limiting the analysis to the first session of the game, in which all players participated, 86% of the players always contributed a strictly positive amount of time, and an additional 7% chose not to contribute only once (out of seven rounds). One possible explanation for this behavior is that time contributions of one unit, corresponding to the social optimum, are also supported as a Nash equilibrium, and players were quickly able to coordinate their contributions at that level. However, as *SI Appendix*, Fig. S1 shows, players also seemed to shun this strategy, in favor of higher contributions (two and three time units) that, although associated with payoffs identical to the social optimum, were not supported by a Nash equilibrium. As a result, in the median round, the average contribution was always above two units.

At the end of the game, participants were invited for a debriefing session, to be held after all groups played the game. Participants had the chance to discuss their experience in the game, their strategies, and what they learned from it. The session was coordinated by a researcher from the National University of Laos, who led participants to recall the game through questions about participants' decisions and emphasized topics such as the optimal rodent-control strategies in the game and the benefits of collective action. The protocol for the debriefing session is presented in *SI Appendix*.

Training. Training on rodent control was implemented in all villages in the study, regardless of treatment status, and focused on the implementation of collective rodent hunts, previously shown to be the most cost-effective rodent-control activity in northern Laos (28). The purpose of this training was to overcome any limitation in the ability to implement this approach to rodent control. The training was implemented in cooperation with local extension officers.

Statistical Analysis.

Checking the integrity of the randomized design. We used t tests to check for balance between treated and control households in terms of potential determinants of cooperation and rodent damage. Treated and control households are statistically different from each other (at the 5% level of significance) only with respect to asset ownership and the fact that the main rice plot borders other plots, as shown in *SI Appendix*, Tables S3 and S4. The P values of an F test of joint significance of these variables on treatment status are 0.491 (household-level variables) and 0.987 (village-level variables).

Attrition rates were 15.5% (between baseline and time of treatment/midline), 7.6% (between baseline and endline survey), and 4.2% (between midline and endline survey). Although somewhat high (particularly at midline, reflecting its timing at the start of the rainy season, when most respondents spend considerable amount of time away from the village), attrition was never correlated with treatment status (*SI Appendix*, Table S5).

The game was played in treatment villages 1 d after the midline survey. A relatively small number of households (18 households, or 9.3%) who were invited to play the game did not participate. As shown in *SI Appendix*, Table S6, we found few significant differences between compliers (i.e., those who were invited and participated in the game) and noncompliers. The P value of an F test of joint significance of all variables on compliance status is 0.168. As with attrition at midline, the most likely explanation for their lack of participation seems to be timing of the game and its proximity to the start of the main production season.

Estimating treatment effects. We estimate the impact of playing the game using the following specification:

$$y_{i,1} = \alpha + \beta T_i + \gamma y_{i,0} + X_i \delta + \epsilon_i, \quad [1]$$

where $y_{i,0}$ and $y_{i,1}$ stand for the outcome variables for household i at baseline and endline (November 2017 and December 2018), respectively, and T is an indicator variable that is equal to one in those villages where the game was played. X_i is a vector of covariates included to address imbalances in the distribution of covariates at baseline (asset ownership and whether the main plot borders a neighboring plot), district fixed effects, and exposure to earlier work directed at controlling rodents during dry season in these villages discussed in ref. 35. SEs are clustered at the village level. Given the relatively small number of clusters, we also report *P* values obtained by using the wild bootstrap proposed in ref. 36.

Although compliers were not significantly different from noncompliers (*SI Appendix*, Table S6), it is possible that the decision to accept the invitation to participate in the game reflected the correlation between unobserved determinants of the participation decision and potential outcomes. We addressed that concern by using randomization of treatment status as an instrumental variable for the decision to participate in the game (37) and estimate LATEs. First-stage estimates are presented in *SI Appendix*, Table S7, while the estimates on the population of compliers are presented in *SI Appendix*, Table S8.

Accounting for bias due to SDR. All outcomes were self-reported, and, as such, our ITT estimates were potentially biased by respondents' overreporting of positive behaviors or underreporting of negative ones (SDR). We measured the tendency to respond in that way through the use of the short Balanced Inventory of Desirable Response (BIDR-16), developed by ref. 38 and validated by ref. 39 in a context related with the work reported here. The BIDR-16 includes 16 questions that aim at measuring two dimensions of SDR: respondents' need for approval (Impression Management) and the need to perceive oneself favorably (Self-Deceptive Enhancement). Questions used and the construction of each of these measures are presented in *SI Appendix*.

ITT estimates with these additional controls are presented in *SI Appendix*, Table S9. Because data on these questions were collected 6 mo after the endline, we lost 38 observations due to attrition (which was uncorrelated with treatment status: $P = 0.251$). The two measures of SDR were never independently or jointly statistically significant, and the ITT estimates obtained when controlling for SDR were not statistically different from those presented in Table 2, section B.

Heterogeneous effects. We focused on heterogeneity along the following dimensions: education (measured by years of schooling of household head), gender of the household head, size of agricultural land owned by the household, group membership (an indicator variable that is equal to one if any household member is an active member of a group in the village), experience with recent rodent damage (a set of indicator variables that are equal to one when the household indicates that there was small or a large damage on the main rice plot due to rodents in the wet season 2017), distance of main rice plot from the residence, and expectations regarding others' willingness to cooperate in solving a collective problem of water supply to the village. To identify these effects, we estimate the following equation:

$$y_{i,1} = \alpha + \beta T_i + (T_i \times I_i)\beta^\star + I_i\delta^\star + \gamma y_{i,0} + X_i\delta + \epsilon_i, \qquad [2]$$

where I_i are the household characteristics just defined, and all other variables have the same meaning as above. Results are presented in *SI Appendix*, Table S10.

Cost–benefit analysis. We made two main assumptions: the ratio of unmilled to milled rice (1 kg of unmilled rice = 0.70 kg of milled rice) and the number of beneficiary households (82 households per village). The price of 1 kg of milled rice before the harvest (September 2018) and after the harvest (December 2018) was ∼5,840 LAK and 5,080 LAK, respectively. The exchange rate for the two time periods was ∼1.158 US dollars (USD) and 1.160 USD for 10,000 LAK. Given these values, we estimated the value of savings per household as being between 370,000 LAK (or 43 USD) and 321,000 LAK (or 37USD), leading to a BCR in the range 8.5 to 12.1. Detailed values of this calculation, including costs, are presented in *SI Appendix*.

Identifying mechanisms. To quantify what participants learn from playing the game, we asked the following two questions in the endline survey:

1) Thinking about the game, did you learn anything new about the effect of rodent control activities on rice production?

which allows us to quantify any learning about the benefits of collective action; and

2) Thinking about the game, did you learn anything new about your neighbor's willingness to cooperate on rodent control?

which allows us to quantify any learning about others' willingness to cooperate in rodent control. The answers to these questions are reported in *SI Appendix*, Table S11. Given the format of the questions, we only have answers from participants in the game (as nothing can be learned from a game that hasn't been played). Although we were only able to reinterview 133 out of 175 players, attrition between the two surveys was random (*SI Appendix*, Table S5).

We used causal mediation analysis (33) to quantify the importance of the different causal pathways of the effect of treatment on subsequent collective activities. In this framework, the total effect can be disaggregated into a mediator effect (ACME) and a residual direct effect that measures the effect of all other mechanisms (ADE). In our case, we used what participants learned from the game (importance of collective action and other's willingness to cooperate) as mediators and estimated the following equations:

$$m_i(t) = \alpha_m + \beta_m T_i + X_i\delta_m + \epsilon_{mi}, \qquad [3]$$

$$y_{i1}(t, m) = \alpha_o + \nu_o m_i + \beta_o T_i + \theta y_{i0} + X_i\delta_o + \epsilon_{oi}, \qquad [4]$$

where m_i are the mediators (measured posttreatment) and X_i is the vector of covariates used in Eq. **1**, expanded to include expected willingness of others to cooperate in the solution of a hypothetical community problem that required collective action (in this case, access to water).

To obtain estimates of the ACME, we used OLS to estimate Eq. **3**. Predicted values of the mediator variable in both the treated and nontreated cases were then used to obtain an estimate of the ACME as the difference of the outcome model (Eq. **4**, estimated using OLS) under the two conditions (treated and nontreated). We used the package Mediation, described in ref. 40, to estimate these effects, which are presented in Table 3.

The estimates of ACME can be interpreted as causal if two assumptions hold. The first, sequential ignorability, assumes that no unobserved confounder matters both for mediator and outcome (i.e., $\mathrm{E}[\epsilon_{mi}, \epsilon_{oi}] = 0$). As with other tests of exclusion restrictions assumptions, a direct test of this assumption is not feasible. We followed ref. 33 and used simulation to quantify how large the effect of a potential unobserved confounder, present in both Eqs. **3** and **4**, must be in order for the original ACME estimates to become zero. The results of this sensitivity analysis are presented in *SI Appendix*, Table S12. Estimates of the impact of learning about others' willingness to cooperate on measures of cooperative behavior were clearly more robust to the potential presence of such an unobserved confounder than the estimates of the effect of learning about the benefits of collective action.

The second assumption was that the two mediators are independent. Given the possibility that both learning channels were triggered by similar experiences in the game (e.g., through a large use of time for rodent control by other players), we followed ref. 41, who extended this approach to multiple mediators, and estimated a varying coefficient linear structural equations model:

$$m_i(t, w) = \alpha_m + \beta_{m,i} T_i + \gamma_{m,i} w_i + X_i\delta_{m,i} + \epsilon_{m,i}, \qquad [5]$$

$$y_i(t, m, w) = \alpha_o + \nu_o m_i + \xi_o w_i + \beta_o T_i + \theta y_{i0} + X_i\delta_o + \epsilon_{oi}, \qquad [6]$$

where w_i stands for the alternative mediator (and for which we also estimated an analog of Eq. **5**). We used the R package Mediation, described in ref. 42, with the adaptations required to account for the lack of variation of the mediators in the control group, to estimate these equations. The ACME estimates for the two competing explanations are presented in *SI Appendix*, Table S13. The main difference from the results presented in Table 3 is that the first mechanism (learning about the benefits of collective action) is no longer statistically significant in explaining participation in collective hunting, reinforcing our conclusion that learning about others' willingness to cooperate seems to be the main mechanism through which playing the game leads to behavioral change.

Playing the game and learning about others' willingness to cooperate. We estimated the relation between learning about others' willingness to cooperate and the behavior of others in the game using a partial linear regression (43):

$$L_i = \phi(C_{-i}) + \delta Z_i + \epsilon_i, \qquad [7]$$

where L_i is an indicator variable that is equal to one if the player states that he or she learned that others are more willing to cooperate than expected, which we express as a nonlinear function of the average contribution of other players in the game (denoted by $\phi(C_{-i})$), while assuming a linear relation with other control variables, Z_i (age and gender of the player, as well as literacy and whether he or she has always lived in the same village or not). The nonlinear relation between others' choices in the game

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and respondent's learning about willingness to cooperate is presented in Fig. 1. Eq. **7** was estimated by using the package plreg described in ref. 44.

Ethical Approval. This study was approved by the Monash University Human Research Ethics Committee (MUHREC-13905). Participants were read an explanatory statement presenting the objectives of this study and provided verbal consent before participating in any activity.

Data Availability. Anonymized household data, village data, and player data have been deposited in the Open Science Framework (https://osf.io/rj9nx/) (45).

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