Effective community mobilization: evidence from Mali.*

Maria Laura Alzua,[1] Juan Camilo Cardenas,[2] Habiba Djebbari,[3]

[1]Universidad de la Plata,

[2]Universidad de los Andes

[3]Aix-Marseille Univ., CNRS, EHESS, Central Marseille, IRD, AMSE, Marseille, France

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Abstract

Experts argue that adoption of healthy sanitation practices such as hand washing and latrine use requires focusing on the whole community rather than on individual behaviors. According to this view, one limiting factor for ending open defecation lies in the capacity of the community for collective action: each member of a community bears the private cost of contributing by washing hands and using latrines but benefits through better health outcomes depend on whether other community members also opt out from open defecation. We rely on a community-based intervention carried out in Mali as an illustrative example (Community Led Total Sanitation or CLTS). Using a series of experiments conducted in 121 villages and designed to measure the willingness of community members to contribute to a local public good, we investigate the process of participation in a collective action problem setting. Our focus is on two types of activities: (1) gathering of community members to encourage public discussion of the collective action problem and (2) facilitating the adoption of individual actions to attain the socially-preferred outcome. When the facilitator starts by introducing a topic and a group discussion follows, can the facilitator further improve outcomes? Will a group discussion that follows facilitation improve, reduce, or have no effect on collective action? We find evidence that cheap talk raises public good provision and that facilitation by a community member does not improve upon open discussion.

JEL Classification: H41, O12, C93, Q56
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1 Introduction

Overall disappointment with top-down policy led to the inclusion of participatory approaches in development programs (Chambers, 1983). Community-based programs rely for the most part on community mobilization to encourage behavioral change (parent teaching associations, village health committees, community-based targeting of social programs, etc.). Possible mechanisms for success include social learning, emergence of shared norms, exploitation of local complementarities. Community mobilization can then generate support for a local development issue and a reordering of priorities that result in immediate actions. Failure may occur because the public arena is captured by interest groups opposed to change or because of lack of capacity to plan actions to implement the desired change. Existing evidence on the effectiveness of community-based development projects is rather mixed (Mansuri and Rao, 2004, 2012). Results may not only depend on local capacity for collective action (Cameron et al., 2022) but also on the process of participation.

Our objective is to empirically investigate the process of participation. We rely on a community-based sanitation program carried out in rural Mali called Community Led Total Sanitation (CLTS) to construct our hypotheses and design our research protocol. This participatory approach to sanitation is widely used throughout the developing world. CLTS aims to "trigger" behavioral change towards healthier sanitation practices by relying on community mobilization Kar and Chambers (2008).[1] The principle is to mimic as much as possible a spontaneous endogenous process of participation with minimal inducement by a facilitator external to the community. In the Mali CLTS, we identified two processes. First, participation is expected through the convening a community meeting during which an external facilitator introduces the subject (sanitation) and let a public discussion take place. The external facilitator then identifies a "community champion" who clearly understand the socially-desirable goal (putting an end to open defecation) and individual actions needed to attain it (using toilets equipped with a slab and a hand-washing device). The community champion is asked to direct the discussion, explain the goal and means to reach it.

[1] In this community-based program, there are no funds or in-kind benefits to allocate across projects or households, and the role of external facilitators is minimal.

We assess the overall effect of the undirected and directed group discussions that characterize CLTS community mobilization process. We also investigate the sequence of activities matters: will directed group discussion followed by an undirected one be as effective than undirected group discussion followed by a directed one? The latter sequence is the one CLTS uses during community mobilizations. If strong opponents to change capture the undirected discussion, a directed discussion that follows the undirected one may fail to convince group members to cooperate. Even if we remain agnostic about the underlying mechanisms, in real-life settings, undirected group discussions are likely to arise after community mobilization, potentially undermining the gains from the community mobilization. Another key question is whether undirected group discussion is less effective than the directed one. This would be expected if lack of capacity to identify and plan actions to implement the desirable outcome is a stronger barrier to collective action than the belief that others will free-ride.

To test our hypotheses, we designed and conducted a a series of experiments in the field in 121 villages in Mali. Our experiment is designed to offer a framework that approximate that of the community mobilization of a CLTS program. With these experiments, we measure the willingness of community members to contribute to a local public good. Participants make decisions in groups of average size 23 (1 per village). During the first round, community members make their contributions without communicating. In the next two rounds, two "communication treatments" are introduced: undirected group discussion and directed group discussion. Discussions in these two rounds last for the same time before participants privately make their decisions (decisions are kept confidential throughout the experiment). In the directed group discussion, a community member (game participant) is randomly selected by the experimenter. This "community champion" is instructed on the group optimal solution and asked by the experimenter to share the information with the rest of her group. We randomly manipulate the order under which groups of participants are exposed to the communication treatments.

Community members are 8.6 percentage points more likely to contribute to a local public good under communication, when proportion of contributors under the no-communication condition is 72 percent, a significant 14 percent increase. Based on within-subject comparison, contributions increase by 7.1 percentage points from the no-communication round to the first communication round and by 10 percentage points from the no-communication round to the last communication round. We test whether there is a differential effect of the two communication condition due to the order at which subject were treated (directed first followed by undirected or the opposite). We find no significant order effects at any round. Using between-subject data pooled across random order assignment, we find no evidence that directed communication result in higher contributions than undirected communication. We thus reject the two hypothesis we set up to test: (1) directed communication is not more effective than undirected communication, (2) the impact of communication is invariant to the order at which directed and undirected communication take place.

The study contributes to our understanding of participatory approach in a sanitation intervention by testing hypotheses related to mechanisms that may explain its success. The existing literature assessing participatory approaches to development in general Casey (2018), and community-based sanitation programs in particular Venkataramanan et al. (2018), offer various explanations for success or failure but limited evidence on these mechanisms. This study also makes a methodological contribution by offering a new way of using lab-in-the-field experiments in development research. They increase the external validity of controlled lab experiments findings by recruiting a non-standard subject pool who make decisions that are contextualized by the environment in which they are made (see, e.g., Aflagah et al. (2022); Cardenas and Carpenter (2008)). They are also fruitfully combined to larger social experiments to in order to provide a measure of social capital that can be used to measure the impact of an intervention (REF). To the best of our knowledge, this is the first study that designs the lab-in-the-field experiment to approximate key features of an intervention

in order to test them.

An important remark is in order before drawing any policy implication. The design of our experiment differ from the group discussions held during CLTS community mobilization in one important dimension. In CLTS, the community champions are not randomly selected by the external facilitator; they self-select into this role. They are likely to differ from the randomly-selected group member in our experiment in terms of charisma and social position in the community. In the experiment, we abstract away from the "quality" of the community champion. In this sense, the external validity of the findings are limited. The absence of a differential effect between directed and undirected group discussion may not translate to CLTS.

This study offers several insights on the workings of a prototypical participatory approach. Group discussions held during the community mobilization result in higher propensity for collective action that may explain the success of CLTS. But there is no evidence that community members need to be provided guidance of how to attain the collectively-preferred outcome. This suggests that lack of capacity to identify and plan actions to reach a collective goal is not a significant barrier to cooperation. Triggering of a public discussion is as effective as the more intrusive directed discussions. Spontaneous group discussions that can be expected to take place after the intervention is over are not expected to undo the gains from community mobilization.

2 CLTS sanitation intervention as an exemplary illustration of a participatory approach

Experts argue that adoption of good sanitation practices requires focusing on the whole community rather than on individual behaviors (Kar and Chambers, 2008). According to this

view, the limiting factor for ending open defecation is neither informational, nor technical or financial, but rather lies in the capacity of the community for collective action. CLTS was first implemented in Bangladesh in 1999 and is now present in more than 50 countries in the global South. At least 20 countries adopted CLTS as their national sanitation strategy for rural areas. CLTS has wide support by international organizations (e.g., World Bank, UNICEF, WaterAid) as an effective participatory solution to improve sanitation.

Throughout this paper, we rely on a community-based intervention carried out in Mali called Community Led Total Sanitation (CLTS) as an illustrative example.[2] Using data from a RCT designed to assess the effectiveness of the intervention, we found massive improvements in household sanitation as well as evidence of improved child growth (Pickering et al. 2015). In another study based on this intervention, we found that information regarding health gains from safe sanitation plays no role. There is no difference in knowledge about the risks associated with poor sanitation in intervention and non-intervention villages. In both groups, households were already quite knowledgeable about these risks (Alzua et al., 2020). Gertler et al. (2022) combines data from different locations (India, Indonesia, Mali and Tanzania) to analyze the effects of variants of CLTS. All but the Mali experiment show modest impacts. Unlike in the other countries, CLTS in Mali was implemented in remote rural areas.

In Mali, the CLTS intervention was implemented by the government with support from UNICEF. The objective of CLTS is to end open defecation. To attain this objective, everyone in the community has to have access to a private latrine with a cover that is equipped with a hand washing station (bucket with water and ashes or soap). Typically, external facilitators in charge of the program gather the community for 3-5 hours with the objective of triggering the adoption of healthy sanitation practices. During this initial gathering of the community, a number of activities are conducted to raise awareness on the risks associated with open

[2] referencing the main findings in our two previous papers

defecation and help develop a plan to build latrines; see Alzua et al 2020 for details of these activities.

Community members are not told what to do or how to achieve it. Rather, through the activities (walk of shame, mapping of OD places...), the facilitator aims to make salient the costs of unhealthy sanitation and hygiene practices and prompt community members to become sanitation champions. This initial gathering of the community is followed by a period of intensive monitoring to encourage progress towards goals agreed upon (building, repairing and using latrines). Finally, when the initial plan is met, the community is certified as "Open-Defecation Free" with a celebration that is valued by community members.

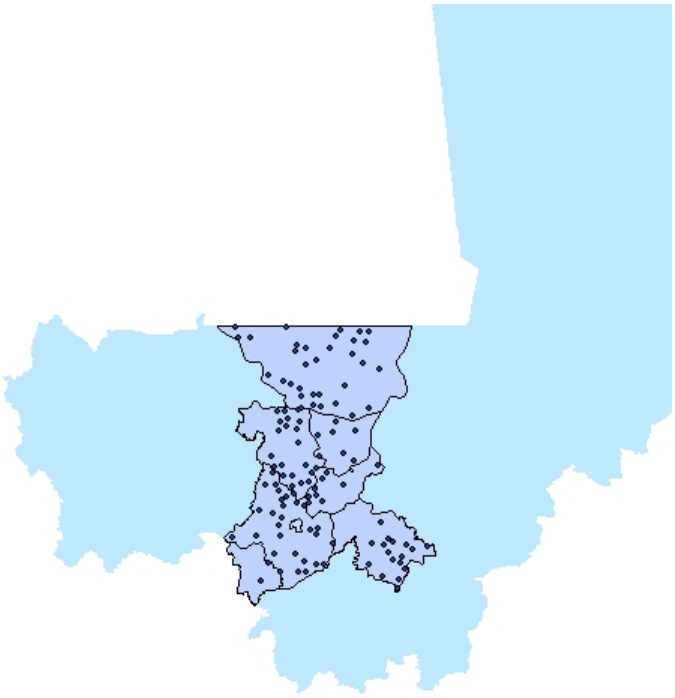
CLTS is a relevant illustrative example of a participatory intervention aiming at solving a classic example of a collective action problem, whereas each member of the community bears the private cost of contributing by building and using latrines and the benefits through better health outcomes depend on what the rest of the group do. The benefits of improved sanitation are higher the higher the proportion of people in the community adopting better sanitation practices.

3 Study design

3.1 Study participants

Participants to the experiment are drawn from study villages sampled for the purpose of assessing the impact of CLTS in Mali. The sample includes 121 small rural villages from the Koulikoro region with low latrine coverage and no sanitation program in place. Figure 1 shows the map of Mali with the province of Koulikoro highlighted. The dots indicate the location of the study villages. A timeline for the intervention and research activities is provided in the Appendix (Table A.1).

Figure 1: Study villages in the province of Koulikoro, Mali.



In each of the villages, after completion of the endline survey (April-June 2013), the respondent played a lottery with 50% chances to receive an invitation for an experimental session. Attendance is voluntary and invited households choose a family member to represent them. Though households are randomly selected to participate to the games, selection within household is not expected to be random. But this actually reproduces the way most community-driven programs operate: households get invited to participate in community meetings, and it is the household who chooses who to send.

We conducted 363 sessions (3 per village) with about 3,000 players. Sessions gathers 23.4 players on average (Table A.2). Only 28% of game participants were male. Average age is 35. More than 75% of them are illiterate, and 85% of them never went to school. Illiteracy

raised a practical challenge when designing the experiment, see next section.

In the following, we first present the design and implementation of our experiment. We aimed to reproduce some features of the community mobilization process. Our main outcomes of interest are experimental contributions to a local public good. Our experimental units are individual community members.

3.2 Experimental design

The game under no-communication.

We designed a discrete public good game following Marwell and Ames (1979). The choice of a discrete version of the public good game is motivated by the challenges regarding literacy and for the sake of simplicity (Cardenas and Jaramillo, 2007). Participation does not require the use of pencil. There are two goods, a private one and a public one, and m participants. Each participant was invited to participate to this game under three treatment conditions. The base treatment condition is a standard public good game without communication. In the two other treatments, we allow for communication in the form of cheap talk. In the undirected group discussion, participants are allowed to talk to each other freely for five minutes. In the directed group discussion condition, a randomly selected participant is designated as the facilitator: the experimenter tells her what actions everyone should take in order to maximize the group payoff and she is instructed to convey this message to the rest of the group.

The experimenter provides each participant $i = 1, \ldots, m$ with one token. Choice set includes two actions $x_i = 0, 1$, to keep the token ($x_i = 0$) or to invest it in the public good ($x_i = 1$). If the token is kept, it yields a payoff p to player i only. If the token is invested in the group project, it yields a payoff of a to every player j including i. The payoff function is given by: $y_i = p(1 - x_i) + a(\sum_{j=1}^{m} x_j)$. Thus, the public good produced depends linearly on

each individual decision to contribute. All decisions are made simultaneously and privately, without knowing what others will do. Assuming participants only care about their monetary payoffs, and as long as $a < p$, there will be no incentives to contribute to the group account, i.e., $\forall i = 1, \ldots, m : x_i^{Nash} = 0$, resulting in a socially inefficient outcome. In this case, each player obtains $y_i = p$, and the group outcome is $\sum_{i=1}^{m} y_i = mp$. However if every player contributes to the group account, i.e. , $\forall i = 1, \ldots, m : x_i^{soc.opt.} = 1$, then the social optimum is obtained. In this case, the earnings for each player are $y_i = ma$, and the group outcome is $\sum_{i=1}^{m} y_i = m^2 a > mp$. In our experiment, we set $a = 1$ and $p = 10$. At least 11 participants attended the experiment in each village. We refer to this game as the base game.

The communication conditions

The communication rounds follow a first round of the public good game during which participants are not allowed to communicate with each other before making their individual contribution decision. Each participant is then exposed to two variants of the base game for which communication is allowed. In the **undirected communication** condition, participants can have a 5-minutes discussion before privately making their individual contribution decision. In the **directed communication** condition, a "community champion" is randomly chosen among participants. He is instructed to moderate a 5-minutes discussion and explicitly told that when every participant contributes the group maximizes its gains.[3] After the 5 minutes participants privately make their contribution decision. Because we expected that the order at which groups of participants were exposed to the two communication conditions may matter for contributions, we randomly manipulate their order across groups of participants (village-level randomization). Individual contributions are kept private and confidential until the end of the three rounds. At that time, the experimenter announces

[3]Transcript is as follows: "*The group of a whole gets the highest number of points if everyone contributes. The group gets the lowest number of points if noone does. You can now try to convince the other participants to contribute so that the group gets the highest number of points.*"

the total amount of contributions for each round and payments are made privately to each participant.

3.3 Framing and implementation of the experiments

There is a wide range of possibilities for presenting the games to participants, from an abstract game with no reference to a particular cooperation problem to a heavily framed situation that can hint players to the study's larger purpose, i.e., studying their behavior in order to understand how the community-driven sanitation program may have affected them. Here, we chose a weak framing that serves to have participants' mindset on collective action. This mild framing is meant for villagers to act according to past experience and underlying social norms.

Our public good games are framed as *foroba* games (i.e., common pot in the local language). The name given to the token is *niyoro*, also a Bambara term for a token used in common transactions. Use of *foroba* and *niyoro* as labels should remind them of a familiar setting in which people usually contribute to a common pot and get a valuable amount in return. We decided against framing the public good according to the sanitation issue that is central to the research project in order to not contaminate our results with specific issues with the intervention that took place. Yet, we maintain a weak framing to collective action.

Experimenters worked in teams of five, each individual with specific tasks to perform (see Appendix A.3). Given the number of experimental sessions, instructions to experimenters were made as simple as possible. Experimenters were trained on the protocol for 8 days.[4]

Illiteracy was a challenge we partly addressed by using a discretized version of the standard public good game. We distributed two paper tokens to participants at each round (see

[4]Full protocol available upon request.

Appendix Figure A.1): they had to privately choose to put down in the *foroba* either the piece of paper with the *niyoro* picture (i.e., to contribute) or the one without. We explained the base game extensively to make sure that players understood it. At the beginning of each session, the experimenter read the instructions and answer questions until she was sure that all participants fully understood. Participants knew they will be rewarded with prices according to the number of points accumulated during the session. Prices were valuable items not related to sanitation. An experimental session lasted for around 1 hours.

The games were incentivized: players earned points that were then translated into rewards. In order not to interfere with the sanitation intervention that took place in some of these villages, we converted the points into small valuable household items unrelated to sanitation (e.g. batteries, pens, paper pads, lighters) instead of providing monetary payments.

4 Empirical findings and discussion

We observe increases in the contribution rate through time from a level of 72% in the first round without communication (Figure 2). This is a relatively high rate of contributors compared to that from lab experiments with university students, but similar to the one obtained from lab experiments in the field in developing countries (Cardenas and Carpenter, 2008).[5] Though we do not provide feedback information between rounds on decisions made by others, contributions can be expected to decrease through time because of a learning effect. We find that the increase in cooperation level is greater at round 1 than round 2 as compared to the base round (Appendix Table A.4). These lower gains may be explained by the fact that participants are more experienced (learning effects), an interpretation consistent with the literature. The marginal gain from the second round of discussions (round 3 of the

[5]In Zimbabwe, Barr (2001) find that participants to a VCM contribute between 48 and 52% of endowment. In Vietnam and Thailand, it is respectively between 72 and 76% and between 61 and 73% (Carpenter et al. 2004). In Kenya, Ensminger (2000) find that people contribute 58% of their endowment. In Peru, Karlan (2005) finds that 81% of participants contribute in a threshold public game. Figures for the U.S. are lower (between 30-40%).

experiment) may also be lower because returns to communication are decreasing.

These lower marginal gains in cooperation may depend on the sequence of treatments through which participants were exposed, i.e., directed followed by undirected group discussion or the opposite. Since we randomized the order of this sequence across communities, we can test whether learning effects are different according to order assignment. We first check that order assignment is indeed random based on the data collected in the first round (no-communication or base condition). We cannot reject that it is not (Figure 3). But that order assignment is random does not preclude differential learning effects across the sequence of treatments. Rather, such design allows testing whether they are.

Does directed group discussion followed by an undirected one as effective than undirected group discussion followed by a directed one? If undirected group discussions are captured by selfish individuals, conditional cooperators may be discouraged to act collectively and this effect may undermine subsequent effort to direct the discussion towards the socially-optimal actions and outcome. Our mixed between- and within-subject design, unlike a pure between-subject one, allows testing for the sequence effect. We find no evidence that the sequence of activities matter in community mobilization (Table 4). There is no significant effect of order at the second round, nor at the third round. Both sequences are as effective at improving cooperation. This evidence also suggests that undirected group discussions that are likely to take place after the community mobilization activity will not affect the gains from it.

We now compare gains in contributions across communication conditions. Because we do not find evidence of order effects, these gains can be interpreted as causal (learning effects are the same on average across order). Restricting the sample to the communication rounds (Figure 4 and Appendix Table A.5), we find no significant difference in contribution rates under directed and undirected group discussion. Making salient the collective goal through

the voice of a community member does not lead to a higher outcome than simple undirected group discussions. Note that, since this community champion is randomly picked, we are measuring the effect of the information keeping "leader" quality constant. Our evidence thus suggests that participants do not lack capacity to identify and plan actions to implement the desirable outcome. In CLTS, community champions are not randomly selected and it is possible that they are better at convincing the other community members to act collectively than the random member. As such, the absence of a differential effect between directed and indirected group discussion questions the relevance of directed discussions.

We find strong gains in cooperation under communication (pooled directed and undirected communication conditions) as compared to the base game with no communication (Figure 5). This is consistent with the findings from the existing literature on public good games. Community members are 8.6 percentage point more likely to contribute to the *foroba* when group discussion is allowed. This represents a significant 14% change in the contribution rate.

We also estimate the effects of the communication conditions across rounds according to the sequence order experienced. (Figure 6). We find that directed group discussion, by making salient the best course of action for all, does improve cooperation over undirected group discussion. Looking at the left-hand side panel, we find a significant 5 p.p. increase in fraction of contributors from the undirected group discussion round to the directed one. In contrast, when undirected group discussions follows directed ones, there is only a small and not statistically significant change in the fraction of contributors (right-hand side panel). Directed and undirected discussion result in a similar contribution rate (8 p.p. compared to 10 p.p. increase).

5 Conclusions

In this paper, we report experimental findings from a series of public good games played in the field in conditions designed to mimic the operations of a community-based intervention. Our paper thus contributes to a large literature discussing the benefits and limitations of participatory approaches (Casey 2018).

We examined the workings of participation in the widely-praised CLTS sanitation intervention. Community mobilization is the key activity in this intervention. We identified two distinct processes in the community mobilization for the Mali CLTS: (1) "triggering" by an external facilitator who introduces the subject (sanitation) and let an undirected public discussion take place; (2) delegation by the external facilitator of her the authority to direct the group discussion to a "sanitation" champion. We find no support for the claim that making salient the collective goal through the voice of a community member leads to a higher outcome than simple undirected group discussions. This, in turn, suggests that lack of capacity to identify and plan actions to reach a collective goal is not a significant barrier to cooperation. We also randomly manipulated the order of sequence of directed and undirected discussions. Even if strong opponents to collective action capture undirected discussions, there is no evidence that community members get locked into a low cooperation level that cannot be improved with guidance on how to reach the collective goal. [further research]

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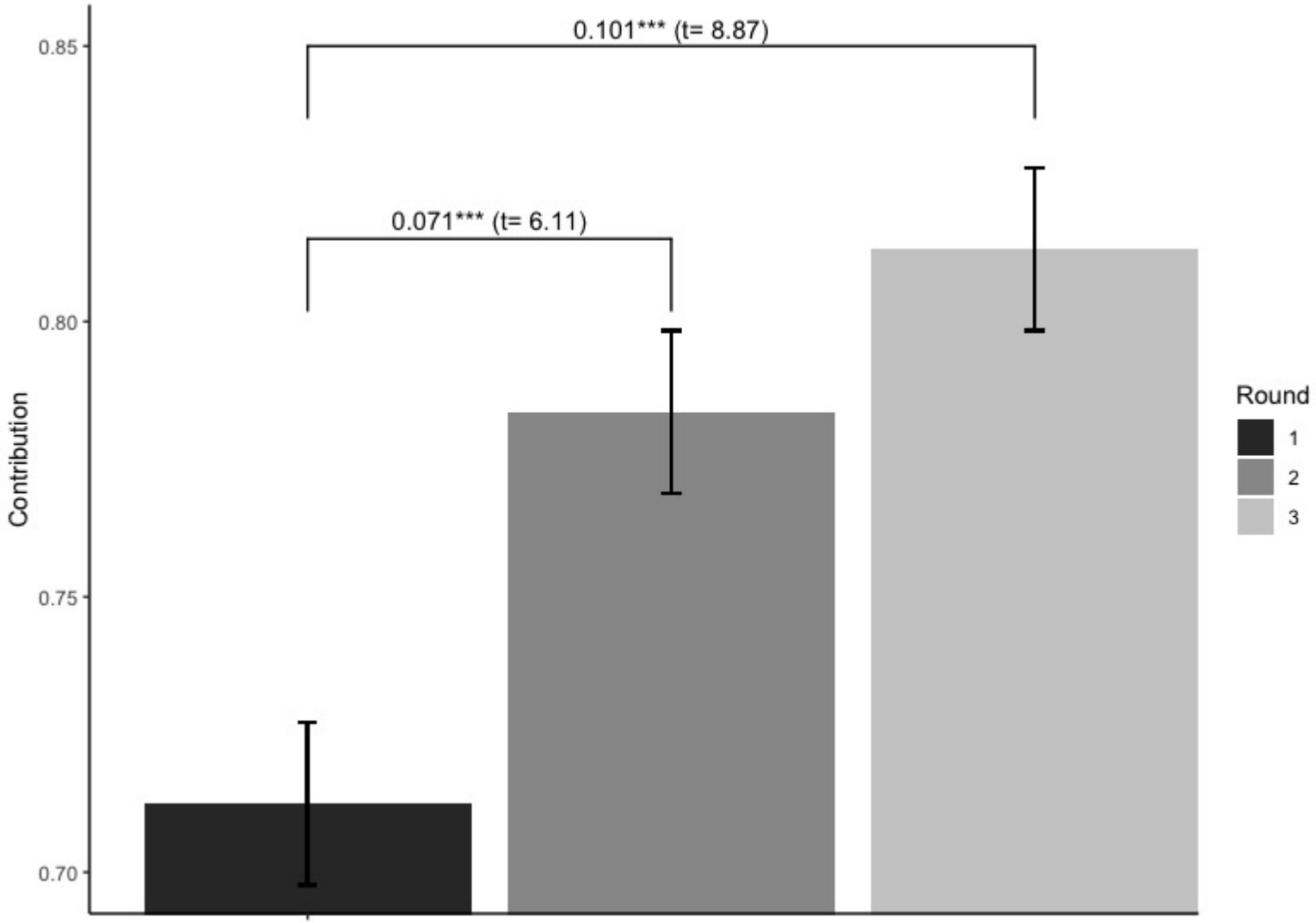
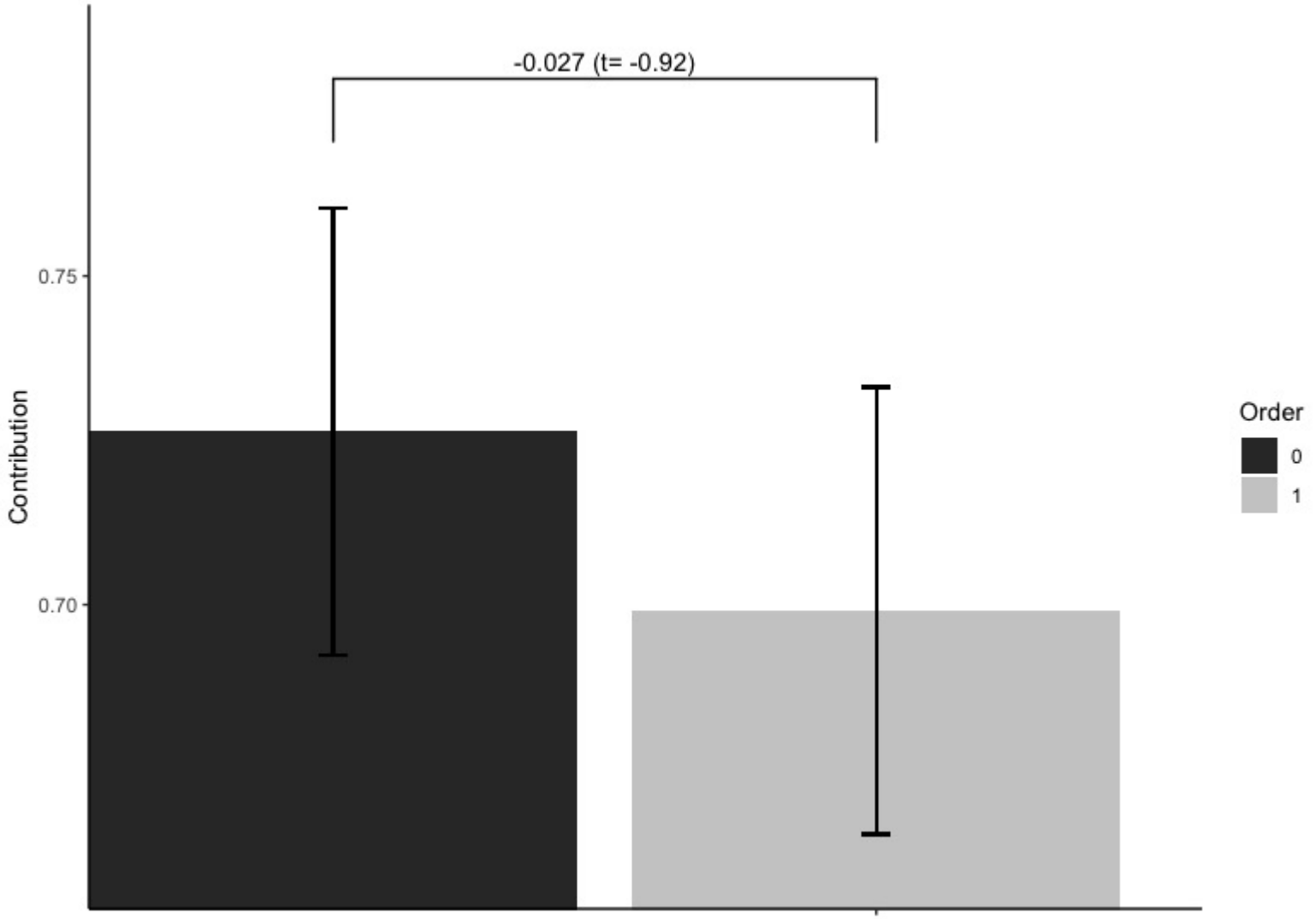
Figure 2: Average contributions across rounds.



Note: Round=1 for no-communication base condition; Round=2,3 for pooled cheap talk treatments (Directed+Undirected discussions). Robust SE.

Figure 3: Balance test: average contributions across order assignment at base condition.



Note: All the data used for the estimation come from Round 1 (no-communication base condition). Order=1 for villages exposed to directed then undirected group discussions and Order=0 for villages exposed to undirected then directed group discussion. Village-level clustered SE.

Notes: ***,** and * indicate statistical significance at the 1, 5 and 10 percent level. Standard errors clustered at the village level in parenthesis. The dependent variable is an indicator of whether the participant contributes to the public good.

Table 1: Sample comparison: all adults vs. game participants.

	All adults	Game participants
Speak bambara	0.76	0.73
	(0.43)	(0.44)
Head of household	0.14	0.24
	(0.35)	(0.43)
Spouse of the head	0.15	0.69
	(0.35)	(0.46)
Other household member	0.71	0.07
	(0.45)	(0.26)
Sex: male	0.52	0.25
	(0.50)	(0.43)
Muslim	0.57	0.86
	(0.49)	(0.34)
Married	0.31	0.93
	(0.46)	(0.25)
Main activity: farming	0.29	0.24
	(0.45)	(0.42)
Main activity: herding	0.06	0.02
	(0.23)	(0.13)
Main activity: merchant	0.03	0.03
	(0.16)	(0.18)
Main activity: construction	0.12	0.07
	(0.32)	(0.26)
Main activity: forestry	0.04	0.06
	(0.20)	(0.23)
In wage labor	0.01	0.01
	(0.11)	(0.12)
In family labor	0.70	0.62
	(0.46)	(0.49)
Self-employed	0.29	0.37
	(0.45)	(0.48)
Hours worked in the past week	31.64	37.06
	(16.08)	(16.29)
No. observations	25,436	2,242

Table 2: Comparison: random leader vs. other game participants.

	Random leader	Other participants
Speaks bambara	0.73	0.76
	(0.44)	(0.43)
Head of household	0.26	0.22
	(0.44)	(0.42)
Spouse of the head	0.66	0.69
	(0.47)	(0.46)
Other household member	0.08	0.09
	(0.27)	(0.29)
Sex = male	0.28	0.26
	(0.45)	(0.44)
Religion = muslim	0.86	0.88
	(0.35)	(0.33)
Married	0.92	0.91
	(0.27)	(0.29)
Household assets index	0.44	0.44
	(0.15)	(0.15)
Farm assets index	0.57	0.58
	(0.20)	(0.21)
Livestock index	21.64	24.71
	(33.37)	(42.44)
Locus of control	-0.28	-0.27
	(0.60)	(0.65)
Social capital index	2.89	2.86
	(0.71)	(0.75)
# of organizations to which household belongs	2.07	2.14
	(1.20)	(1.37)
# of position of power held by household in village	0.75	0.79
	(1.02)	(0.99)
=1 if anticipates everyone else to contribute	0.46	0.43
	(0.50)	(0.50)
=1 if contribute to foroba (no communication)	0.72	0.71
	(0.45)	(0.46)

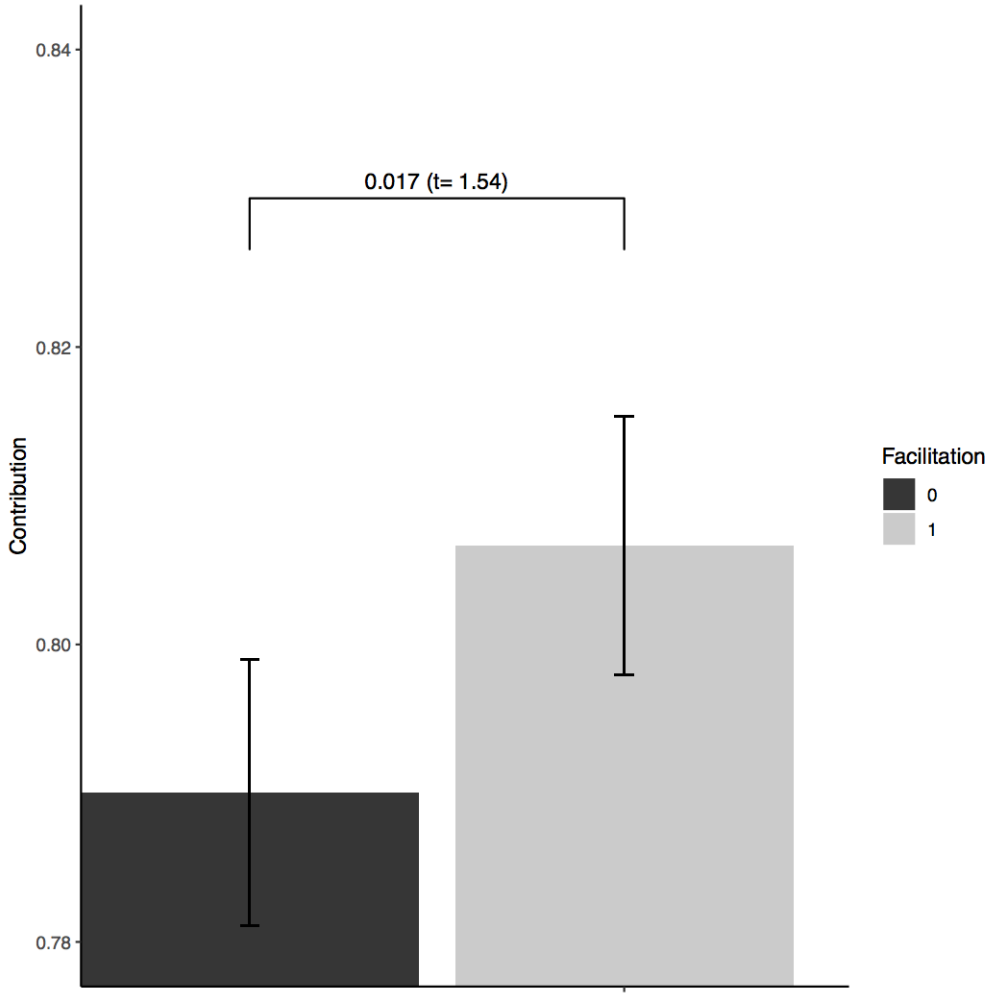
Table 3: Testing randomness of leader.

	(1)
Speaks bambara	0.0191
	(0.0311)
Head of household	-0.0161
	(0.0247)
Spouse of the head	0.0103
	(0.0251)
Sex = male	0.0181
	(0.0198)
Age	0.000385
	(0.000440)
Religion = muslim	0.0140
	(0.0145)
Married	-0.0158
	(0.0227)
Household assets index	0.0115
	(0.0411)
Farm asset index	-0.0123
	(0.0308)
Livestock index	0.0000840
	(0.000169)
Locus of control	0.00174
	(0.00903)
Social capital index	-0.00720
	(0.00712)
# of organization to which hh belongs	0.00402
	(0.00565)
# of position of power held by hh in vill	-0.00225
	(0.00532)
=1 if anticipates everyone else to contribute	-0.0105
	(0.00954)
=1 if contributes in foroba (no communication)	-0.00320
	(0.0110)
Constant	0.0326
	(0.0430)
N	2253

Table 4: Testing for order effects: OLS estimation.

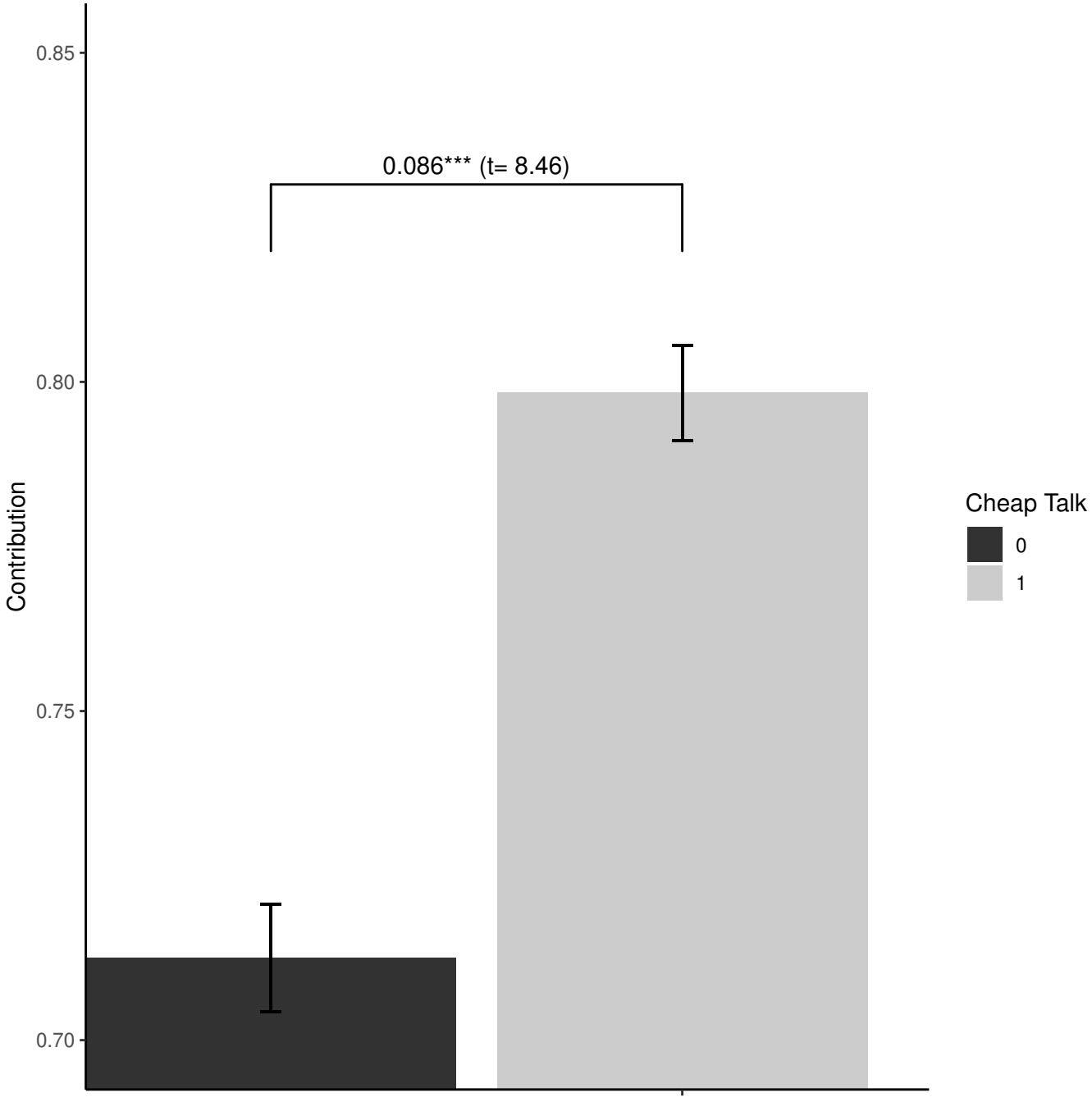
	(1)	(2)
Round 2	0.0711***	0.0550***
	(0.0137)	(0.0180)
Round 3	0.101***	0.102***
	(0.0151)	(0.0206)
Random order	-0.0176	-0.0273
	(0.0277)	(0.0297)
Round 2 * Random order		0.0316
		(0.0272)
Round 3 * Random order		-0.00262
		(0.0301)
Constant	0.721***	0.726***
	(0.0195)	(0.0207)
No. Obs.	8316	8316
p-value (order effects jointly equal to 0)		0.36

Figure 4: Average contributions across communication conditions.



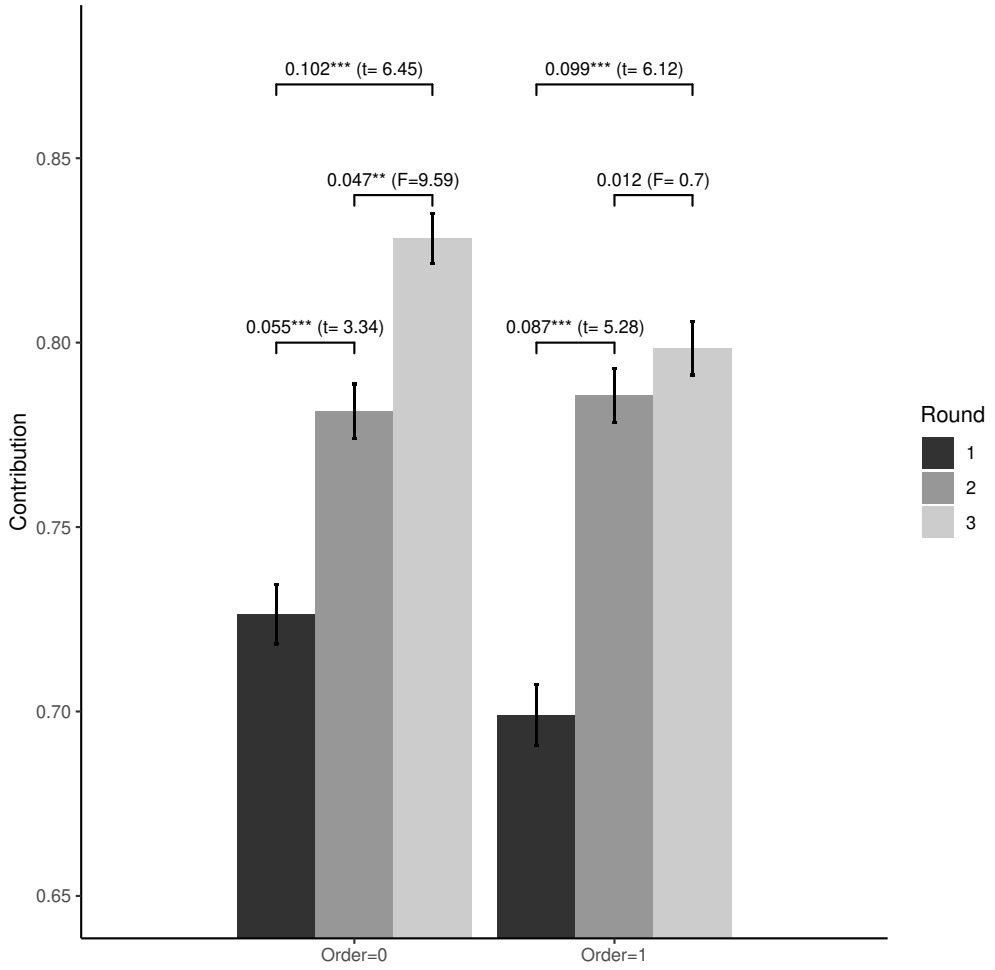
Note: Data restricted to communication rounds (Round=2 and Round=3). Facilitation=1 indicates **directed group discussion** (value =1 for Round=2 observations from villages exposed to Order=1 and Round=3 observations from villages exposed to Order=0). Facilitation=0 indicates **undirected group discussion** (value =1 for Round=2 observations from villages exposed to Order=0 and Round=3 observations from villages exposed to Order=1). Robust SE.

Figure 5: Average contributions: pooled cheap talk conditions vs. no communication.



Note: t-statistic of the difference (robust SE).

Figure 6: Average contributions across rounds by order treatment.



Note: Order=1 for villages exposed to facilitation (**directed group discussion**) then **undirected group discussion** and Order=0 for villages exposed to opposite order. Round=1 for no-communication base treatment; Round=2,3 for cheap talk treatment (if Order=0, Round=2 is **undirected group discussion** and Round=3 is **directed group discussion** ; if Order=1, it is the opposite. Village-clustered robust estimation.

Table A.1: Timeline of activities

Date	Activities
April-June 2011	Baseline experimental games and survey
September 2011-June 2012	Implementation of the CLTS intervention
April-June 2013	Endline experimental games and survey

Table A.2: Descriptives statistics on game participants.

	No.Obs.	Mean	SD
Literacy (1=read and write)	2860	0.15	0.35
Age	2967	35.7	12.2
Sex (1=Male)	2985	0.27	0.44
Mother language (Bambara=1)	2997	0.73	0.44
Number of players per village	2997	23.3	2.8

Figure A.1: Experiment material: paper token

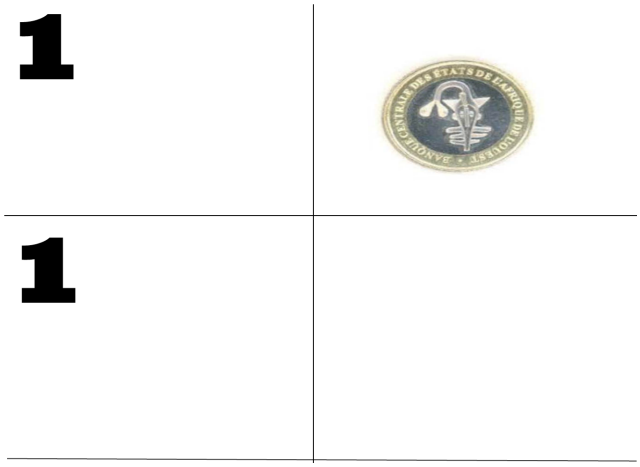


Table A.3: Composition of the experimenter team

Team member	Role
Recruiter	Keeping track of the identity of the participants
Facilitator	Explaining the games
Accountant	Determining and distributing material rewards
Observer	Checking that participants follow protocol
Supervisor	Supervising the team of experimenter

Table A.4: Average contributions across rounds and order.

	Round 1	Round 2	Round 3	Test	Difference	Statistic
Order = 0	0,726	0,781	0,828	H_0: $Y_0^2 = Y_0^3$	0.047**	F= 9.59
SD	-0,446	-0,413	-0,377	H_0: $Y_0^1 = Y_0^2$	0.055***	t= 3.34
No. obs.	4089	4089	4089	H_0: $Y_0^1 = Y_0^3$	0.102***	t= 6.45
Order = 1	0,699	0,786	0,798	H_0: $Y_1^2 = Y_1^3$	0.012	F = 0.70
SD	-0,459	-0,411	-0,401	H_0: $Y_1^1 = Y_1^2$	0.087***	t= 5.28
No. obs.	4227	4227	4227	H_0: $Y_1^1 = Y_1^3$	0.099***	t= 6.12

Notes: Order = 0 is **undirected group discussion** followed by **directed group discussion**; Order = 1 is the opposite. Y_j^i is the average contribution at round i for order j. Standard errors of the differences are clustered at village level.

Table A.5: Average contributions under facilitation and discussion.

	Average Contribution	SD	No. Obs.
Directed group discussion	0.807	0.395	8316
Round 2, Order=1	0.786	0.411	4227
Round 3, Order=0	0.828	0.377	4227
Undirected group discussion	0.790	0.407	8316
Round 2, Order=0	0.781	0.413	4089
Round 3, Order=1	0.798	0.401	4227
Test	H_0: Directed = Undirected		8316
Difference	0.017	0.011	
Statistic	t = 1.54		

Notes: Order = 0 is **undirected group discussion** followed by **directed group discussion**; Order = 1 is the opposite. Standard errors of the differences are clustered at village level.