ELSEVIER Journal of Development Economics 70 (2003) 263–289
JOURNAL OF Development ECONOMICS
www.elsevier.com/locate/econbase

Real wealth and experimental cooperation: experiments in the field lab

Juan-Camilo Cardenas*

School of Environmental and Rural Studies, Universidad Javeriana, Trans 4 # 42-00, Piso 8, Bogotá, Colombia

Received 1 March 2002; accepted 1 June 2002

Abstract

This paper explores how wealth and inequality can affect self-governed solutions to commons dilemmas by constraining group cooperation. It reports a series of experiments in the field where subjects are actual commons users. Household data about the participants' context explain statistically the usually observed wide variation found within and across groups in similar experiments. Participants' wealth and inequality reduced cooperation when groups were allowed to have face-to-face communication between rounds. There are implications for a greater awareness of nonpayoff asymmetries affecting cooperation in heterogeneous groups, apart from heterogeneity in the payoffs structure of the game.

JEL classification: C93; D31; D63; O13; Q15; Q2
Keywords: Cooperation; Collective action; Local commons; Common-pool resources; Inequality; Wealth; Field experiments

1. Introduction

How do poverty and inequality affect the capabilities of communities to overcome the *tragedy of the commons*? Could societies rely upon the rural poor to manage sustainably local commons that may provide benefits to others? The incentives to overuse common-pool resources are well understood, and a variety of market and state-based solutions have been advanced and put into effect with various levels of success. There is also evidence that in some circumstances, in fact, members of a community may voluntarily cooperate to produce a socially superior outcome (Ostrom, 1990; Ostrom et al., 1994; Berkes, 1989;

* Tel.: +57-1-320-8320x4850; fax: +57-1-320-4847.
E-mail address: jccarden@javeriana.edu.co (J.-C. Cardenas).

0304-3878/03/$ - see front matter
doi:10.1016/S0304-3878(02)00098-6

Baland and Platteau, 1996), despite the incentives associated with the so-called tragedy. This paper explores how wealth and other related factors of group composition may affect the likelihood of cooperation and self-governance in a local commons dilemma by using evidence from field experiments conducted in three rural villages in Colombia.

In particular, this paper presents evidence that the actual levels and composition of wealth, as well as the real world occupation of the participants in the experiments, play a role in the level of cooperation and the solution of the local commons dilemma they faced through the experiment sessions. At the group level, we found that group efficiency improved for nearly all groups when introducing face-to-face communication, quite consistent with most experimental evidence in public goods and CPR experiments. However, and also consistent with previous experiments (Ostrom et al., 1994), the gains in efficiency varied greatly across groups despite having faced the same treatment design and payoffs incentives. The data collected on the real world institutions and behavior help explain part of this variability. In brief, the most interesting and significant results are that at the group level, both average group wealth and variance in the distribution of wealth decreased the level of social efficiency achieved by the groups. At a micro level, such relations are confirmed by statistical analysis showing that individuals were less likely to cooperate if their individual wealth was higher and their economic activity was less dependent on local commons dilemmas, supporting the group level relation between group efficiency and group's average wealth. Further, individuals were less likely to cooperate if they showed a larger 'wealth distance' relative to the average wealth of the other seven participants in the group, particularly if they were in the lower percentile in the wealth distribution, supporting the relation between the variance in group wealth and group efficiency.

2. Debates on inequality, heterogeneity and the solution to local commons dilemmas

How wealth and inequality play a role on the level of cooperation in collective action dilemmas remains a crucial but open question in the literature, starting with Olson's (1965) postulate that the privileged in a group would voluntarily provide the public good in collective action dilemmas[1]. New empirical and theoretical works suggest, however, that unequal distributions of wealth or heterogeneity within group members can reduce their capacity to coordinate their actions towards Pareto superior outcomes. One argument is that heterogeneity, wealth inequality and social distance in a group can hinder key triggers of cooperation and collective action such as reciprocity and trust in the solution of these

[1] In the case of public goods, Bergstrom et al. (1986) have also proposed that individuals with higher incomes would increase contributions, while those with lower would free-ride on the provision by the rich. However, there are basic structural differences between a commons dilemma and a public good contributions one, being the former the focus of this paper. Not only is the choice variable in one dilemma a mirror of the other, that is, while individuals extract from a common pool in the first, they contribute to the provision of a public good in the other, but other more critical differences must be taken into account. Unless explicitly constructed otherwise, the provision of the public good is based in a linear function of the sum of contributions, while the commons dilemmas usually involve a quadratic or at least a concave function on the sum of individual extraction decisions. The model used here, however, involves features from both types of problems and in a sense could be considered an impure nonlinear public goods problem as will be discussed later on.

dilemmas (Bardhan, 1993, 2000; Dayton-Johnson, 1997; Alesina and La Ferrara, 1999; Bardhan et al., in press; Ostrom, 1998; Varughese and Ostrom, 2001). Some have also suggested that there might be a quadratic relationship between inequality and cooperation, as developed theoretically by Dayton-Johnson (1997) and Dayton-Johnson and Bardhan (in press), where conservation of the conservation of the commons emerges at very low or very high levels of wealth inequality but not as much in the middle; Molinas (1998) has also explored empirically such nonlinear relation regarding cooperation and wealth for a sample of rural cooperatives in Paraguay.

In the case of rural villages using common-pool resources (CPRs), communities display a variety of types of heterogeneity regarding, for instance, the marginal net gains of using the commons by each individual. Some may have better equipment to extract the resource, reducing their effort cost relative to others. Some may need more of the resource because of their higher extraction costs or endowment of equipment. Those with better exit options such as better land or better education might gain comparatively less from resource harvesting, while others may derive most of their income from the resource. Most of the literature available on how inequality affects cooperation in these dilemmas has focused on this general notion that asymmetries in different components of the material benefits and costs for the users may support or reject the Olsonian proposition. In fact, Sandler (1992) develops further Olson's propositions and shows that the relation between heterogeneity in the payoffs structures and collective action can go either way depending on the assumptions about the public good production function. Elsewhere, I discuss in detail such literature[2], and test such arguments with another set of experiments in the same three villages introducing these asymmetries in the payoffs function of the players (Cardenas et al., 2002).

However, individuals' willingness to cooperate might also depend on a set of nonmaterial incentives involved in the solution of these dilemmas (Crawford and Ostrom, 1995; Ostrom, 2000). Shared values and norms (Ostrom, 1998, 2000) are key triggers of cooperation, and communication among members is a facilitator of these. When contracts are incomplete, as in CPR dilemmas, individuals depend greatly on information about other group members' history and on their own history and reputation so that an institutional environment of 'community' or social capital emerges (Cardenas and Ostrom, 2001). This information affects directly the possibility of building trust and reciprocity, which are key for cooperation, and wealth composition in a group may determine the way such information affects decisions.

A plausible approach for testing empirically these different propositions is through economic experiments, where one can study how changes in certain institutions and incentive structures affect individual behavior and group outcomes. In fact, some attempts have been made by experimentalists to study the effects on cooperation of different types of heterogeneity and social distance manipulated in the experimental laboratory. Hoffmann

[2] Varughese and Ostrom (2001) and Bardhan and Dayton-Johnson (2002) have reviewed the wide range of factors that may be involved in the relation between heterogeneity and the solutions to commons dilemmas. Recent theoretical models suggesting that cooperation in local commons dilemmas may decrease with inequality can be found in Baland and Platteau (1997a,b), Bardhan et al. (1998), Dayton-Johnson and Bardhan (2002) and De Janvry et al. (1998). A more recent workshop at the SantaFe Institute was devoted entirely to such relation (see http://discuss.santafe.edu/sustainability).

et al. (1996), Kramer and Brewer (1984), Chan et al. (1996), Lawler and Yoon (1996), Schmitt et al. (2000) and Hackett et al. (1994) have shown through different types of experiments how group heterogeneity affects in various ways cooperation, often in contradiction with the propositions by Olson (1965) or Bergstrom et al. (1986). Hoffman et al. (1996) showed how increasing the social distance between the subjects in a dictator game reduced the offers by subjects in the role of dictators to other participants. Kramer and Brewer (1984) argue that individuals might be prone to cooperate more when there is a greater sense of group identity, which is probably stronger in more homogenous groups where members develop a group identity based on what they are, do or have. Kollock (1998) reports a series of experiments where the membership to the same fraternity, a different one or no membership makes a significant difference in the level of cooperation by college students despite the symmetry in the material incentives. Notice that such values are brought to the lab, and not experimentally induced, and constitute key information people use in their decision.

Other empirical works also are relevant here. Alesina and La Ferrara (1999) show, using a General Social Survey (1974–1994) sample from US citizens, that the participation in social activities decreases in more unequal and more racially or ethnically heterogeneous groups. They also cite results from La Ferrara (1998) data from Tanzania where "the degree of participation in groups which provide economic benefits or informal insurance to their members is inversely related to income inequality in the community".

The focus here is on how heterogeneity among subjects, based on their social and economic conditions, may affect cooperation not because of asymmetries in the material payoffs, but because of variations in the effort costs that the group incurs to achieve a socially efficient outcome through self-governed mechanisms. These transaction costs of self-governance could be affected by the nonmonetary values that players may place on the costs and benefits of cooperating or defecting on the others in the group, even if they face symmetric benefits and costs from using the local commons.

To study this, we used an experimental setting where the material incentives were symmetric for all players, but where we observe other types of heterogeneity (e.g. across individuals' perceptions, values, history and reputation) that could affect the mechanisms that communities use for overcoming these dilemmas (e.g. trust, reciprocity, social distance, shared norms, etc.) and that have been shown to explain successful collective action (Ostrom, 2000). Instead of manipulating these effects artificially through experimental institutions or incentives, we observed them from the real context of the subjects, i.e. we gathered information on the actual wealth and other social and economic characteristics of the participants and studied if they may bring such information into the field lab, by exploring possible relations between these and their experimental behavior and outcomes. Since each group of eight people was composed of neighbors from the same community who had a previous history mostly known to each other, and since we allowed the groups to have face-to-face discussions as a space for devising self-enforced mechanisms, the design allowed us to create a rather realistic but controlled environment of social exchange among the participants.

Through these exercises, we replicated in the field lab some of the existing experimental evidence on cooperation in groups facing CPR dilemmas (Ostrom et al., 1994; Ledyard, 1995). We also expanded the evidence, however, by learning more about the set of preferences and institutional constraints that people face, and how these may affect the outcome in the field laboratory. We believe that we can explain part of the rather wide and puzzling variability usually found in these types of experiments by accounting for some of the real characteristics and context of the subjects.

3. A field experiment to study local commons dilemmas

The experimental setting emulates the actual incentives the participants face in their daily life where households benefit from harvesting multiple products from a patch of forest or mangrove where access is rather easy for villagers, despite the particular de jure property rights over the resources. Usually, the individual's benefits from a forest are increasing on one's extraction of products (e.g. firewood, logging, fishing), but decreasing with aggregate extraction due to a reduction on public goods benefits from the forest, such as water supply or biodiversity conservation (Cardenas, 2000).

3.1. Field experimental design: the payoffs table

Following much of the experimental literature on CPR experiments, we designed a decision-making exercise where a group of players in a group uses a resource for which there is joint access. Individual payoffs depended on the individual's choices and the choices by the rest of the group. Technically, our design is equivalent to a negative group externality case, and shares the properties of a CPR problem (Ostrom, 1990) where subtractibility is high and excludability is difficult.

In our decision-making exercise, the resource problem was described in the context of a forest. Groups of eight players faced the decision to use the same forest from which they derived direct benefits such as firewood, and indirect benefits such as water quality and biodiversity preservation. The net gains from choosing a particular individual effort in extracting forest products are given in Appendix A. The mathematical model from which the table is derived in detail is shown in Appendix B. Each player received the same payoff table, and had to choose the number of months she wanted to allocate in extracting products from the forest ("My months in the forest", columns 0 to 8 in Appendix A). The decision had to be made privately and individually, i.e. it was never known to the rest of the group during or after the session. Once the players made their decision and wrote it on a slip of paper, they handed this to the monitor who added the total group's months in the forest, which he announced publicly. With that total, each player knew "Their months in the forest" (rows 0 to 56 in Appendix A) and could calculate how many points (to be converted to Colombian pesos[3]) earned in that particular round. This procedure was repeated for up to 20 rounds for most of the groups.

---

[3] At the time of the experiments, the exchange rate was about 1350 pesos per US$1. For an average of 18 rounds, we expected subjects to earn somewhere between one and two minimum wage days of work as compensation for their participation.

3.2. The nature of the local commons dilemma

Appendix A shows clearly the conflict between maximizing individual earnings and maximizing group earnings. To illustrate this from the standpoint of one of eight players, suppose each of the other seven players chooses to spend 2 months collecting firewood from the surrounding forest. Since the sum of the seven players' choices is 14 months, Appendix A indicates that the eighth player's payoff-maximizing choice—the individual's Nash best response—is to spend 8 months collecting firewood. Note that her payoff in this outcome is 776 points, while each of the other seven individuals receive 535 points (for each of the other individuals, the sum of the others' choices is 20 months, while the individual choice is 2 months). Now, imagine that the eighth player chooses 3 months instead of 8, while the other seven players continue to choose 2 months. This can be thought of as a group-oriented choice—it is costly because that player's payoff is now 652 points instead of 776. However, each of the other players' payoffs increases from 535 points to 606. As for the standard benchmarks, it is straightforward to show that in this design, the optimal (group payoff maximizing) amount of time each individual should spend collecting firewood is 1 month (see details of the theoretical model in Appendix B). On the other hand, since a pure strategy Nash equilibrium requires that every player's choice be a best-response to every other player's best-response, in this context, the Nash equilibrium is reached if every individual decides to spend 6 months collecting firewood from the forest. It is worth noting that at the Nash equilibrium, subjects earn only 155 points, about 24% of the payoffs attainable at the efficient outcome. Table 1 summarizes the choice variable and outcomes for the two benchmarks of comparison for our analysis.

3.3. Experiments, participants and field setting

The experiment followed most of the conventions used in CPR experiments (Ostrom et al., 1994) in that it involves groups of eight subjects who participate in a series of rounds where they make their individual decisions. For this particular experimental design, we recruited in total 10 groups of 8 participants from three different villages in Colombia. In each of the villages, the community members have joint access to a forested area from which they derive most of their firewood, logging, hunting and water benefits.

Table 1
Two benchmarks for equilibria in the commons game

Social optimal solution	Individual decision (X^{opt})	$X_S^{opt} = 1$
($ group maximization)	Earnings ($) per round per player	Y_S^{opt}=$645
	Group earnings	SUMY$_S^{opt}$=$5160
		100% efficiency
Nash solution	Individual decision (X^{nash})	$X_S^{nash} = 6$
($ individual maximization)	Earnings ($) per round per player	Y_S^{nash}=$155
	Group earnings	SUMY$_S^{nash}$=$1240
		24% efficiency

The subjects sat at individual desks that were distributed in a circle with enough separation between the desks so they could not look at another's work. Except in rounds where communication was allowed, the desks faced away from the center of the circle. In each round, each subject would choose how many units of time, $x_i \in [0,8]$, to spend collecting firewood from the experimental forest. Each individual received a payoff table and the tables were common information. Each session began with welcoming remarks where the subjects were told that the session would last approximately 2 h. A monitor then read the instructions to the participants (see instructions translated in Appendix C).

The groups played 8–11 initial rounds of the game, without knowing exactly how many rounds the game would last, and not knowing a second series would be played with different rules. During these initial rounds, individuals made their choices without communicating with the others or the monitors. After this first stage, the monitors would stop the game and announce a new set of rules for the forthcoming rounds. The monitor read from a large poster, announcing that from then on, the group would be allowed to have a 5-min open discussion before the decision for the next round. The discussion could be about anything they wanted in relation to the game, but could not include threats or promises of transfers of cash after the game. After the decision in each round, the participants returned to their individual desks and made their individual and still private decision. The groups played this sequence [discussion → individual decision] for about 9–12 more rounds, and again without knowing the final round.

The average gain for a player during an entire session of about 18 rounds equaled—as planned—the value of 1.5 to 2 days of work at the minimum local wage. This amount was aimed at compensating them for participating in the game and a community workshop 2 days later was designed to discuss the experiment results and their experience with actual institutions for managing local commons. In addition, they filled out an exit survey questionnaire after the game with follow-up questions about the game, household data on their economic activities, participation in social life and preferences about certain issues related to our study.

4. Experimental results

The results from the 10 groups who participated under this symmetric payoffs treatment are consistent with most of the experimental evidence in CPR situations and also most public goods experiments. First, in a noncooperative game situation, group outcomes neither achieve the social optimal solution, nor do they approach the predicted symmetric Nash of purely selfish individuals. Secondly, the introduction of face-to-face communication among group members has a positive impact on behavior and earnings, despite the nonbinding condition of the discussions and the privacy of decisions.

Fig. 1 shows the evolution of the choice variable x (months in the forest) over time for the 10 groups (i.e. 80 individual decisions in each round), during the first and second stage of the exercises. The graph plots the mean of the 80 decisions and one standard deviation above and below the mean for those 80 observations.

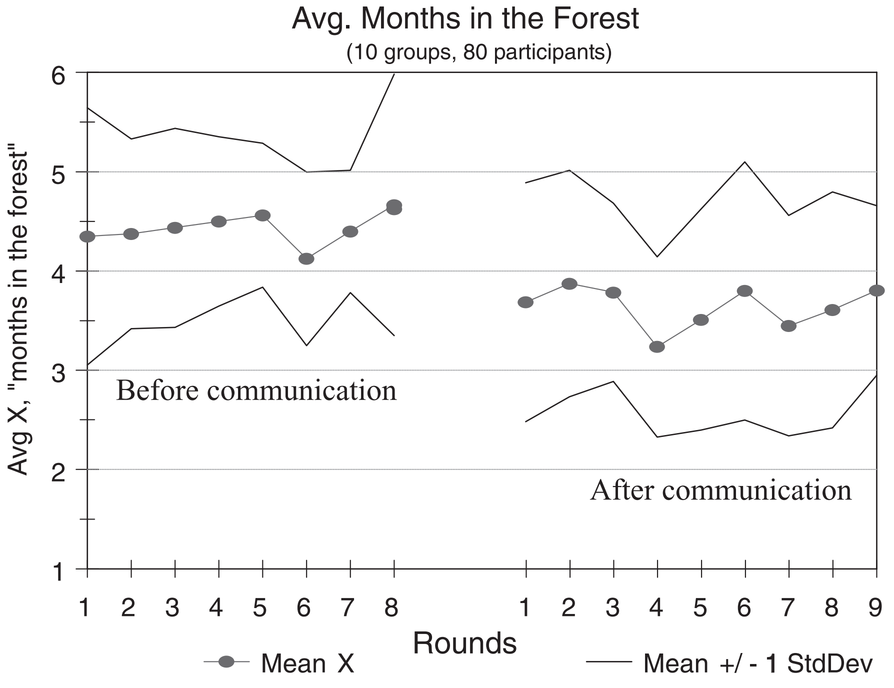

Fig. 1. Average individual "months in the forest" over rounds.

For rounds 1 through 8, the participants made their decisions individually and without communication within the groups. Beginning in round 11 and through round 19, all groups had 3–5 min for an open discussion before each decision round[4].

Fig. 1 shows, and statistical tests confirm, that on average, face-to-face communication induced a reduction in $\bar{x}_i$ at the individual level which created partial social gains for the groups. The $\bar{x}_i$ at the end of the noncommunication rounds was 4.38 months. The $\bar{x}_i$ at the end of communication was 3.76 months. A nonparametric test (Wilcoxon–Mann–Whitney) for the aggregate data confirms that communication induced a statistically significant effect in behavior. Comparing the individual choices for the last three rounds before communication to the last three after communication, the nonparametric test yields a Z value of -3.2408 (p-value = 0.0012). However, if we examine the data by groups, we observe considerable variation in the degree of effectiveness. In Table 2, we compare the choices ($\bar{x}_i$) and the earnings of the two stages for each of the groups. Each of the 10 rows represents a group, and the last row the average for all groups. The first two columns

[4] Some groups—but not all—did play rounds 9 and 10, as well as rounds ≥ 19. We wanted to avoid the problem of players knowing which was going to be the last round in each stage which in repeated games can increase defection rates when approaching the last round. All groups played up to rounds 8 and 19 in each stage, and these are the data we use for purposes of comparability in the analysis. However, within groups, we did not observe a significant change during those last rounds compared to what was happening at round 19.

Table 2
Average individual choices (*X*) and average individual earnings (*Y*$) by period (all rounds and last three rounds in each stage)

Groups	Choices (*X*) and earnings (*Y*$) in Stage 1 (no communication)		Choices (*X*) and earnings (*Y*$) in Stage 2 (communication)	
	All rounds stage 1		All rounds stage 2	
	X choice	*Y*$ earnings	*X* choice	*Y*$ earnings
CQS11	4.96	296.64	2.39	584.64
CQW41	3.36	493.75	3.22	512.11
CEW42	4.72	327.94	3.89	434.79
CES12	4.82	316.64	3.57	473.60
CNW41	4.84	311.50	3.78	449.77
CES11	3.93	429.88	3.60	476.26
CNS12	3.74	454.76	4.44	355.86
CNW42	4.38	376.05	3.68	457.28
CNS11	4.86	309.68	3.94	414.85
CEW41	4.77	324.25	4.93	300.91
All groups	4.44	364.11	3.74	446.01

provide information for the first stage of noncommunication rounds. The next two columns show the same information for the second stage of the game under communication.

Notice that most groups improved their payoffs, thanks to the possibility of face-to-face communication during the second phase, between rounds, but that the gains from such communication varied considerably given that the reduction in the average months in each group varied also across groups. Further, a simple correlation test for the average months before and after communication shows that there is no association between stages so that higher levels of cooperation at the second stage might be the result of the same behavior before communication. Those groups with higher earnings during the communication phase were not necessarily the groups with higher earnings during the no-communication phase, eliminating the possibility that increased earnings during the communication stage were simply due to a pattern of some groups increasing or decreasing earnings over time.

4.1. Unexplained variation across groups

Notice that most groups did decrease the average "months in the forest" and therefore increased their earnings. However, Table 2 and Fig. 1 show the wide variation in the average behavior over stages and rounds, which translates into wide differences in the outcomes at the group level. On the other hand, when examining the data within each group, we see a fraction of individuals with x_i choices on average close to 1 month—the social optimum requirement, while other individuals appear to be following a strategy close to the best response function given by the Nash prediction. Again, such variations cannot be explained by the structure of incentives since all groups and players faced the exact same structure in each round, and neither can they be explained by the rules before and after communication, which were the same for all subjects.

An alternative explanation could rely on the skills and levels of understanding of the problem by the participants[5], especially considering their low levels of schooling which averaged less than 4 years (StdDev = 2.798), while their age averaged little above 37 years (StdDev = 14.239). However, none of the demographic characteristics at the group and individual levels, central or variance statistics showed significant relations at the partial or multivariate regression analysis performed to explain variations in the choices or outcomes at the first or second stages. Simple correlation tests were run between group average behavior and the group average of these demographic variables to test for some associations, but the results were not significant. Further, the tests run at the individual level did not show either explanatory power of these variables. As it will be shown later, other characteristics of the group composition, such as actual wealth, have much more explanatory power of this variation.

From video observations and audio data that we taped during most of the sessions, it appears instead that the variation in the results is closely related to how each group used face-to-face communication to induce changes in individual behavior and improve outcomes. The familiarity that players showed with the problem and with each other in the group varied tremendously, as well as the intensity and degree of participation in the conversations.

Two major elements emerged from the analysis of the conversations during the experiments and from the community workshops as explanatory of the variability across and within groups: first, the degree of familiarity of players with the dilemma being dealt with, namely a local commons problem; second, the familiarity of a player with the others in the group when dealing with solving the conflict of strategies that maximized individual vs. group outcomes. The audio/video data suggested that economic factors that determined the relation of players with the dilemma (i.e. with the resources for which there is joint access, and with the rest of users who have access to them) might explain the degrees of cooperation and social efficiency achieved by the different groups. Below we present a statistical analysis designed to capture such effects, first at a group and then at a micro level.

4.2. A group level analysis: familiarity with the problem and with each other

Of several types of indicators, economic variables such as occupation, sources of income and wealth better explained the variability of the social efficiency achieved by the 10 groups. A simple example is shown in Fig. 2 where we plot vertically the average social efficiency (simply the average earnings from Table 2 divided by the maximum possible of 645 points) achieved by each group in the last three rounds of communication; and in the horizontal axis, the average real wealth of the eight participants in the group[6]. To calculate our indicator of household's wealth, we estimated at local market prices the value for land, livestock and equipment owned by the household of each participant, which they reported

[5] One particular advantage of running experiments in the field is the much wider variation for these types of attributes within the subjects pool, if we compare with similar experiments run with college students.

[6] The group CEW41 in the graph deserves a note. In Table 1, it is the one group that did not improve the results during the second stage. This group did not engage in any conversation that attempted to change the game decisions, and devoted this time to thank the organizers for these activities, and to repeat that they were "...doing just fine, and learning a lot".

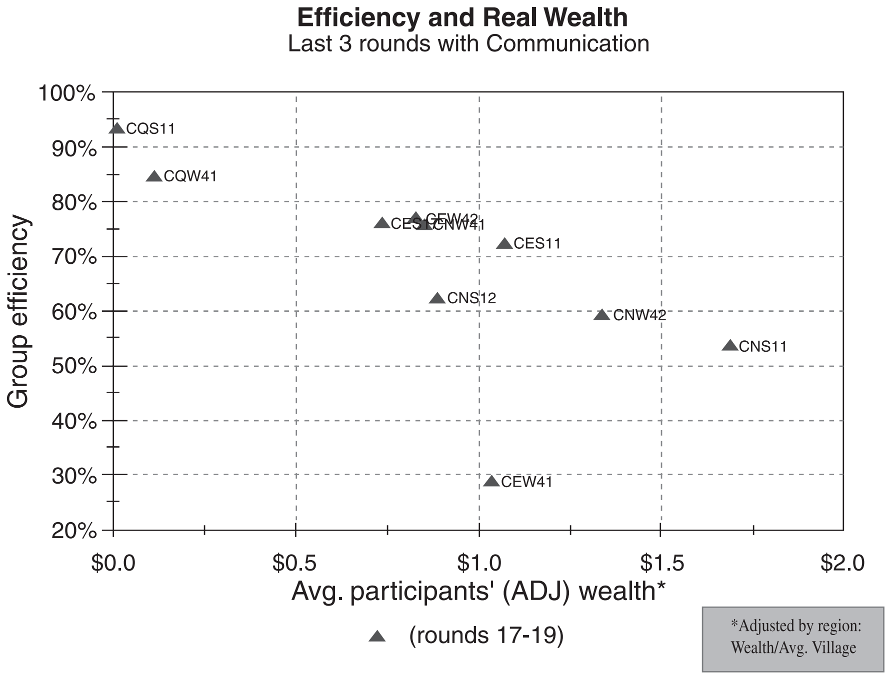

Fig. 2. Group efficiency and group's average real wealth.

in the anonymous exit survey mentioned before (the units are million pesos, 1998 prices). In order to make the wealth indicator comparable across the three villages, we adjusted the estimated values by dividing each individual's wealth by the village average, given the large differences in land market prices across villages.

According to the audio and video tapes, participants often drew parallels with their own experience using the local forest for extracting firewood, fibers or food. We wondered if such familiarity with the problem might have an effect on the experimental results. The group level and individual level analyses support this idea. Those with less land, livestock and machinery equipment are usually households that depend highly on using the forest as an important income source, and therefore face such dilemmas more frequently than those who can better support themselves with their own assets.

The second dimension we examined was how familiar players felt with the other seven participants in their group. Some groups had conversations where one could sense closeness among the players, while others had more formal and distant conversations. For instance, in the very first round with communication, for the group that achieved the highest efficiency (CQS11), one participant addressed the others with a warm and emphatic "compañeritos! (little buddies!) We are doing this wrong!", after which he went to explain how everyone could be better off by following the group maximizing strategy of 1 month in the forest, which in fact they achieved and maintained quite closely over time. Meanwhile, in other groups, the dialogue was much more difficult and cold. For instance, when 2 days later the

Table 3
Explaining average choices and group outcomes as a function of different inequality measures

Indep. variables	Dependent variables (using data from last three rounds under communication)						
	Average *X* choice (months in the forest)			Social efficiency (% of maximum group earnings)			(*)
	Avg *X*(B2)	Avg *X*(B2)	Avg *X*(B2)	SocEfic(B2)	SocEfic(B2)	SocEfic(B2)	SocEfic(B2)
Intercept	2.3836	1.8179	1.8518	0.9244	1.0892	1.0320	1.1345
	(0.0001)	(0.0115)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Avg. wealth$_i$	0.7854	1.4923	1.5231	– 1.1083	– 0.2703	– 0.2622	– 0.3271
	(0.0415)	(0.0003)	(0.0001)	(0.1569)	(0.0004)	(0.0001)	(0.0087)
Std. Dev. wealth	0.5418	–	–	– 0.1142	–	–	–
	(0.1039)			(0.1166)			
Gini wealth	–	0.7968	–	–	– 0.2262	–	–
		(0.2477)			(0.1107)		
Variance of (log (wealth$_i$))	–	–	0.1068	–	–	– 0.0217	– 0.0422
			(0.0022)			(0.0082)	(0.1091)
N (no. groups)	9	9	9	9	9	9	10
Adj. R^2	0.9310	0.9127	0.9792	0.8867	0.8883	0.9496	0.5501
F-test (*p*-value)	0.0001	0.0003	0.0001		0.0006	0.0001	0.0254

In parenthesis are the *p*-values for Ho: Coeff. = 0.
(*) Estimation including group CEW41.

group results were discussed during a workshop, we organized to return partial results to the community, a participant who is well known as one of the wealthier ones locally energetically claimed that he was trying to convince the others that by bringing the months in the forest down, they could all earn more cash. Another participant in his group then replied to this contention by saying "I did hear Don ________ [name omitted] saying that but usually I don't see him in the face, I was not believing him much"[7].

In order to test formally the hypothesis that the actual wealth of the participants affected their individual behavior and the group outcome, let us start with the group level data and the first regression estimations (see Table 3) of the average group choice and average level of group efficiency for the last three rounds of communication, as functions of the average wealth of the group, and of the groups' wealth heterogeneity. For the latter, we use three alternative measures of inequality, the standard deviation of wealth, the GINI coefficient and the log of the variance of wealth, finding consistent outcomes across. A tenth group (CEW41) was dropped from this estimation as a potential outlier, and supported on the fact that it showed a clear dissimilarity—after reviewing the audio and video tapes—from the common pattern throughout the other groups in improving cooperation after communication, and a much poorer understanding of the decision-making exercise and dilemma. The exact same model was estimated for the 10-group data set showing poorer estimation efficiency, although maintaining the same signs and findings. The last column to the right in Table 3 shows the results including the tenth observation, and can provide an idea of the loss in terms of overall econometric performance. The statistical results at individual and round decision levels in the sections ahead, however, provide the evidence to reinforce this first finding at the group level.

[7] Notice the difference between the "Don" and the "Compañeritos".

These results could suggest that the effectiveness of face-to-face communication can be greatly enhanced by groups composed of players who are more familiar with local commons dilemmas (lower wealth implying a greater dependence and familiarity), and by groups composed of more homogenous players that can more easily devise self-governing mechanisms to solve the dilemma. Since both effects, level of individual wealth and heterogeneity of wealth within a group, may be playing both a role, we need to make sure that their effects are not just because they are highly collinear—which they are as in many cases with income and wealth data. Furthermore, the significance of the other proxies used for group inequality in the regressions, which are less related to the units of average wealth[8], also shows the same consistent result of a positive relation of group heterogeneity and average months, which supports the notion that both factors play a separate and combined effect on behavior and social efficiency. Causality in this case, despite being a cross-section data set, can be defended on the grounds that the wealth characteristics existed before the experimental behavior outcomes.

Notice that the results in Table 3 are based on data for the last three rounds of the communication phase, and not for the entire subset of rounds. The reason for using the last three rounds at the group level during the second stage, to study how wealth affected behavior during the experiment, responds to the analysis, via the video and audio tapes, of the learning and face-to-face communication processes where the agreements were being built by each group. Only after a few rounds of the second stage the groups, some earlier than others, were able to realize the social optimum solution and begun to implement it through the face-to-face communication periods. By the middle of this second phase, most groups had tried to device and enforce an agreement to lower their 'months in the forest', and only during the last rounds of the phase we could actually test the success and robustness of such agreement.

However, using the entire data set at the group level can be illustrative. Table 4 shows the same estimated model for a data set where each observation of the dependent variable is the average for all the rounds during the communication stage.

Although the negative signs remain for the relation between group cooperation and wealth (average and inequality), the results are not as strong—except for the third column model—when using the data for all the rounds of the communication phase for the reasons just discussed. Once again, the smallness of sample size limits the possibilities of the statistical analysis and therefore the need to lower the level of study to the individual and round decision levels, as shown in the next section. Also, a time trend variable did not increase the explanatory power of the model since there was no decrease in the cooperation level over rounds, as is usually observed in linear public goods experiments.

It is also worth mentioning that the same regression model, when using the data from the noncommunication stage, has much lower levels of explanatory power, suggesting that the effects of real wealth in the behavior and outcomes are specially acting as a barrier to cooperation during the face-to-face communication phase of the experiment.

---

[8] The correlation coefficient between the average wealth for the group and the log of the variance of wealth for the group is -0.4596, while the same correlation of average wealth with the standard deviation of wealth is 0.8982.

Table 4
Replication of estimations in Table 3, using all rounds in the communication stage

Indep. variables	Average X choice (months in the forest) for all rounds in the second stage		
	Avg X	Avg X	Avg X
Intercept	2.970 (0.000)	2.858 (0.001)	2.044 (0.001)
Avg. wealth$_i$	0.452 (0.564)	0.797 (0.070)	1.063 (0.003)
Std. Dev. wealth	0.288 (0.417)	–	–
Gini wealth	–	0.133 (0.740)	–
Variance of (log (wealth$_i$))	–	–	0.180 (0.018)
N (No. groups)	9	9	9
Adj. R^2	0.319	0.313	0.741
F-test (p-value)	0.134	0.137	0.007

In parenthesis are the p-values for Ho: Coeff. = 0.

4.3. Individual level analysis: wealth distance and individual willingness to cooperate

It has been argued that wealth heterogeneity in these groups imposed a barrier to an endogenous solution to the local commons dilemma. The group level analysis suggests such argument, and the video and audio data provide additional elements to explore the processes within the groups that create such barriers. Ultimately, the group outcomes are the result of individual private decisions, and such decisions should then be mediated by the institutional constraints within the face-to-face mechanism.

Table 5
Individual round choices as a function of reciprocity, wealth and social distance (t_data) (simple, fixed groups, fixed individual models)

Indep. variables	X choice (months in the forest) during all communication rounds		
	Simple OLS	Fixed effects (groups)	Fixed effects (individuals)
Intercept	2.3967 (0.0001)	1.6539 (0.0001)	0.7749 (0.0670)
ΔsumX(reduction) by other 7 players between $(t-1)$ and t[a]	– 0.0235 (0.0379)	– 0.0244 (0.0308)	– 0.0271 (0.0026)
Wealth$_i$	0.8601 (0.0001)	1.1560 (0.0001)	8.5718 (0.0001)
Wealth distance to other 7 players[b]	1.2299 (0.0001)	1.8223 (0.0001)	0.9197 (0.1373)
Wealth[a] wealth distance	– 0.4094 (0.0001)	– 0.5760 (0.0001)	– 3.2542 (0.0001)
Fixed effects: no. dummies not reported	0	9	72
N (including all 10 groups)	688	688	688
Adj. R^2	0.0949	0.1054	0.4342
F-test (p-value)	0.0001	0.0001	0.0001

[a] Defined as DELTSUM7= [lag(SUMX7) – SUMX7]⇒If DELTSUM7>0 then the other 7 players reduced their months in the forest from the previous round. Thus, if the estimated coefficient is negative, a reduction by the others is accompanied by a reduction by player i, implying a reciprocity effect.

[b] WLTHD2SA=@abs(Wealth$_i$ – Σwealth$_j$/7), $j \neq i$.

Therefore, a natural next step in the analysis would be to estimate a model that explains the individual choices during this communication phase as a function of a dynamic adjustment or a learning process, and of the nonmaterial incentives created by the environment of trust and reciprocity emerging from the conversations among these eight people in each round.

The results of such regression models are shown in Table 5, where we estimate the individual x_i choice (months in the forest) during all rounds in the communication phase as a function of (i) the behavior by the others in the group in previous rounds and (ii) the individual wealth and wealth distance between the individual and the rest of the group. In the case of the dynamic effect, we can provide a test for the hypothesis that players free-ride or reciprocate as rounds go by and they observe what the others do. Basically, if a player responds to others' behavior as an opportunistic free-rider (or homo-economicus), she should increase her months in the forest in round t, if the rest of the group had decreased their months in the forest by round $(t-1)$. On the other hand, if a player responds according to a strategy of reciprocity, she should decrease her months in the forest when the others decreased theirs, and increase if the other seven players increase as a negative reciprocity response. Notice the incentives to free-ride as one moves towards the upper parts of the payoffs table Appendix A used for the experiment. For testing such reciprocity hypothesis, we can construct a variable that measures in each round the change in the sum of months in the forest for the other seven players from the previous round, and include it as an explanatory variable for individual behavior. Reciprocators should decrease their extraction as a response to the others' cooperation, while free-riders should increase theirs[9].

The proxy used for wealth distance is simply the absolute value of the difference between one's wealth and the average wealth of the other seven players in the group. Thus, this analysis presumes each individual knows his/her relative wealth in comparison to group members. This approach is supported by the closeness in which these participants spend their daily lives in relation to other participants in the same village. We have also included the cross-effect of wealth and wealth distance to test whether the marginal effect of wealth distance on behavior might be different for individuals in different positions in the wealth scale; in other words, whether the effect of the distance effect is the same for rich than for poor individuals, an argument that also emerged during the workshops and interviews.

The regressions run with data at the individual level, and using three different estimation procedures, a simple ordinary least squares and two fixed effects models, are shown in Table 5[10]. The results confirm on the one hand the reciprocity effect in the response in each round, and also suggest that individuals who were part of more

[9] If the change is positive (Lagged(SumX_{-j}) $-$ SumX_{-j}), it means that the other seven players as a group decreased their extraction of the forest, i.e. were willing to cooperate. If a player responds to such signal by decreasing her months in the forest, it suggests a reciprocity effect, which would also apply to a player decreasing her months if the change by the rest of the group was to be negative.

[10] Recent concerns on experimental data have been emerging regarding the independence of observations, and that there might be group fixed effects for every group of players, or individual fixed effects for a number of decisions by one individual. Therefore, the three models, simple OLS, fixed group effects and fixed individual effects, were estimated for the same data set.

heterogeneous groups and who had higher levels of economic wealth chose significantly higher "months in the forest" at the end of the communication phase, i.e. were less willing to cooperate. Further, there is no evidence of a trend towards the predicted Nash equilibrium based on purely selfish maximization of payoffs, and in fact, there is no statistical difference, within groups, of a difference in behavior between the beginning and the end of the second phase under communication[11].

The negative and significant coefficient of about 0.025 for the variable (ΔsumX) measuring the change in behavior in the last rounds by the rest of the group implies that if the others decreased their sum of months by, say, 10 units, the average player reduced her x_i by 0.25 months. A positive coefficient is what the homo-economicus hypothesis would have predicted that a reduction in the extraction by the others should be followed by an increase in extraction by each player, up to the point of the symmetric Nash equilibrium.

In the case of individual wealth which shows a positive and significant effect on X, "months in the forest", at least two—nonexclusive—explanations seem plausible. The first argument is that lower levels of wealth can be associated with greater familiarity with similar dilemmas in real life, given that less private alternatives increase the marginal value of collective ones such as using joint access natural resources for self-consumption or selling in the market, and therefore such individuals should have a better understanding of the gains from mutual cooperation. In another paper (Cardenas, 2001), I discuss how for these same 10 groups one can correlate the social efficiency achieved at the end of the experiment with the percentage of players with their main income being from extractive activities, or negatively with the percentage of players having land as their main income source. A second possible explanation for a positive sign between wealth and level of free-riding is that wealthier participants may show smaller marginal utilities from the cash earned in the experiment, and therefore their marginal net utility from the effort to promote and enforce a cooperative agreement would be much lower, or the marginal value of potential losses is again smaller than for the poorer participants[12].

The hypothesis that wealth distance between a player and the rest of her group could affect negatively cooperation is confirmed by the positive sign of such coefficient in Table 5, i.e. as the distance in wealth increases, so did the level of free-riding (increase in x_i), and the negative and significant sign for the cross-effect reinforces the argument that such negative effect in cooperation was stronger for players in the lower levels of wealth within the groups.

The great variation in the types of dialogues recorded during the sessions can illustrate these arguments in a qualitative way, but lack of space here impedes us to

[11] Also, there is no evidence either that behavior in the first phase under no communication affected individual decisions in the second phase, controlling for other factors. If we compare the average decisions by the last three rounds of the first phase and the first three in the communication phase, we obtain a correlation coefficient, using individual players data, of 0.2621 only.

[12] Thanks to Jaime Forero (Universidad Javeriana) for raising this possibility. The tapes show a great variability in the effort that participants put into the conversations round after round for generating an agreement, and following-up its fulfillment in the next round. However, it is not possible to identify each participant in the tapes to its socioeconomic characteristics, since we did not labeled or tagged participants for the video recording to allow a more informal setting for the discussions.

expand on this statistical finding, except for the anecdote mentioned before regarding the group where players were not following the advice by a wealthy participant. In a further exercise using qualitative data analysis techniques on these audio and video data, Lopez (2001) explored, using 'focus groups' techniques, the presence or absence of the main elements that the experimental literature has suggested as explanatory of why face-to-face communication increases cooperation in group dilemmas. Her results illustrate how the groups that achieved the higher levels of social efficiency showed dialogues that were more direct and horizontal among members of more homogenous groups.

5. Final conclusions and comments

The results presented here expand the evidence on how wealth and group heterogeneity may affect the possibilities of cooperation in groups by showing how actual individuals' wealth and wealth distance affect cooperation in the experiment by reducing the effectiveness of communication within the groups. However, the analysis departs from the usual study of inequality and wealth affecting material outcomes and payoffs structures, and explores how wealth may influence other nonpecuniary aspects of cooperation and group dilemmas. In the case of experimental evidence, the results here suggest that people may bring into the game some aspects regarding their wealth and wealth relations to others, and that they might use such information strategically in the decision-making (Cardenas and Ostrom, 2001). Do people bring this information about their real context into the field lab, and do they use it for their decision? We believe so and provide statistical support for it.

However, it should not be concluded from here that wealth and wealth heterogeneity should always reduce cooperation in local commons dilemmas. Rarely in rural settings, the nonmaterial incentives arising from wealth emerge without other forms of material benefits and costs involved in the use of the commons, and these have inconclusive effects on cooperation as the literature reviewed in Introduction suggests. Most likely, wealthier households in communities have also different stakes at the commons dilemma, and their willingness to cooperate will at the end depend on their valuation of the net material and nonmaterial incentives. The same holds for the less wealthier who also face similar balances of incentives. The poor might also show large stakes at the game if their lack of economic assets makes them highly dependent on extracting resources at rates beyond the capacity of the resource to renew itself.

By isolating the factors discussed in this experimental research, we can also derive some implications for better policy design. Through a better understanding of the composition of groups, one can forecast levels of cooperation emerging from self-governance institutions and focus attention on intervening in settings where inequality may create more difficulties for communities to solve the dilemma.

Finally, there are methodological lessons from this exercise, and in particular, the advantages of bringing the experimental lab to the field and learn from observing the wider variance in certain characteristics of the participants in the subject pool, if compared to college students as usually done.

Acknowledgements

This paper would not exist without the ideas and stimuli from Samuel Bowles and Cleve Willis. Also, special thanks are given to Jeff Carpenter, Elinor Ostrom, John Stranlund and James Walker. Very useful comments by two referees, and discussants at the Workshop's Colloquia (Indiana University), the MacArthur Network on Norms and Preferences, the Public Choice/ESA 2000 meetings, and the Tokyo GDN2000 Global Development Network Conference Research Medals Competition, are greatly appreciated. In Colombia, I must thank the field practitioners and fellows from Humboldt, WWF and Fundacion Natura who helped pre-test and conduct the experiments. Very special thanks go to Luis Guillermo Baptiste and Sarah Hernandez at Humboldt, Carmen Candelo at WWF and Juan Gaviria, Nancy Vargas and Danilo Salas with Natura at the time. Financial support for the fieldwork was provided by the MacArthur Foundation, the Instituto de Investigacion de Recursos Biologicos Alexander von Humboldt (Colombia), the WWF Colombian program, Fundacion Natura-Colombia. Funding at later stages of the research by Resources for the Future and MacArthur Research and Writing Grant is also appreciated.

Appendix A. Payoff table

Their months in the forest	My months in the forest									
	0	1	2	3	4	5	6	7	8	
0	619	670	719	767	813	856	896	933	967	0
1	619	669	717	764	809	851	890	926	959	1
2	617	667	714	760	804	845	883	918	950	2
3	615	664	711	756	798	838	875	909	940	3
4	613	660	706	750	792	831	867	900	929	4
5	609	656	701	744	784	822	857	889	917	5
6	605	651	695	737	776	813	847	877	905	6
7	600	645	688	729	767	803	836	865	891	7
8	595	638	680	720	757	792	824	852	877	8
9	588	631	672	711	747	780	811	838	862	9
10	581	623	663	700	735	768	797	823	846	10
11	573	614	653	689	723	755	783	808	830	11
12	565	605	642	678	711	741	768	792	813	12
13	556	594	631	665	697	726	752	775	795	13
14	546	583	619	652	683	711	736	758	776	14
15	536	572	606	638	668	695	719	739	757	15
16	525	560	593	624	653	678	701	721	737	16
17	513	547	579	609	636	661	683	701	717	17
18	501	534	565	594	620	643	664	681	696	18
19	488	520	550	578	603	625	645	661	674	19
20	475	506	535	561	585	606	625	640	653	20
21	461	491	519	544	567	587	605	619	630	21
22	447	476	502	527	548	567	584	597	608	22
23	433	460	485	509	529	547	563	575	585	23

Appendix A (*continued*)

Their months in the forest	My months in the forest									
	0	1	2	3	4	5	6	7	8	
24	418	444	468	490	510	527	541	553	561	24
25	402	428	451	472	490	506	520	530	538	25
26	387	411	433	453	470	485	498	507	514	26
27	371	394	415	434	450	464	476	484	490	27
28	355	377	396	414	430	443	453	461	466	28
29	338	359	378	395	409	421	431	438	442	29
30	322	341	359	375	389	400	409	415	418	30
31	305	324	341	355	368	378	386	392	394	31
32	288	306	322	336	347	357	364	368	371	32
33	272	288	303	316	327	335	341	345	347	33
34	255	270	284	296	306	314	319	323	324	34
35	238	253	266	277	286	293	297	300	300	35
36	221	235	247	257	265	272	276	278	278	36
37	205	218	229	238	245	251	254	256	255	37
38	189	200	211	219	226	231	233	234	233	38
39	173	184	193	201	206	211	213	213	212	39
40	157	167	175	182	188	191	193	193	191	40
41	142	151	159	165	169	172	174	173	171	41
42	127	135	142	148	152	154	155	154	152	42
43	113	120	126	131	134	136	137	136	133	43
44	99	106	111	115	118	119	119	118	115	44
45	86	92	96	100	102	103	103	101	99	45
46	73	78	82	86	87	88	88	86	83	46
47	61	66	69	72	73	74	73	71	68	47
48	51	54	57	59	60	61	60	58	55	48
49	40	44	46	48	49	48	47	45	43	49
50	31	34	36	37	38	37	36	34	32	50
51	23	25	27	28	28	28	27	25	23	51
52	16	18	19	20	20	19	18	17	15	52
53	10	12	12	13	13	12	11	10	8	53
54	6	7	7	7	7	7	6	5	4	54
55	2	3	3	3	3	3	2	2	1	55
56	0	1	1	1	1	1	0	0	0	56

Appendix B. Theoretical model for the experimental design

The payoffs for the experiments were generated by a simple model of a fixed number of homogenous individuals where agents were instructed to view the game as if the problem was to exploit a local forest for firewood. In each round of the game, each individual is given an endowment of time e that can be allocated between collecting firewood and providing labor to an unrelated market. Let x_i denote the amount of time individual i spends collecting firewood from the common, and let w denote the prevailing wage for labor. Then, i's decision to provide $(e-x_i)$ units of labor to the formal sector yields a payoff of $w \times (e-x_i)$. Time spent collecting firewood from the forest yields a private benefit, which we assume takes the form $g(x_i)=\gamma x_i - \phi (x_i)^2/2$, where γ and ϕ are strictly positive and are chosen in part to guarantee $g(x_i)>0$, for $x_i \in [1,e]$. The strict concavity of $g(x_i)$ indicates diminishing marginal private returns to time spent collecting firewood.

Subjects were told explicitly that their decision to spend time extracting firewood would also affect water quality in the area adversely, for instance, because of erosion and sedimentation at the upper watershed. We assumed that water quality q is a quadratic function of the aggregate amount of time individuals in the community spend collecting firewood; specifically, $q(\Sigma x_j) = q^0 - (\Sigma x_j)^2/2$, where q^0 is interpreted to be water quality in the absence of firewood extraction. Again, these parameters are chosen in part to guarantee $q(\Sigma x_j)>0$ for all feasible Σx_j. An individual's valuation of water quality is $f(\Sigma x_j) = q(\Sigma x_j)$.

Define $u(x_i,\Sigma x_j)$ to be the sum of the sources of utility for an individual exploiter of the local forest. Parameters were chosen, in part, to guarantee that $u(x_i,\Sigma x_j)>0$ for all possible x_i and Σx_j. To facilitate scaling individual payoffs, we take an individual's payoff function to be a positive, monotonic transformation F of u. In particular, $F(u) = k(u)^\eta$, where k and η are all positive constants. An individual's payoff function is then

$$U_i\left(x_i, \sum x_j\right) = k\left[\left(q^0 - \left(\sum x_j\right)^2/2\right) + \left(\gamma x_i - \phi(x_i)^2/2\right) + w_i(e - x_i)\right]^\eta \qquad (1)$$

Each group consisted of $n = 8$ subjects, and each subject was allocated $e = 8$ units of time in each round. Pre-testing of the experimental designs at the University of Massachusetts and at the Humboldt Institute for Biodiversity in Villa de Leyva, Colombia led us to denominate units of time as months per year. Scale concerns led us to choose the following final parameter values: k=(4/16,810), $\eta = 2$, $q^0 = 1372.8$, $\gamma = 97.2$, $\phi = 3.2$, $w_i = 30$ and $e = 8$. Individual payoffs were therefore calculated from the payoff function:

$$U_i\left(x_i, \sum x_j\right) = (4/16810)\left[\left(1372.8 - \left(\sum x_j\right)^2/2\right) + \left(97.2x_i - 3.2(x_i)^2/2\right) + 30 \times (8 - x_i)\right]^2 \qquad (2)$$

Subjects were given the table of payoffs Appendix A as a function of individual choices and the choices of all other participants. In each group, all subjects received the same payoff table, and they were notified of this so that this was common knowledge.

Nash strategies and the balance between self-interested and other-regarding behavior. Because extracting firewood generates a pure public bad in the form of lower water quality, standard theory predicts that purely self-interested individuals will spend more time harvesting firewood than is socially optimal. Indeed, one common reference point for experiments of this type is the one-shot, complete-information Nash equilibrium (the standard model of purely self-interested strategic behavior) and another is the outcome at which group welfare is maximized. Since the player's payoffs are identical, optimality requires symmetric individual choices. Let x denote the common amount of time each individual spends collecting firewood in any symmetric outcome. Using Eq. (1), the joint welfare function is $W(x) = n(k)[(q^0 - (nx)^2/2)+(\gamma x - \phi(x)^2/2) + w \times (e - x)]^\eta$. The first-order condition for the maximization of $W(x)$ requires $-xn^2 + \gamma - \phi x - w = 0$. Solving for x and substituting the actual parameter values yields optimal individual amounts of time spent harvesting firewood, $x^* = (\gamma - w)/(\phi + n^2) = 1$. That is, if all eight players choose 1 month in the forest, the Pareto optimal solution is achieved, generating $645 in earnings for each player in one round. The equivalent FOCs for the Nash symmetric equilibrium corresponds to the case when each player chooses $X^{nash} = 6$, with earnings in a round of $155.

Appendix C. Experiment instructions (English translation)

These instructions were originally written in Spanish and translated from the final version used in the field work. The instructions were read to the participants from this script below by the same person during all sessions. The participants could interrupt and ask questions at any time.

Whenever the following type of text and font, e.g. [...MONITOR: distribute **PAYOFFS TABLE** to participants...] is found below, it refers to specific instructions to the monitor at that specific point, when in italics, these are notes added to clarify issues to the reader. Neither of these was read to participants. Where the word "poster" appears, it refers to a set of posters we printed in very large format with the payoffs table, forms and the three examples described in the instructions. These posters were hanged in a wall near to the participants' desks and where the eight people could see them easily.

COMMUNITY RESOURCES GAME (Instructions)

Greetings...

We want to thank every one here for attending the call, and specially thank the field practitioner _____(*name of the contact person in that community*), and _____ (*local organization that helped in the logistics*) who made this possible. We should spend about two hours between explaining the exercise, playing it and finishing with a short survey at the exit. So, let us get started.

The following exercise is a different and entertaining way of participating actively in a project about the economic decisions of individuals. Besides participating in the exercise, and being able to earn some prizes and some cash, you will participate in a community workshop in 2 days to discuss the exercise and other matters about natural resources. During the day of the workshop, we will give you the earnings you make during the game. Besides a basic "show-up" prize for signing up and participating (examples: flash lamps, machetes, school kits, home tools), you will receive a cash bonus that will be converted into cash for purchases for your family. The funds to cover these expenditures have been donated by various organizations that support this study among which we have the

Instituto Humboldt, el Fondo Mundial para la Protección de la Naturaleza, y la Fundación Natura.

C.1. Introduction

This exercise attempts to recreate a situation where a group of families must make decisions about how to use the resources of, for instance, a forest, a water source, a mangrove, a fishery, or any other case where communities use a natural resource. In the case of this community ________ (name of the specific village), an example would be the use of firewood or logging in the ________ (name of an actual local commons area in that village) zone. You have been selected to participate in a group of eight people among those that signed up for playing. The game in which you will participate now is different from the ones others have already played in this community; thus, the comments that you may have heard from others do not apply necessarily to this game. You will play for several rounds equivalent, for instance, to years or harvest seasons. At the end of the game you will be able to earn some prizes in kind and cash. The cash prizes will depend on the quantity of points that you accumulate after several rounds.

C.2. The PAYOFFS TABLE

To be able to play you will receive a **PAYOFFS TABLE** equal to the one shown in the poster. [...MONITOR: show **PAYOFFS TABLE** in poster and distribute **PAYOFFS TABLE** to participants...].

This table contains all the information that you need to make your decision in each round of the game. The numbers that are inside the table correspond to points (or pesos) that you would earn in each round. The only thing that each of you has to decide in each round is the number of MONTHS that you want to allocate EXTRACTING THE FOREST (in the columns from 0 to 8).

To play in each round you must write your decision number between 0 and 8 in a yellow GAME CARD like the one I am about to show you. [...MONITOR: show **yellow GAME CARDS** and show in the poster...] It is very important that we keep in mind that the decisions are absolutely individual, that is, that the numbers we write in the game card are private and that we do not have to show them to the rest of members of the group if we do not want to. The monitor will collect the eight cards from all participants, and will add the total of months that the group decided to use extracting the forest. When the monitor announces the group total, each of you will be able to calculate the points that you earned in the round. Let us explain this with an example.

In this game, we assume that each player has availability of a maximum of 8 MONTHS to work each year extracting a resource like firewood or logs. In reality, this number could be larger or smaller but for purposes of our game we will assume 8 as maximum. In the PAYOFFS TABLE, this corresponds to the columns from 0 to 8. Each of you must decide from 0 to 8 in each round. But to be able to know how many points you earned, you need to know the decisions that the rest in the group made. That is why the monitor will announce in each round the total for the group. For instance, if you decide to use 2 months in the forest and the rest of the group together, add to 20 months in the forest, you would gain ____ points. Let us look at two other examples in the poster.

[...MONITOR: show poster with the THREE EXAMPLES...].

Let us look how the game works in each round.

C.3. The DECISIONS FORM

To play each participant will receive one green DECISIONS FORM like the one shown in the poster in the wall. We will explain how to use this sheet. [...MONITOR: show the **DECISIONS FORM** in the poster and distribute the **DECISIONS FORMS**...].

With the same examples, let us see how to use this DECISIONS FORM. Suppose that you decided to play 5 in this round. In the yellow GAME CARD, you should write 5. Also, you must write this number in the first column A of the decisions form. The monitor will collect the eight yellow cards and will add the total of the group. Suppose that the total added 26 months. Thus, we write 26 in the column B of the decisions form. [...MONITOR: In the poster, write the same example numbers in the respective cells...].

To calculate the third column (C), we subtract from the group total, MY MONTHS IN THE FOREST and then we obtain THEIR MONTHS IN THE FOREST which we write in column C. In our example, $26 - 5 = 21$. If we look at the PAYOFFS TABLE, when MY MONTHS are 5 and THEIR MONTHS are 21, I earn ___ points. I write then this number in the column D of the DECISIONS FORM.

It is very important to clarify that nobody, except for the monitor, will be able to know the number that each of you decide in each round. The only thing announced in public is the group total, without knowing how each participant in your group played. Let us repeat the steps with a new example. [...MONITOR: Repeat with the other two examples, writing the numbers in the posters hanging in the wall...].

It is important repeating that your game decisions and earnings information is private. Nobody in your group or outside of it will be able to know how many points you earned or your decisions during rounds. We hope these examples help you understand how the game works, and how to make your decisions to allocate your MONTHS in each round of the game. If at this moment you have any question about how to earn points in the game, please raise your hand and let us know. [...MONITOR: pause to resolve questions...].

It is very important that while we explain the rules of the game you do not engage in conversations with other people in your group. If there are no further questions about the game, then we will assign the numbers for the players and the rest of forms needed to play.

C.4. Preparing for playing

Now write down your player number in the green DECISIONS FORM. Write also the place ___ and the current date and time __/__/__, __:__am/pm. In the following poster, we summarize for you the steps to follow to play in each round. Please raise your hand if you have a question. [MONITOR: Read the steps to them from the poster].

Before we start, and once all players have understood the game completely, the monitor will announce one additional rule for this group. To start the first round of the game, we will organize the seats and desks in a circle where each of you face outwards. The monitor will collect in each round your yellow game cards. Finally, to get ready to play the game, please let us know if you have difficulties reading or writing numbers and one of the monitors will seat next to you and assist you with these. Also, please keep in mind that from now on no conversation or statements should be made by you during the game unless you are allowed to. We will have first a few rounds of practice that will NOT count for the real earnings, just for your practicing of the game.

DECISIONS FORM

	Column A	Column B	Column C	Column D
Round No.	**MY MONTHS IN THE FOREST** (From your decision)	**TOTAL GROUP MONTHS IN THE FOREST** (Announced by the Monitor)	**THEIR MONTHS IN THE FOREST** [Column B minus Column A]	**MY TOTAL POINTS IN THIS ROUND** (Use your PAYOFFS TABLE)
Practica				
1				
2				
Total				

GAME CARD (Example):

GAME CARD	
PLAYER NUMBER:	1
ROUND NUMBER:	
MY MONTHS IN THE FOREST:	

COMMUNITY RESOURCES GAME
(Summary Instructions)

Objective of the game: To earn as much points as possible at the end of the rounds, which will be converted into cash prizes for your household.

How is it played: In each round, you must decide how many months in a year between 0 and 8, you want to devote to extract resources from a forest. The points you earn in each round depend on your decision and the decisions by the rest of the group, according to the PAYOFFS TABLE (blue table).

What do you need: To play you need a blue PAYOFFS TABLE, a green DECISIONS FORM and several yellow GAME CARDS. Also you need a player number.

Steps to play in each round:

(1) Using the blue PAYOFFS TABLE, decide how many MONTHS IN THE FOREST you will play.
(2) In the DECISIONS FORM write your decision (MY MONTHS IN THE FOREST) in Column A for the round being played at that moment.
(3) In a yellow GAME CARD write the round number, and your decision MY MONTHS IN THE FOREST. Make sure it corresponds to the DECISIONS FORM. Hand the yellow game card to the monitor.
(4) Wait for the Monitor to calculate the total from all the cards in the group. The Monitor will announce the TOTAL GROUP MONTHS.
(5) In the green DECISIONS FORM write this total in Column B (TOTAL GROUP MONTHS IN THE FOREST).
(6) In the green DECISIONS FORM calculate Column C (THEIR MONTHS IN THE FOREST) equals to Column B minus Column A.
(7) In the green DECISIONS FORM write in Column D the total points you earned for this round. To know how many points you made, use the PAYOFFS TABLE and columns A and C (MY MONTHS and THEIR MONTHS). We will also calculate this quantity with the yellow cards to verify.
(8) Let us play another round (Go back to step 1).

Rule A: THERE IS NO COMMUNICATION WITHIN THE GROUP
Besides the rules described in the instructions that we just explained, there is an additional rule for the participants in this group:

You will not be able to communicate with any member of your group before, during or after you make your individual decision in each round. Please do not make any comment to another participant or to the group in general. After the last round we will add the points you earned in the game.

Rule B: COMMUNICATION WITH MEMBERS OF THE GROUP
Besides the rules described in the instructions that we just explained, there is an additional rule for the participants in this group:

Please make a circle or sit around a table with the rest of your group. Before making your decision in each round, you will be able to have an open discussion of maximum 5 min with the members of your group. You will be able to discuss the game and its rules in any fashion, except you cannot use any promise or threat or transfer points. Simply an open discussion. The rest of the rules hold.
We will let you know when the 5 min have ended. Then you will suspend the conversation and should make your individual decision for the next round. These decisions will still be private and individual as in the past rounds and cannot be known to the rest of the group or other people.

References

Alesina, A., La Ferrara, E., 1999. Participation in Heterogenous Communities. NBER Working Paper No. 7155. National Bureau of Economic Research.

Baland, J.-M., Platteau, J.-P., 1996. Halting Degradation of Natural Resources: Is there a Role for Rural Communities? Oxford Univ. Press, New York.

Baland, J.M., Platteau, J.P., 1997a. Coordination problems in local-level resource management. Journal of Development Economics 53, 197–210.

Baland, J.M., Platteau, J.P., 1997b. Wealth Inequality and Efficiency in the Commons: the Unregulated Case, Oxford Economic Papers, vol. 49.

Bardhan, P., 1993. Symposium on management of local commons. Journal of Economic Perspectives 7 (4), 87–92 (Fall).

Bardhan, P., 2000. Irrigational Cooperation: An Empirical Analysis of 48 Irrigation Communities in South India. Economic Development and Cultural Change, vol. 48 (4). University of Chicago Press, pp. 847–866. July.

Bardhan, P., Dayton-Johnson, J., 2002. Unequal irrigators: heterogeneity and commons management in large-scale multivariate research. National Research Council (US). The Drama of the Commons. National Academy Press, Washington, DC.

Bardhan, P., Bowles, S., Gintis, H., 2000. Wealth inequality, wealth constraints and economic performance. In: Atkinson, A., Bourguignon, F. (Eds.), Handbook on Income Distribution, Elsevier.

Bergstrom, T., Blume, L., Varian, H., 1986. On the private provision of public goods. Journal of Public Economics 29, 25–49.

Berkes, F. (Ed.), 1989. Common Property Resources: Ecology and Community-Based Sustainable Development. Belhaven, London.

Cardenas, J.C., 2000. Rural Institutions, Poverty and Cooperation: Learning from Experiments and Conjoint Analysis in the Field. Doctoral Dissertation. Department of Resource Economics. University of Massachusetts Amherst. Amherst.

Cardenas, J.C., 2001. How do groups solve local commons dilemmas? Lessons from experimental economics in the field. Environment, Development and Sustainability, vol. 2, Nos. 3–4. Kluwer Academic Publishing, pp. 305–322.

Cardenas, J.C., Ostrom, E., 2001. What Do People Bring into the Game? How Information Helps to Reduce the Tragedy of the Commons. Working Paper W01-4. Workshop in Political Theory and Policy Analysis, Indiana University.

Cardenas, J.C., Stranlund, J.K., Willis, C.E., 2002. Economic asymmetry and the provision of local environmental quality. Ecological Economics, vol. 40, Issue 3, March. Elsevier.

Chan, K., Mestelman, S., Moir, R., Muller, R.A., 1996. The voluntary provision of public goods under varying income distributions. Canadian Journal of Economics XXIX (1), 54–69 (1998).

Crawford, S.E.S., Ostrom, E., 1995. A grammar of institutions. American Political Science Review 89 (3), 582–600 (September).

Dayton-Johnson, J., 1997. Rule and cooperation on the local commons: theory with evidence from Mexican farmer-managed irrigation systems. Department of Economics, University of California, Berkeley. Mimeo. November.

Dayton-Johnson, J., Bardhan, P., 2002. Inequality and conservation on the local commons: a theoretical exercise. Economic Journal 112 (481), 577–602.

De Janvry, A., McCarthy, N., Sadoulet, E., 1998. Endogenous provision and appropriation in the commons. American Journal of Agricultural Economics 80 (August), 658–664.

Hackett, S., Schlager, E., Walker, J., 1994. The role of communication on resolving commons dilemmas: experimental evidence with heterogeneous appropriators. Journal of Environmental Economics and Management 27 (2) (September), 99–126.

Hoffman, E., McCabe, K., Smith, V.L., 1996. Social distance and other-regarding behavior in dictator games. American Economic Review, June 86 (3), 653–660.

Kollock, P., 1998. Transforming social dilemmas: group identity and co-operation. In: Danielson, P.A. (Ed.), Modelling Rationality, Morality and Evolution. Oxford Univ. Press, New York, pp. 185–209.

Kramer, R.M., Brewer, M.B., 1984. Effect of group identity on resource use in simulated commons dilemma. Journal of Personality and Social Psychology 46 (5), 1044–1057 (May).
La Ferrara, E., 1998. Inequality and Participation: Theory and Evidence from Rural Tanzania. Mimeo.
Lawler, E., Yoon, J., 1996. Commitment in exchange relations: test of a theory of relational cohesion. American Sociological Review 61 (February), 89–108.
Ledyard, J.O., 1995. Public goods: a survey of experimental research. In: Kagel, J.H., Roth, A.E. (Eds.), Handbook of Experimental Economics. Princeton Univ. Press, Princeton, New Jersey, pp. 111–194.
Lopez, M.C., 2001. Comunicación para La Cooperación en el Medio Rural: Un Análisis desde la Economía Experimental". Tesis de Grado, Maestría en Desarrollo Rural. Universidad Javeriana, Bogota.
Molinas, J., 1998. The determinants of cooperation: the impact of inequality, gender, external assistance and social capital on local-level cooperation. World Development, vol. 26 (3). Elsevier, pp. 413–431. March.
Olson, M., 1965. The Logic of Collective Action: Public Goods and The Theory of Groups. Harvard Univ. Press, Cambridge, MA.
Ostrom, E., 1990. Governing the Commons: the Evolution of Institutions for Collective Action. Cambridge Univ. Press, Cambridge.
Ostrom, E., 1998. A behavioral approach to the rational choice theory of collective action. American Political Science Review 92 (1), 1–22 (March).
Ostrom, E., 2000. Collective action and the evolution of social norms. Journal of Economic Perspectives 14 (3), 137–158 (Summer).
Ostrom, E., Gardner, R., Walker, J., 1994. Rules, Games and Common-Pool Resources. University of Michigan Press, Ann Arbor.
Sandler, T., 1992. Collective Action: Theory and Applications. University of Michigan Press, Ann Arbor.
Schmitt, P., Swope, K., Walker, J., 2000. Collective action with incomplete commitment: experimental evidence. Southern Economics Journal 66 (4) (April 2000), 515–533.
Varughese, G., Ostrom, E., 2001. The contested role of heterogeneity in collective action: some evidence from community forestry in Nepal. World Development, vol. 29 (5). Elsevier, pp. 747–765.