Cheap Talk and Coordination in the Lab and in the Field: Collective Commercialization in Senegal

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Abstract

Most developing-country farms are small and engage in cooperative agriculture. Prior literature has argued that mechanisms aimed at facilitating smallholder coordination such as cooperatives are central to stimulating market participation. At the same time, cooperatives have not always been able to engage in collective action. In this paper, we conduct neutrally framed coordination games and a natural field experiment to test the effect of cheap talk among members of groundnut-producing cooperatives in Senegal. In both experiments, we ask farmers how much they intend to contribute to the group prior to them actually doing so and then, confidentially reveal aggregate intentions to other cooperative members. Based on survey and administrative data, we find that (1) revealing farmers' intentions improves collective commercialization and this effect increases with group size and (2) learning in the lab transfers to behavior in the day-to-day environment. Implications for policy and future work are discussed.

Keywords: Cooperatives, collective commercialization, coordination, cheap talk, field experiments, development
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1. Introduction

According to the United Nations, more than 12 percent of the world's population belongs to a cooperative and these organizations employ ten percent of workers.[1] In fact, most developing-country farms are small and family-run (e.g., Lowder et al., 2016) and many of them engage in cooperative agriculture.[2] Prior literature has thus argued that mechanisms aimed at facilitating smallholder coordination such as cooperatives are central to stimulating market participation (e.g., Ostrom, 1990; Barrett, 2008; Markelova et al., 2009; Wanyama, 2014). At the same time, cooperatives in low income countries have not always been able to engage in collective action (e.g., Fafchamps and Hill, 2005; Aldana et al., 2007; Hellin et al., 2007; Bernard et al., 2008; Ragasa and Golan, 2014).

In this paper, we primarily rely on a two-pronged empirical approach to test the effect of cheap talk on coordination among members of 79 groundnut-producing cooperatives in Senegal. We start by conducting neutrally framed coordination games (i.e., lab-in-the-field experiments/LFEs) with 839 randomly selected members of these cooperatives. We then conduct a natural field experiment (heretofore, the RCT) among the full set of 2800 cooperative members. In both the LFEs and the RCT, we ask farmers how much they intend to contribute to the group prior to them actually doing so. Our main treatment varies exposure to aggregate intentions, a type of cheap talk, in order to assess impacts on farmers' contributions to the group (in the LFEs) and sales through the cooperative (in the RCT). The combination of two types of field experiments in addition to survey and administrative data collected by the cooperatives, provides a fuller empirical test of our hypotheses. For example, the LFEs allow for variation in key parameters such as group size that cannot be exogenously manipulated in the day-to-day environment. Meanwhile, the RCT enables an exploration of farmers' actual commercialization behavior in the day-to-day environment as well as the effect thereon of farmer participation in the LFEs. The inspiration for this design was drawn from two sources. First, prior data from this context suggest that beliefs about others' sales to the cooperative are key to one's decision to sell through the group (e.g., Bernard et al., 2014, which we return to below).[3] Second, there is extensive theoretical and lab experimental literature suggesting that cheap talk communication can impact coordination (e.g., Farrell, 1987; Cooper et al., 1992; Crawford, 1998; Charness and Grosskopf, 2004; Blume and Ortmann, 2007; Avoyan and Ramos, 2017).

We contribute two key findings to the literature. First, revealing farmers' intentions to sell through the cooperative improves collective commercialization. The results are economically significant: On average, farmers exposed to cheap talk in the RCT contribute more than twice as much towards collective commercialization than in the control group. As predicted, this effect is more important in larger groups where coordination is more costly to start with. From a policy standpoint, this suggests that a relatively simple and non-costly information intervention can improve coordination within cooperatives and thus, market participation. This raises the question as to why such an institution did not emerge in absence of this intervention (e.g., Matsuyama, 1997). In order to address this, some additional context is needed. Bernard et al. (2014) document strategic uncertainty among members of 27 groundnut cooperatives

[1] Accessed on July 2, 2021: https://www.un.org/en/observances/cooperatives-day.

[2] Accessed on July 2, 2021: https://www.ifad.org/en/farmer-organizations.

[3] There may of course be other reasons why cooperatives are (un)able to sell collectively; e.g., Casaburi and Macchiavello (2019) and Hill et al. (2014) discuss commitment sanctions and deferred payments respectively.

in Senegal. Practically all respondents believe there are benefits to coordination because collective commercialization alleviates individual-level constraints such as lack of transportation and bargaining power (consistent with for example, Key et al., 2000; Bernard et al., 2008). At the same time, 67 percent of respondents believe that other members would by-pass the cooperative and sell individually to a trader for a lower payoff. This is likely because the majority of sales typically occur to local traders via spot market transactions, thus reinforcing the above beliefs.

Thus, one reason why this intention revelation institution may not have emerged is because communication needs to be N-way in order to impact farmers' beliefs. In other words, it must aggregate and disseminate information across *all* cooperative members who produce the crop in question. This is quite different from free-form, day-to-day communication which tends to be bilateral, particularly between cooperative members who routinely interact. Another reason may be that intentions were collected by a third party in an environment that is characterized by important interpersonal ties. So, participants might have been more willing to reveal and believe intended actions than if the process had been implemented by peers with potentially hidden agendas. While it is likely that broader market forces also drive commercialization (e.g., Ashraf et al., 2009), this intervention would not have had significant impacts if it had missed the mark. So, our findings leave room for future work to explore (1) the nuances of implementing information/communication institutions (e.g., Haaland et al., 2020) and (2) how such institutions interact with other interventions aimed at boosting market participation, contract farming, and entrepreneurship (e.g., Fafchamps and Minten, 2012; Saenger et al., 2014; Dimitriadis and Koning, 2019; Hussam et al., 2020; Rao and Shenoy, 2021).

Second, learning in the LFEs transfers to behavior in the RCT. Specifically, LFE participants are more likely to engage in collective commercialization relative to non-LFE participants from the same cooperative. From a methodological standpoint, this suggests that behavior in artefactual environments can transfer to behavior in naturally-occurring environments (consistent with for example Cardenas and Carpenter, 2005; Cooper and Van Huyck, 2016; Turiansky, 2017). Our study thus advances the literature that links results across the spectrum of experiments and other empirical approaches (e.g., Levitt and List, 2007; Barr et al., 2010; Finan and Schechter, 2012; Stoop et al., 2012; Voors et al., 2012; Camerer, 2015; Hoel et al., 2017). While our findings support the use of LFEs in combination with RCTs to better understand the mechanisms that are at play (e.g., Viceisza, 2016), they also suggest that researchers should take heed as participation in LFEs can affect subjects' future behavior. This is consistent with Zwane et al. (2011) who find that being surveyed can impact subsequent behavior, but also an older literature on the learning that can arise from participating in classroom experiments (e.g., a series of articles by Charles Holt and co-authors at *Journal of Economic Perspectives* in the late 1990s to early 2000s).

The remainder of the paper proceeds as follows. Section 2 discusses our framework and hypotheses in the context of existing literature. Section 3 details our study design and empirical strategy. Section 4 covers our main findings. Finally, Section 5 concludes by discussing potential implications for policy and future work.

2. Framework, hypotheses, and literature

2.1. Framework

As discussed in Section 1, practically all respondents in the Bernard et al. (2014) sample of 300 groundnut farmers believe there are benefits (i.e., higher payoffs) to collective commercialization. This is because coordination can reduce transportation costs and improve bargaining power (consistent with for example, Key et al., 2000). At the same time, 67 percent of farmers believe that other members would by-pass the cooperative and sell individually to a trader for a lower payoff.[4] These statistics point to the fact that collective commercialization in this context is characterized by significant strategic uncertainty. This leads to reinforcing beliefs that mitigate future attempts at collective commercialization, thus giving rise to further coordination failure.

Consistent with this evidence, the type of framework we have in mind is a critical-mass/threshold coordination game. There are $N \in \mathbb{N}$ players, each of whom j has a positive endowment $V_j \in \mathbb{N}$. All players simultaneously choose an amount $A_j \in [0, 1, ..., V_j]$ to send to the group (the equivalent of commercializing collectively) and keep the remainder $V_j - A_j$ (the equivalent of selling individually/via traders). Any quantity A_j sent to the group, earns a high return H if all players jointly send a quantity $A = \Sigma_j A_j \geq T$ to the group, where T represents some threshold (which can be thought of as the amount that the cooperative has negotiated in a contract). If they send a quantity $A < T$, A_j earns a low return L. Whatever a player chooses to keep individually, that is $V_j - A_j$, earns a medium return M, where $L < M < H$. This is consistent with farmers' beliefs about the benefits to collective commercialization.

Player i's expected payoff can be expressed as $\Pi(A_i) = pA_iH + (1-p)A_iL + (V_i - A_i)M$, where $p = P(A \geq T)$.[5] It is clear that equilibria in this game will be driven by player i's belief, p_i, about the likelihood of the threshold being surpassed, p. I.e., contributions to the group (how much the farmer chooses to commercialize collectively) will depend on the player's sense of strategic uncertainty. If they expect A to surpass T, $p_i = 1$, the player should send all of the endowment to the group, i.e., $A_i = V_i$. If they expect A not to surpass T, $p_i = 0$, the player should keep the full endowment, i.e., $A_i = 0$. If they expect a scenario in between these two extremes, the player should diversify by selling $A_i \in \{1, ..., V_i - 1\}$ through the cooperative.

As previously discussed, most cooperative members believe that others are more likely to sell individually to traders than collectively through the cooperative. In short, empirical evidence suggests that $p = 0$ or close to it. So, a key question is whether and if so, how p can be increased, given farmers believe it would be beneficial to do so. In order to state the main hypotheses that form the basis of the experimental design in Section 3, we further extend the above framework. Suppose there is a pre-stage to the game in which all players simultaneously reveal an *intended* action towards the group A'_j (i.e., N-way communication) prior to taking the "true" action A_j. Player i will use intentions A'_j to inform their belief p_i about p only if they believe those intentions. In particular, player i will substitute A'_{-i} for A_{-i} in order to more precisely assess the likelihood of surpassing the threshold T. So, we state the following hypotheses.[6]

[4] They conduct survey based coordination games along the lines of Heinemann et al. (2009).

[5] Here, we abstract from the more general "utility" function and assume that H, M, L capture players' true payoffs. In the empirical analysis, we check/control for preferences.

[6] While some of the literature on communication has suggested that players may seek to deceive others, particularly when sending non-costly

Hypothesis 1. Player i will set $p_i = p = 1$ and thus $A_i = V_i$ if revealed intentions are such that $\Sigma_j A'_j = A' \geq T$. In other words, cheap talk can reduce coordination failure if aggregate intentions surpass the threshold.

Hypothesis 2. As the group size N increases, coordination failure is more likely due to increased strategic uncertainty. In other words, smaller groups are more conducive to coordination.

Hypothesis 3. The effect of cheap talk (i.e., $\Sigma_j A'_j$ relative to T) will increase with the group size N. In other words, due to strategic uncertainty, cheap talk is more effective in larger groups. However, the direction of the effect will depend on whether or not aggregate intentions surpass the threshold. This hypothesis basically combines hypotheses 1 and 2.

2.2. Related experimental literature

There is a relatively extensive theory and lab literature on coordination (in games) and the potential determinants thereof (see for example Farrell, 1987; Ochs, 1995; Crawford, 1998; Goeree and Holt, 2002; Van Huyck and Battalio, 2008, for some overviews). The literature that relates most closely to our setting is on (1) coordination games with Pareto-ranked equilibria, which cover both order-statistic games (e.g., Van Huyck et al., 1990; Van Huyck and Battalio, 2008) and stag-hunt games (e.g. Cooper et al., 1992; Battalio et al., 2001; Charness and Grosskopf, 2004) and (2) preplay communication/cheap talk (e.g., Blume and Ortmann, 2007; Avoyan and Ramos, 2017, and some of the previous references). Devetag and Ortmann (2007) and Cooper and Weber (2019) survey some of this literature and both point at a range of common factors that may enhance coordination. Among those are (1) certain types of cheap talk/communication in certain types of games; (2) smaller group sizes; and (3) lower attractiveness of the secure action relative to the risky one. Kets and Sandroni (2017) address strategic uncertainty and cultural diversity.

3. Study design

As mentioned in Section 1, we are unable to exogenously manipulate two key parameters – the group size N and the threshold T – in the naturally-occurring environment. As such, group-size effects in the day-to-day setting could be capturing other (potentially unobserved) characteristics. For example, larger groups may (1) offer better services to farmers to begin with (e.g., input, credit, or negotiated prices); (2) be more likely to achieve a given threshold level, ceteris paribus; and (3) be associated with greater strategic uncertainty due to a greater number of "moving parts" (i.e., number of players).

Accordingly, our design is based on a two-pronged empirical approach. First, we fully test our predictions through controlled, neutrally framed LFEs in which we are able to observe and manipulate N and T as well as other aspects such as external uncertainty and the premium for coordination (H). Then, we test our results in a naturally-occurring

and non-binding messages, there is no such incentive in this game. Unlike Prisoner's dilemma/public-good type games, a player cannot "free-ride" by sending a high intention A'_j and then taking a low action A_j. If a player truly believes that a high signal will cause others to increase their contributions to the group, this player should align her/his actual contribution with such intentions. This is the same argument that has been made by for example Crawford (1998) – recall the reassurance role of cheap talk in stag-hunt games.

RCT where we do not manipulate the above parameters. The timing was as follows (Table A1 summarizes the relationship between the LFE and RCT samples):

1. *LFEs:* From May to June 2013, randomly selected farmers from 28 cooperatives participated in variants of neutrally-framed, high-stakes, threshold coordination games (Section 3.1). A presurvey collecting basic (behavioral) characteristics was also administered at this time.
2. *RCT:* From November to December 2013, 79 farmer groups (including 26 of the 28 that were in the LFEs) participated in variants of intention-revelation treatments (Section 3.2). Intentions were revealed 2-4 weeks prior to the start of the commercialization period.
3. *Post-survey and administrative data:* In June 2014, a follow-up survey was administered to 10-12 randomly selected members of each cooperative (median size is 24, ranging from 4-91). Administrative data on all individual members' commercialization through the cooperative during the 2014 season were also collected from the group's books/records.

3.1. LFEs

The LFEs were based on two main treatments (see online appendix for detailed instructions): (1) a baseline coordination game (BCG) and (2) the same coordination game but with intentions revealed (i.e., communication/cheap talk) prior to play (CCG). These treatments were randomly assigned across experimental groups of subjects, which were created by randomly drawing members from existing cooperatives to form sets of players of size N equal to 10 or 20. All players in a given experimental group were members of the same cooperative.[7] Table A2 gives the sample distribution across the BCG and CCG.

In the BCG, each player j had an endowment V_j of six chips. Each chip was worth 2000 West African francs/CFA (the equivalent of approximately 4 US dollars) if held individually. This was M. So, players were explained at the beginning of the game that they held an endowment of 12000 CFA. To mitigate windfall/house money effects, this endowment was framed as payment for the presurvey. The payoff for each chip sent to the experimental group was dependent on whether or not the threshold (T) was reached/surpassed. If $A \geq T$, each chip was worth 3000 CFA (the equivalent of H); if not, each chip was worth 500 CFA (the equivalent of L). So, each player had to decide how many of the six chips to send to the group (A_j) and how many to keep individually ($6 - A_j$), as shown in Figure A1.

The CCG was identical to the BCG with one exception: prior to choosing and committing to A_j, each player was asked to reveal their intended action A'_j to the experimenter; that is, how much the player planned to send to the experimental group. This intention, which was confidentially revealed to all other players $-j$ alongside the aggregate intention on a board at the front of the room, indicated a given player's likely action. However, it was not a binding commitment and as such, other players did not know with full certainty that A_j would be the same as A'_j. It is in this sense that it was cheap talk.

[7] All cooperatives that were selected for the LFEs had at least 10 members. For groups that had less than the 30 (=10+20) members, only one LFE session was conducted.

Apart from the BCG and the CCG, we randomly varied N, T, H, and the presence of external uncertainty as separate treatments. We varied H and the presence of external uncertainty because prior literature has shown that attractiveness of the secure versus the risky action may impact coordination (recall Section 2.1). We also varied T because in a day-to-day context (in particular, the RCT) group size N is likely to be correlated with other characteristics such as the perceived likelihood of achieving the threshold. So, experimental variation in T enables us to better understand what mechanism is at play.

To see this, consider the following: If the level of T is fixed, it should be easier to coordinate in large groups than in small groups, since the former are more likely to reach the threshold. So, communication should be *less* informative in large (relative to small) groups for a fixed level of T. On the other hand, if communication is *more* informative in large groups, despite the level of T being fixed, this must be because of greater strategic uncertainty surrounding other players' actions. Similarly, if the average (i.e., per-player) number of chips needed to surpass the threshold is fixed across small and large groups, but communication matters differentially across N, this also suggests that strategic uncertainty is the main mechanism at play. Since we are unable to (1) experimentally control T in the RCT and (2) fully isolate these mechanisms in the day-to-day environment, we thus rely on the LFEs as a crucial part of our design in order to argue mechanisms.

Two primary aids were used when explaining the game. First, monetary payoffs were explained by displaying actual CFA bills on a board at the front of the room, also making "real stakes" more salient. Second, many hypothetical examples were used. For example, the experimenter and his assistant as well as pairs of subjects role-played through different scenarios. We also tested subject understanding by asking specific players to calculate such payoffs. A substantial part of the LFE sessions was dedicated to the instruction phase.

In the CCG, the exact same procedure as in the BCG was followed, except that prior to subjects making their actual decisions (A_j), the experimenter went around the room and asked players in private to reveal their intended actions (A'_j). Subjects were explained that this information would be collected by the experimenter and confidentially displayed in random order on a separate board at the front of the room. It was made clear that this was an intended, but non-binding action. Figure A1 also shows the logic behind the CCG. It was identical to the BCG, except for an additional board (right panel of Figure A1), which contained randomly ordered intentions A'_j and the aggregate A'.

The BCG and the CCG were implemented between sessions (and thus subjects), since introducing intentions mid-session would have complicated the protocol. The (experimental) group size, N, was fixed at either 10 or 20 during a session. So, N was varied across sessions (and subjects). The threshold, T, was 40 or 50 in 10-person groups and 40, 50, 80, or 100 in 20-person groups. T was varied randomly across rounds. H was either 3000 or 2500 CFA per chip and was varied randomly across rounds. Whether or not there was external uncertainty was implemented as follows. Subjects were informed that there was a 50 percent chance that due to bad luck H would be 1500 CFA per chip (instead of 2500 or 3000). This was varied across rounds by flipping a coin.[8]

[8]Each session comprised: (1) a presurvey collecting basic information (available upon request); (2) an introduction covering the purpose of the session and the fact that participants would be paid for decisions made during the session; (3) four rounds of decisions with no feedback, followed by debriefing; (4) a postsurvey collecting other information (available upon request); and (5) payment in private based on one randomly selected round. The sessions lasted 2.5-3 hours and average earnings were 9,500 CFA (approximately 20 US dollars), relative to a daily wage equivalent in

3.2. RCT

We worked with 79 groundnut-producing cooperatives – including 26 of the 28 with which we implemented the LFEs – comprising close to 2800 individuals. These cooperatives are part of two umbrella federations (i.e., conglomerates of cooperatives) in the main groundnut production zone of Senegal. In November 2013, two leaders of each farmer group attended a two-day training conducted by two development specialists. The training focused on the potential, pitfalls, and conduct of collective commercialization; in particular, strategies for identifying distant buyers, negotiating prices, and organizing transportation. Participants were instructed to conduct a meeting with all cooperative members upon returning to their village, to report the gist of what was covered during the training. Trainees were also provided with standardized booklets to keep records of each member's contribution to the group's sales in the upcoming commercialization season. A reward of 10,000 CFA was promised for filling in the booklets with all the requested information. All groups eventually received such reward.

After the training, during December 2013, enumerators went to the villages in order to elicit commercialization intentions from all cooperative members. Prior to doing so, they made sure that the leaders who had taken part in the training had held the briefing meeting. For each farmer group, all members who produced groundnuts for the 2014 commercialization season were asked how they intended to use their production. They were told that the purpose of this survey was to better understand their decisions with regard to groundnut production. They were also informed that a subsequent group meeting would be held, where a message would be delivered to them. They were thus invited to attend that meeting.

The 79 groups were allocated to one of four conditions, depending on the information that would be disclosed in the subsequent meeting (Table A3):[9]

- In Condition A (the control group), members' intentions were not revealed. Enumerators said that a survey would be conducted after the end of the commercialization season. This was also announced in Conditions B-D.
- In Condition B, members' aggregate intentions were revealed.
- In Condition C, members' aggregate intentions as well as the distribution of intentions among members were revealed. I.e., how many members intended to contribute 100kg; how many intended to contribute 200kg, and so on. This was most comparable to the CCG in the LFEs.
- In Condition D, the same information as in C was revealed, but the distribution of intentions was disaggregated by ordinary members versus cooperative leaders (i.e., members who are part of the management committee). This treatment was inspired by the literature on leadership (e.g., Hermalin, 1998; Potters et al., 2007; Jack and Recalde, 2015; Kosfeld and Rustagi, 2015).

this region of 5,000 CFA. For additional details, see Aflagah et al. (2015).

[9] As discussed in some of the papers referenced in Section 2.2, messages have differential impacts depending on the "statistics"/content they convey. Hence, the logic for a variant of information treatments.

3.3. *Sample description*

An average of 10-12 randomly selected farmers per cooperative were surveyed before (time of eliciting intentions) and after commercialization. Table A4 describes the characteristics of such individuals and compares means across groups that received no information about others' intentions and groups that did. Consistent with the context described in Section 1, our sample essentially comprises small farmers, with a total farm size of less then five hectares on average. The full sample cultivates groundnuts, but 58 percent also produce other crops. Groundnut production is also relatively small in magnitude, with the previous and current year's average expected harvest below 1.7 tons. 84 percent of the farmers indicate that they intend to sell some of their groundnut harvest through the group in the upcoming commercialization season. On average, intended sales through the group amounted to 60 percent of farmers' production.

Overall, Table A4 does not show clear differences on pre-intervention characteristics across the two samples. There are, however, indications that generosity (as measured by a hypothetical dictator game) is higher on average for groups where no information was revealed. For a comparable groundnut-producing sample, Bernard et al. (2014) find that altruism is negatively related to group sales. So, in this case that might translate into an upward bias of the information effect. They find, however, that the associated coefficient is quite small in magnitude, in particular compared to the one associated with individuals' aversion to strategic uncertainty. Table A4 also indicates that a greater number of farmers whose group was selected to participate in the LFEs were in the control group. All RCT-related estimations include these and other variables among the controls.[10]

3.4. *Empirical strategy*

With the LFE and RCT protocols being comparable, we rely on a common estimation strategy to assess the impact of others' intentions on the decision to contribute to the group. Individuals are indexed by subscript i and groups by subscript g – where "groups" refers to experimental sessions in the LFEs and day-to-day cooperatives in the RCT. Our basic estimation is as follows:

$$A_{ig} = \alpha + \beta I_g + \varepsilon_{ig} \tag{1}$$

where being exposed to others' intentions, which varies at the group level, is captured by a binary variable I_g (our proxy for A' in Section 2.1), and the associated parameter β measures its effect on the dependent variable of interest (more below). ε_{ig} is a composite error term defined as:[11] $\varepsilon_{ig} = \mu_g + \xi_{ig}$ where μ_g is a group-specific error and ξ_{ig} is the remaining idiosyncratic one. With group members' decisions to contribute to the group being strategic complements, we allow for within-group, individual errors to be correlated. Thus, all our standard errors are clustered at the group-level.[12]

[10] Table A5 reports the same tests across Conditions A-D. The results are similar.

[11] In the estimating equations that follow, ε_{ig} will vary depending on the specification. But, for simplicity we keep the same notation.

[12] We rely on the cluster-correlated Huber-White covariance matrix method to compute cluster-robust standard errors. While our design includes a reasonably large number of clusters, we also computed standard errors based on more conservative randomized inference tests. Results are available upon request, and similar to those reported here.

While the same estimator is used for both the LFEs and the RCT, interpretation of the coefficients differs across the two. In LFE-related estimates, the β parameter is the treatment effect on the treated. In fact, all individuals selected to participate in the game did participate and no one left before all rounds were completed (although that was allowed if they wanted to). In the RCT however, only 59 percent of group members attended the meeting in which aggregate intentions were revealed (last row of Table A4). Without exogenous variation in attendance to the meeting, we estimate equation 1 on the full sample of farmers, regardless of whether or not they attended. The estimated β parameter therefore captures our "intent" to reveal or not aggregate intentions; i.e., an intent-to-treat effect.

Although we do not find evidence of major imbalances across conditions, we augment equation 1 with a vector of individual characteristics collected pre-intervention, X_{ig}, to both account for existing imbalances and enhance the precision of our estimates through reduced unexplained variance in our outcome variable. In LFE-related estimates, this vector of covariates includes age, sex, land size, education (dummy for going to a French school versus a Koranic or no schooling), and measures for risk, time (patience), and social preferences (generosity/altruism).[13] In RCT-related estimates, this includes age, sex, a dummy for whether the individual holds a leadership position in the group, the groundnut harvest (in kg), a dummy for whether the farmer produced other crops, land size, measures for risk, time, and social preferences, and participation in the LFEs. This leads to the following estimating equation:

$$A_{ig} = \alpha + \beta I_{ig} + X_{ig}^{'}\rho + \varepsilon_{ig} \tag{2}$$

Finally, in line with our framework, we test for heterogenous treatment effects with respect to group size by estimating equation 3, where S_g is a dummy for large groups ($N = 20$) in the LFE estimations and actual group size in the RCT estimations; β is the average effect of being exposed to aggregate intentions in small(er) groups; and δ captures the additional effect of this exposure for individuals in large(r) groups.

$$A_{ig} = \alpha + \beta I_g + \gamma S_g + \delta I_g \times S_g + X_{ig}^{'}\rho + \varepsilon_{ig} \tag{3}$$

For our main specifications, we estimate the effect of revealing aggregate intentions on both the extensive margin – with the dependent variable A_{ig} being a dummy for whether the individual contributed any amount to the group – and the intensive margin – where the dependent variable A_{ig} is the total amount that one contributed to the group. Both are consistent with the framework (and notation) discussed in Section 2.

In the RCT-related estimates, we also run several robustness checks. First, we test for significance of β and δ when the dependent variable is members' intentions. Because intentions were collected prior to any information revelation, we expect these parameters to be equal to 0. Second, one may be concerned about social desirability bias in individuals' reporting of collective groundnut sales. For both the intensive and extensive margin estimates, we thus also use effective individual sales from all group members, based on the group's own administrative data (recall the

[13]Generosity/social preference was elicited through a hypothetical dictator game (with a greater number indicating a more generous/altruistic individual). Risk preferences were elicited through a hypothetical Binswanger (1980)-style lottery (with a lower number indicating a more risk averse individual. Finally, patience was elicited through typical, hypothetical preference-over-time questions (with a one-day front-end delay and a higher number indicating a less patient individual).

booklets/cooperative records discussed in Section 3.2); although such data do not include individual characteristics. Third, we also run some Tobit (to account for null reports) and inverse hyperbolic sine transformations specifications. Finally, we correct for multiple hypothesis testing (MHT) using the Holm-Bonferroni method (see Tables 2 and 3).

4. Results

4.1. Cheap talk and coordination

Tables 1 and 2 present our main results based on specifications 2 and 3. The first two columns are for the LFEs while the remaining ones are for the RCT. Our falsification test is presented in columns 3 and 4 where the dependent variable is one's intentions to contribute to the group, collected ahead of the intervention. In columns 5 and 6, we present the main results using data from individual surveys, while the last two columns rely on administrative data collected from the groups' books/records (there are no controls in this case).

In Table 1 we assess the extensive margin – i.e., the effect of revealing others' intentions on an individual's decision to contribute their resources (i.e., number of chips in the LFEs and kilograms/kgs of groundnuts in the RCT) to the group. We find no direct effect of revealing intentions, be it in the LFEs or in the RCT (columns 1 and 5); however, interacting with group size seems to yield support for Hypothesis 3. The differential effect across smaller and larger groups is significant. A similar finding holds in column 6, where the outcome is actual production sold through the cooperative. In comparison, no such results are found in the falsification test (column 4). Further, we can fairly confidently rule out concerns of social desirability bias in the responses, since the results are similar when using administrative data (column 8).

Results on the intensive margin are presented in Table 2 and show a similar pattern.[14] We find a direct effect of cheap talk on the quantity of chips (column 1) and groundnuts (column 5) contributed to the group. The results suggest that farmers in cheap talk groups contribute an additional 121 kg of their harvest towards collective commercialization (column 5) compared to 124 kg in the control group, based on the administrative data (column 7). In addition, the effect increases with group size, both in the LFEs (column 2) and the RCT (column 6). While no such relationships are found in our placebo tests (columns 3 and 4), the administrative data (columns 7 and 8) yield results comparable to those based on the individual survey data.[15] We also correct for MHT (see adjusted p-values in square [..] brackets). The effects in the LFEs (columns 1 and 2) are no longer significant; however, the effects in the RCT persist (columns 5-8). The only exception is the direct effect of cheap talk when using administrative data, but recall that there are fewer (no) control variables in this specification, which also explains the lower R^2. Following Maniadis et al. (2014), we also calculate the post-study probability (PSP) associated with the main cheap-talk effect, i.e., the first and fifth columns of Table 2 for the LFEs and RCT respectively. Tables A6 and A7 illustrate that for low initial priors (i.e., below 0.2) the accuracy of the cheap-talk effect can be questioned. However, for moderate to high priors (i.e., above 0.2) we can be relatively confident that the main cheap-talk effect is accurate.

[14] The RCT findings are robust to using a log transformation of group size.
[15] Results are robust to Tobit (Table A8) and inverse hyperbolic sine (Table A9).

Overall, Tables 1 and 2 provide support for our predictions (in particular hypothesis 3): Cheap talk (intentions) can enhance coordination in situations of strategic uncertainty and this effect varies with the size of the group.[16] To some extent, this relates to Feltovich and Grossman (2015) who explore the interaction between cheap talk and group size in a lab-based threshold public-goods game.

As previously discussed, we are unable to exogenously vary group size in the RCT. Therefore, the differential effect of group size (particularly in the RCT) may be associated with other characteristics and thus, we should be cautious in interpreting the interaction effect. We address this concern in two ways. First, we exploit the experimental design in order to separate the differential effect of group size from the potentially confounding threshold effect by using data from the LFEs. Table A10 compares the differential group-size effect across three main specifications. Columns 1-2 use data from all LFE sessions, regardless of the threshold level required to achieve coordination. These are columns 1-2 from Table 2. Columns 3-4 use data from LFE sessions where the average (i.e., per-player) number of chips required to achieve the threshold is constant across group size. Finally, columns 5-6 use data from LFE sessions where the overall threshold is constant across group size. As discussed in Section 3.1: If communication varies with group size despite the average number of chips needed to surpass the threshold or the overall threshold being fixed across group size, strategic uncertainty is likely to be the main mechanism at play. Indeed, column 6 provides evidence that the story is primarily about group size and its relation to strategic uncertainty. We also note that while the point estimate in column 4 is not statistically significant, it is positive and relatively large (in the order of 20 percent relative to the control group/CCG mean).

Second, Table A12 compares group characteristics across above- and below-median day-to-day group size. We find limited evidence that these groups systematically differ from one another across size, except for being part of different umbrella federations, which we control for in all specifications. In short, we are reasonably confident that the differential group size effect is mainly associated with strategic uncertainty and less so with the threshold required to achieve coordination, which can be thought of as the amount required to fulfill contracts, e.g., when selling to large buyers in the day-to-day environment.

4.2. What information matters?

In order to assess whether the content of the information had an impact, we interact the aggregate intended actions (A') revealed by study participants with the main treatment; i.e., whether or not this information was conveyed to the group (Table A13). Higher aggregate intentions lead to higher quantities sold to the group, but only in cooperatives where such information was revealed (column 2). First, this confirms that it is not just about "collecting intentions", but also "revealing them". Second, this finding supports the view that cheap talk helps farmers overcome coordination failure by shifting their beliefs.[17] One caveat to the analysis is that individual intentions and thus the aggregate, are endogenous, potentially reflecting other characteristics of the groups in question. The results are robust, however, to the inclusion of various individual and group characteristics (columns 3 and 4).

[16] In the upper panel of Table A11, we show that these results cannot be attributed to changes in individuals' risk, time (patience), and social preferences (generosity).

[17] We did not explicitly elicit individual-level beliefs as that would have significantly complicated the implementation of the RCT.

The RCT design also included variations on the type of information that was provided, which in turn allows us to explore which feature of aggregate intentions primarily enhances coordination (recall Table A3). Table 3 presents the disaggregated impact of providing information on groundnut contributions to the cooperative, by informational treatment arm. Similarly to Table 2, we correct for MHT. The adjusted p-values in square [..] brackets suggest that the main results discussed below are robust, except for the direct effects estimated using administrative data (column 5). As before, our falsification tests in columns 1 and 2 show no relationship between one's intentions and the later provision of information. Results in column 3 indicate that provision of information increased group contributions in all three Conditions (B–D), although with important differences in magnitude. In particular, while knowing others' aggregate intention leads to higher contributions (Condition B), the effect is more than two times larger in magnitude if one is also provided with the distribution of others' intentions (Condition C).[18]

Two potential mechanisms may be at play here. First, one may trust a given aggregate more when it originates from a larger number of individuals. For example, if farmers were to find out that most of the aggregate came from one individual's intended contribution, it would be reasonable to expect that this individual would be better off selling alone. So, revealing the distribution of intentions may help individuals refine their expectations. Second, individuals in the group may exhibit interdependent preferences, with group members' payoffs entering each others' utility functions, thus giving rise to social norms of equity and fairness (e.g., Manski, 2000; Sobel, 2005). While these effects may exist even in the absence of communication, they may be particularly salient when intentions and their distributions are revealed since they can be interpreted as signaling what other players consider "the right thing to do" (e.g., Bernheim, 1994). Examples of this type of norm and information signaling are Vesterlund (2003), Gächter et al. (2010), and Hill et al. (2012).

We find a smaller effect of revealing the distribution of intentions separately for leaders versus ordinary members (Condition D) in comparison to the overall distribution (Condition C). Leaders may be perceived as having superior information relative to ordinary members. In this case, that could mean that the leader for example has better knowledge of current market conditions.[19] So, it may be surprising that leaders' intentions lead to positive, but lower contributions to the group than the overall distribution (Condition C).

To further unpack this finding, we conduct an analysis comparable to that in Table A13, but now by informational treatment arm (see Table A14). First, we assess whether the average amount that leaders or group ordinary members intend to sell matters. Second, we check whether leaders' intentions being greater than those of ordinary members matters. I.e., one reason why the impact of Condition D may be smaller than that of Condition C is because ordinary members expect leaders to contribute significantly high amounts to the group and upon learning that leaders' intentions do not match up with this, they reduce their contributions. Table A14 suggests that the results are inconsistent with the above: Aggregate intentions, the overall distribution, and the distribution by leaders versus ordinary members all matter; however, the actual quantities revealed do not seem to matter. In other words, the findings do not support the

[18]One reason for why Condition B matters more than Condition A is that having one's intentions revealed (even confidentially) makes one more likely to stick to them.

[19]Bernard et al. (2014) find that 75 percent of members believed that leaders had better information about traders and/or prices in the region.

idea that members in Condition D may have been deceived by the intentions provided/revealed by leaders.

4.3. Participation in games and day-to-day behavior

Our design further enables us to assess how participation in the LFE coordination games affects later behavior in the naturally-occurring RCT. After the LFEs, all participants were asked to provide feedback regarding the game they had played. While the game was neutrally framed and there was no reference to collective marketing, many participants indicated that the setting they had been faced with closely aligned with the situation they faced when deciding whether or not to sell groundnuts through the cooperative. Considering the above as well as a related literature on transfer in coordination games (e.g., Cooper and Van Huyck, 2016), we hypothesize that experiencing coordination games with members of one's group may affect one's expectations vis-à-vis group members in subsequent, naturally-occurring, collaborative contexts.

We test for this in Table 4 where, in addition to the RCT treatment, we assess the impact of one's participation in the LFEs. Recall that players were randomly selected from the full list of members for their group, but that each session only gathered players who were members of the same cooperative.[20] We find no effect of LFE participation in columns 1 and 2, where the dependent variable is one's intentions to commercialize collectively. However, in columns 3 and 4 we find a positive effect of participating in the LFEs, on top of the treatment effects that were previously identified. Importantly, there is no effect at the group level (i.e., whether the cooperative was selected for the LFEs), but there is a clear effect for whether the individual was (randomly) selected for participation in the LFEs. In fact, the results hold even when restricting our sample to only those cooperatives that were selected for the LFEs. Furthermore, this effect is independent from the RCT: In column 7, we restrict our sample to those cooperatives selected for the LFEs, but that belonged to the control group in the RCT (where no intentions were revealed). The effects are large in magnitude, with participation in the LFEs leading to an additional 74 kgs of groundnuts being contributed to the cooperative during the (naturally-occurring) commercialization season.

In summary, while the LFEs are a useful diagnostic tool to test our hypotheses in a highly controlled environment, they seem to improve farmers' understanding of the strategic complementarities that exist, which in turn impacts collective commercialization. This finding is consistent with Zwane et al. (2011), who find that being surveyed can impact subsequent behavior, and prior experimental work such as Cardenas and Carpenter (2005), Turiansky (2017). It is also consistent with an older literature on the learning that can arise from participating in classroom experiments, e.g., a series of articles by Charles Holt and co-authors at *Journal of Economic Perspectives* in the late 1990s to early 2000s. Our findings contribute to the literature on generalizability of (lab) experiments (e.g., Levitt and List, 2007; Al-Ubaydli and List, 2013; Camerer, 2015; Viceisza, 2016) and a growing set of studies that combine findings from a spectrum of field experiments (e.g., Levitt and List, 2007; Barr et al., 2010; Finan and Schechter, 2012; Stoop et al., 2012; Voors et al., 2012; Camerer, 2015; Hoel et al., 2017).

[20] This was mainly driven by the fact that cooperatives are typically defined at the village level. With villages sometimes being far apart, we chose to (1) organize each session in the village school and (2) avoid further complications such as communicating decisions by phone (e.g., Hill et al., 2012). In Table A15 we report tests for equality of characteristics between LFE participants and non-participants within groups that were selected for the LFEs.

5. Conclusion

Most developing-country farms are small and family-run and many of them engage in cooperative agriculture. Thus, prior literature has argued that mechanisms aimed at facilitating smallholder coordination such as cooperatives are central to stimulating market participation. At the same time, cooperatives have not always been able to engage in collective action. In this paper, we primarily rely on a two-pronged empirical approach to test the effect of a cheap-talk institution on coordination among members of groundnut-producing cooperatives in Senegal. We start by conducting neutrally framed coordination games (LFEs) with randomly selected members of these cooperatives. We then conduct a natural field experiment (RCT) among the full set of cooperative members. In both experiments, we ask farmers how much they intend to contribute to the group prior to them actually doing so and then, confidentially reveal aggregate intentions to other cooperative members.

Combining data from these field experiments with survey and administrative data, we make two key contributions:

1. Revealing farmers' intentions to sell through the cooperative (cheap talk) improves collective commercialization (coordination) and this effect increases with group size. By exploiting the two-tier nature of our design, we are able to say with reasonable confidence that this effect is primarily the result of reduced strategic uncertainty. We also find that the content of cheap talk matters. For example, the distribution of intentions seems to lead to higher coordination than the mere sum of intentions, possibly in line with the literatures on conformity and interdependent preferences. Perhaps more surprisingly, knowing the overall distribution and its breakdown by leaders versus ordinary members yields somewhat lower impacts on coordination than knowing just the overall distribution.
2. Participation in the LFEs impacts subsequent behavior in the RCT. Specifically, those who were randomly selected to participate in the games were more likely to engage in collective commercialization, even though the games were neutrally framed. We hypothesize that this learning/transfer could be due to the following channels: (1) a training effect – i.e., participants better understand issues of strategic complementarities – and (2) an expectation effect – i.e., participants fine-tune their beliefs about group members' behavior in day-to-day situations of strategic uncertainty such as collective commercialization. In short, researchers should take heed when combining multiple types of field experiments.

Our findings suggest that a relatively simple and non-costly information intervention can improve coordination and market participation within cooperatives, thus begging the question as to why such an institution did not emerge in isolation. In the words of Matsuyama (1997): *"If the coordination problem were simple enough that even the outsider [] would know how to solve it, it would have been taken care of a long time ago by those directly involved with the problem."* One reason is that communication needs to be N-way in order to impact farmers' beliefs. In other words, it must aggregate and disseminate information across *all* cooperative members who produce the crop in question. This is quite different from free-form, day-to-day communication which tends to be bilateral, mainly among cooperative members who routinely interact. Another reason is that intentions were collected by a third party in an environment that is characterized by important interpersonal ties. So, participants might have been more willing to reveal and believe intended actions than if the process had been implemented by peers with potentially hidden agendas.

While it is likely that broader market forces also drive commercialization (e.g., Ashraf et al., 2009), this intervention would not have had significant impacts if it had missed the mark. So, our findings leave room for future work to explore (1) the nuances of implementing information/communication institutions (e.g., Haaland et al., 2020) and (2) how such institutions interact with other interventions aimed at boosting market participation, contract farming, and entrepreneurship (e.g., Fafchamps and Minten, 2012; Saenger et al., 2014; Dimitriadis and Koning, 2019; Hussam et al., 2020; Rao and Shenoy, 2021). One potential extension to our work would be an app that aggregates and disseminates the type of information we collected across cooperative members. Future work can explore that and related interventions.

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Tables

Table 1: Extensive margin: treatment effect on the probability of collective action

Dependent variable Source	Contributed to group LFE		Intended to contribute to group RCT - survey data		Contributed to group RCT - survey data		Contributed to group RCT - admin. data	
	1	2	3	4	5	6	7	8
Treatment	0.03	-0.04	0.06	0.05	0.07	-0.08	0.15	-0.21
	(0.03)	(0.04)	(0.03)	(0.06)	(0.05)	(0.09)	(0.10)	(0.21)
Size of Group	0.03	-0.02	0.00	0.00	0.01	0.00	-0.00	-0.01
	(0.03)	(0.04)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Treatment X Size		0.11		0.00		0.01		0.01
		(0.05)		(0.00)		(0.00)		(0.01)
Control group mean	0.89	0.89	0.80	0.80	0.13	0.13	0.27	0.27
R^2	0.05	0.06	0.10	0.10	0.16	0.16	0.08	0.09
N	3,316	3,316	873	873	873	873	2,752	2,752

Notes: Standard errors are clustered at the group level. Columns 1 and 2 use data from the LFEs. The dependent variable is the number of chips. The size in those specifications is a dummy variable that is 1 if the experimental session was run with 20 farmers and 0 if it was run with 10 farmers. Columns 3 and 4 use farmers' intended sales (of groundnuts) to the group, which were collected prior to the RCT interventions. Columns 5 and 6 use the self-reported (survey) collective commercialization. Columns 7 and 8 use administrative data on collective commercialization obtained from the cooperatives (recall booklets). In columns 3–8, size is the actual group size ranging from 4-91 (with median size being 24). Controls in the LFE regressions include age, sex, land size, education (dummy for going to a French school versus a Koranic or no schooling), generosity, risk aversion measures and time preferences elicited through hypothetical questions (see Table A4), and dummies for the federation and whether the farmer is a leader. Controls in the RCT regressions with survey data include the same variables (except for education) and a dummy for whether the farmer produced crops other than groundnuts, all measured pre-intervention.

Table 2: Intensive margin: treatment effect on quantities contributed to the group (correcting for MHT)

Dependent variable Source	# Chips contributed LFE		Contribution intentions (kg) RCT - survey data		Quantity contributed (kg) RCT - survey data		Quantity contributed (kg) RCT - admin. data	
	1	2	3	4	5	6	7	8
Treatment	0.40	-0.03	239.66	307.57	121.07	-78.93	129.47	-153.42
	(0.19)	(0.31)	(159.16)	(426.91)	(35.51)	(63.31)	(70.70)	(148.10)
	[.129]				[.006]		[.141]	
Size of Group	0.03	-0.31	-5.90	-3.72	7.18	0.75	-1.15	-9.30
	(0.24)	(0.27)	(5.57)	(10.15)	(1.85)	(1.28)	(2.18)	(3.53)
Treatment X Size		0.65		-2.44		7.19		8.83
		(0.38)		(11.62)		(2.23)		(3.57)
		[.142]				[.009]		[.062]
Control group mean	3.03	3.03	830.39	830.39	39.58	39.58	123.79	123.79
R^2	0.09	0.10	0.43	0.43	0.17	0.18	0.08	0.09
N	3,316	3,316	873	873	873	873	2,752	2,752

Notes: Standard errors are clustered at the group level. Holm-Bonferroni adjusted p-values are in square brackets. The correction for MHT is done for the direct impact of the treatment and the interaction with group size, across all specifications. Columns 1 and 2 use data from the LFEs. The dependent variable is the number of chips. The size in those specifications is a dummy variable that is 1 if the experimental session was run with 20 farmers and 0 if it was run with 10 farmers. Columns 3 and 4 use farmers' intended sales (of groundnuts) to the group, which were collected prior to the RCT interventions. Columns 5 and 6 use the self-reported (survey) collective commercialization. Columns 7 and 8 use administrative data on collective commercialization obtained from the cooperatives (recall booklets). In columns 3–8, the dependent variable is the quantity of groundnuts sold through the group and the size is the actual group size ranging from 4-91 (with median size being 24). Controls in the LFE regressions include age, sex, land size, education (dummy for going to a French school versus a Koranic or no schooling), generosity, risk aversion measures and time preferences elicited through hypothetical questions (see Table A4), and dummies for the federation and whether the farmer is a leader. Controls in the RCT regressions with survey data include the same variables (except for education) and a dummy for whether the farmer produced crops other than groundnuts, all measured pre-intervention. A dummy for whether the individual indicated positive intentions to sell through the group is also added.

Table 3: Effect on quantity sold by treatment condition (correcting for MHT)

Dependent variable Source	Contribution intentions (kg) RCT - survey data		Quantity contributed (kg) RCT - survey data		Quantity contributed (kg) RCT - admin. data	
	1	2	3	4	5	6
B	224.66	384.10	76.54	-154.33	94.56	-349.51
	(158.64)	(391.49)	(38.01)	(92.63)	(83.92)	(178.26)
C	186.43	391.81	169.47	-141.87	117.05	-124.39
	(207.98)	(530.65)	(57.39)	(61.45)	(88.38)	(175.38)
D	207.89	72.79	121.45	46.96	177.40	-4.59
	(168.65)	(422.89)	(47.96)	(71.45)	(84.32)	(156.30)
Joint significance (p)			[.029]		[.226]	
Size of Group	-6.20	-4.60	7.39	1.26	-1.17	-8.74
	(5.46)	(9.78)	(1.81)	(1.29)	(2.19)	(3.43)
B X Size		-5.41		8.14		12.89
		(10.52)		(3.48)		(4.18)
C X Size		-7.69		11.40		7.70
		(14.01)		(2.28)		(3.60)
D X Size		4.30		3.01		6.28
		(12.43)		(2.06)		(3.73)
Joint significance (p)				[.000]		[.019]
Control group mean	830.39	830.39	39.58	39.58	123.79	123.79
R^2	0.44	0.44	0.18	0.21	0.09	0.10
N	873	873	873	873	2,752	2,752

Notes: Standard errors are clustered at the group level. Holm-Bonferroni adjusted p-values are in square brackets. The correction for MHT is done for the direct impact of the treatment and the interaction with group size, across all specifications. The first two columns test the experimental integrity using intentions of collective sales. The other columns use as the dependent variable the total sales, from survey (3 and 4) and administrative data (5 and 6) respectively. Size is the actual group size ranging from 4-91 (with median size being 24). Controls in the regression with survey data include age, sex, land size, generosity, risk aversion, and time preferences (see Table A4), and dummies for the federation, whether the farmer is a leader, and whether the farmer produced crops other than groundnuts, all measured pre-intervention. A dummy for whether the farmer indicated positive intentions to sell through the group is also included for columns 3 to 6.

Table 4: Participation in LFEs and behavior in RCT

Dependent variable	Contribution intentions (kg)		Quantity contributed (kg)		Quantity contributed (kg)		
Sample	All Cooperatives				Game Cooperatives		Control Cooperatives
	1	2	3	4	5	6	7
Farmer was in LFEs	-136.32	-181.08	68.60	95.83	73.27	92.20	74.17
	(367.59)	(174.88)	(63.71)	(43.26)	(66.81)	(43.83)	(33.96)
Group was in LFEs	15.19	151.03	110.59	49.44			-3.93
	(412.55)	(162.18)	(68.91)	(53.55)			(17.96)
Treatment	151.17	204.15	130.40	122.53	204.01	140.04	
	(264.67)	(152.32)	(38.22)	(36.01)	(87.41)	(66.63)	
LFEs non participants mean	996.49	996.49	88.40	88.40	181.46	181.46	10.75
R^2	0.01	0.44	0.05	0.18	0.04	0.25	0.23
N	873	873	882	873	279	279	194
Controls	No	Yes	No	Yes	No	Yes	Yes

Notes: Standard errors are clustered at the group level. In the first two columns, intentions on collective commercialization (in kg) are used as the dependent variable. For all other regressions, the dependent variable is the actual quantity sold through the cooperative. All regressions include as controls age, sex, land size, generosity, risk aversion, and time preferences (see Table A4), cooperative size, and dummies for the federation, whether the farmer is a leader, and whether the farmer produced crops other than groundnuts, all measured pre-intervention.

Appendix

Figure A1: LFEs – visual aid

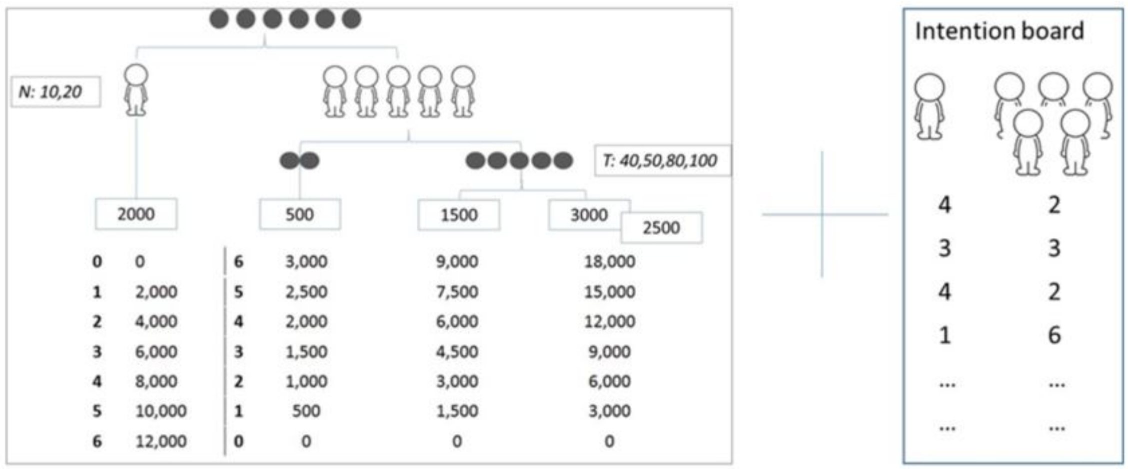

Table A1: Sample distribution across RCT and LFEs

	# of Cooperatives		# of farmers in survey		# farmers in admin data	
Was in LFEs?	No	Yes	No	Yes	No	Yes
RCT Condition A	10	7	114	80	320	207
RCT Condition B	15	6	173	68	640	201
RCT Condition C	14	6	160	59	360	276
RCT Condition D	14	7	156	72	418	331
Total	53	26	603	279	1738	1015

Condition A is the control group in the RCT.
Conditions B, C, and D are treatment variations (see Section 3.2 or Table A3).

Table A2: Distribution of Baseline and Communication Coordination Games

Variable	BCG	CCG	Total
# Sessions	28	28	56
# Rounds	112	110	222
# Players	429	410	839
# Observations	1716	1600	3316

Table A3: RCT treatments

RCT Condition	What is revealed?		
A	–	–	–
B	Aggregate intentions	–	–
C	Aggregate intentions	Distribution	–
D	Aggregate intentions	Distribution	Leader vs. Member

In all conditions, there was a training, intentions were collected, and a survey was conducted.

Table A4: Balance tests with respect to the binary treatment - farmer level

	N	All	No communication	Communication	P-value of diff.
Age	882	46.27	45.81	46.40	0.58
Sex (1=male; 0=female)	873	0.67	0.70	0.67	0.43
Leader (1=yes, 0=no)	873	0.18	0.22	0.18	0.19
Size of land (ha)	873	4.25	3.89	4.36	0.69
Risk (1 to 5)	873	2.80	2.81	2.80	0.96
Generosity (1 to 7)	873	2.92	3.19	2.84	0.00
Patience (1 to 5)	873	2.52	2.50	2.52	0.87
Federation (1=CCPA, 0=FEGPAB)	873	0.52	0.47	0.54	0.13
PO exposed to lablike exp.: 1=yes; 0=no	882	0.32	0.41	0.29	0.00
2013 harvest (kg)	873	1,680.85	1,868.76	1,627.16	0.32
Expected 2014 harvest (kg)	873	1,628.10	1,593.37	1,638.02	0.86
Intended to coll. com. : 1=yes, 0=no	873	0.84	0.80	0.85	0.10
Intentions coll. com. (kg)	873	966.43	830.39	1,005.30	0.25
Intentions indiv. com. (kg)	873	148.05	200.95	132.93	0.34
Intentions consumption (kg)	873	141.75	207.62	122.93	0.09
Intentions seeds stock (kg)	873	303.12	265.67	313.81	0.49
Farmed other crops : 1=yes, 0=no	873	0.57	0.59	0.57	0.55
Attended int. revelation meeting: 1=yes; 0=no	882	0.59	0.56	0.59	0.44

Notes: The p-values in the last column are reported based on two-tailed t-tests and the standard errors are clustered at the group level. All variables were measured before the intervention, that is before revealing intentions. Generosity/social preference was elicited through a hypothetical dictator game (with a greater number indicating a more generous/altruistic individual); risk through a hypothetical Binswanger (1980)-style lottery (with a lower number indicating a more risk averse individual); and patience through typical, hypothetical preference-over-time questions (with a one-day front-end delay and a higher number indicating a less patient individual).

Table A5: Balance tests with respect to the treatment conditions - farmer level

	N	All	A	B	C	D	P-value of diff.
Age	882	46.27	45.81	46.90	47.98	44.53	0.04
Sex (1=male; 0=female)	873	0.67	0.70	0.64	0.71	0.65	0.35
Leader (1=yes, 0=no)	873	0.18	0.22	0.18	0.17	0.17	0.60
Size of land (ha)	873	4.25	3.89	5.53	3.67	3.76	0.45
Risk (1 to 5)	873	2.80	2.81	2.72	2.85	2.85	0.79
Generosity (1 to 7)	873	2.92	3.19	2.88	2.64	3.00	0.00
Patience (1 to 5)	873	2.52	2.50	2.54	2.70	2.32	0.17
Federation (1=CCPA, 0=FEGPAB)	873	0.52	0.47	0.53	0.54	0.53	0.50
Group exposed to lablike exp.: 1=yes; 0=no	882	0.32	0.41	0.28	0.27	0.33	0.01
2013 harvest (kg)	873	1,680.85	1,868.76	1,395.65	1,855.39	1,653.70	0.29
Expected 2014 harvest (kg)	873	1,628.10	1,593.37	1,748.09	1,670.35	1,484.56	0.82
Intended to coll. com. : 1=yes, 0=no	873	0.84	0.80	0.87	0.84	0.85	0.34
Intentions coll. com. (kg)	873	966.43	830.39	966.20	1,068.47	985.15	0.64
Intentions indiv. com. (kg)	873	148.05	200.95	205.02	123.58	62.95	0.26
Farmed other crops : 1=yes, 0=no	873	0.57	0.59	0.61	0.57	0.53	0.29
Attended int. revelation meeting: 1=yes; 0=no	882	0.59	0.56	0.56	0.60	0.61	0.70

Notes: The p-values in last column are obtained by running a one-way ANOVA test using STATA, with standard errors clustered at the group level.

Table A6: PSP estimates: LFE cheap-talk effect in Table 2 - column 1 ($\beta = 0.66$)

	$k = 1$	$k = 2$	$k = 5$	$k = 10$	$k = 15$
π					
0.01	0.118	0.084	0.043	0.025	0.018
0.02	0.212	0.156	0.082	0.048	0.037
0.05	0.410	0.323	0.188	0.116	0.089
0.1	**0.595**	**0.502**	0.328	0.217	0.172
0.2	**0.767**	**0.694**	**0.524**	0.384	0.318
0.3	**0.850**	**0.795**	**0.653**	**0.516**	0.444
0.35	**0.877**	**0.830**	**0.703**	**0.573**	**0.501**
0.55	**0.942**	**0.917**	**0.843**	**0.753**	**0.695**

Notes: $PSP = \frac{(1-\beta^k)\pi}{(1-\beta^k)\pi+[1-(1-\alpha)^k](1-\pi)}$, where typical power $= 1 - \beta$, $\alpha = 0.05$, π = a prior, k represents the number of independent researchers working on specific associations in a field (i.e., competition).

Table A7: PSP estimates: RCT cheap-talk effect in Table 2 - column 5 ($\beta = 0.99$)

	$k = 1$	$k = 2$	$k = 5$	$k = 10$	$k = 15$
π					
0.01	0.167	0.094	0.043	0.025	0.018
0.02	0.288	0.173	0.083	0.048	0.037
0.05	**0.510**	0.351	0.189	0.116	0.089
0.1	**0.688**	**0.533**	0.329	0.217	0.172
0.2	**0.832**	**0.719**	**0.525**	0.384	0.318
0.3	**0.895**	**0.815**	**0.655**	**0.516**	0.444
0.35	**0.914**	**0.847**	**0.704**	**0.573**	**0.501**
0.55	**0.960**	**0.926**	**0.844**	**0.753**	**0.695**

Notes: See Table A6 for definition of PSP.

Table A8: Effect on collective commercialization - Tobit model

Dependent variable Source	Contributed to group RCT - survey data			
	1	2	3	4
Treatment	278.60	-622.40	460.23	-88.53
	(295.31)	(550.79)	(258.33)	(512.10)
Size of Group	27.50	-2.87	25.79	7.97
	(6.20)	(15.59)	(6.50)	(15.38)
Treatment X Size		31.70		18.94
		(16.94)		(16.57)
Control group mean	39.58			
Pseudo R^2	0.03	0.03	0.04	0.04
N	882	873	873	873
Control	No	No	Yes	Yes

Notes: The table shows the estimations of the main equation of interest (collective commercialization on treatment) using a Tobit model for the null quantities reported. The dependent variable is the quantity of groundnuts sold through the group. Controls include age, sex, land size, generosity, risk aversion measures and time preferences elicited through hypothetical questions (see Table A4), and dummies for the federation, whether the farmer is a leader, and whether the farmer produced crops other than groundnuts, all measured pre-intervention. A dummy for whether the individual indicated positive intentions to sell through the group is also added.

Table A9: Effect on collective commercialization - inverse hyperbolic sine transformation

Dependent variable Source	Contributed to group RCT - survey data			
	1	2	3	4
Treatment	0.42	-1.13	0.64	-0.52
	(0.39)	(0.68)	(0.34)	(0.63)
Size of Group	0.05	-0.00	0.05	0.01
	(0.01)	(0.02)	(0.01)	(0.02)
Treatment X Size		0.06		0.04
		(0.02)		(0.02)
Control group mean	39.58			
R^2	0.10	0.11	0.17	0.17
N	882	873	873	873
Control	No	No	Yes	Yes

Notes: Standard errors are clustered at the group level. The dependent variable is the quantity of groundnuts sold through the group, transformed by the inverse hyperbolic sine. Controls include age, sex, land size, generosity, risk aversion measures and time preferences elicited through hypothetical questions (see Table A4), and dummies for the federation, whether the farmer is a leader, and whether the farmer produced crops other than groundnuts, all measured pre-intervention. A dummy for whether the individual indicated positive intentions to sell through the group is also added.

Table A10: Isolating the effect of group size from that of the threshold (LFEs)

Dependent variable Source (recall Section 3.1)	# Chips contributed LFE					
	1	2	3	4	5	6
Treatment	0.40	-0.03	0.22	-0.03	0.36	-0.02
	(0.19)	(0.31)	(0.21)	(0.30)	(0.24)	(0.29)
Size of Group	0.03	-0.31	-0.27	-0.53	-0.00	-0.38
	(0.24)	(0.27)	(0.21)	(0.35)	(0.23)	(0.25)
Treatment X Size		0.65		0.53		0.77
		(0.38)		(0.41)		(0.44)
Control group mean	3.03		3.09		3.09	
R^2	0.09	0.10	0.08	0.09	0.11	0.12
N	3,316	3,316	2,116	2,116	2,320	2,320
Fixed threshold?	No		Yes - Average		Yes - Overall	

Notes: Standard errors are clustered at the group level. All columns use data from the LFEs. The dependent variable is the number of chips. The size in all specifications is a dummy variable that is 1 if the experimental session was run with 20 farmers and 0 if it was run with 10 farmers. Columns 1 and 2 use data from all LFEs regardless of the threshold required for coordination. Columns 3 and 4 only use data from the LFEs where the average (per-player) number of chips required to reach the threshold was the same across large and small groups. Columns 5 and 6 only use data from the LFEs where the overall threshold was fixed across small and large groups. Controls in the regressions include age, sex, land size, education (dummy for going to a French school versus a Koranic or no schooling), generosity, risk aversion measures and time preferences elicited through hypothetical questions (see Table A4), and dummies for the federation and whether the farmer is a leader.

Table A11: Robustness check: effect of the treatment on patience, generosity and risk

Dependent variable Source	Patience		Generosity RCT - survey		Risk	
	1	2	3	4	5	6
Binary treatment						
Treatment	-0.15	-0.14	0.08	0.09	0.17	0.40
	(0.11)	(0.28)	(0.10)	(0.20)	(0.11)	(0.33)
Size of Group	-0.00	-0.00	0.00	0.00	-0.00	0.00
	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)
Treatment X Size		-0.00		-0.00		-0.01
		(0.01)		(0.01)		(0.01)
R^2	0.07	0.07	0.08	0.08	0.04	0.04
N	873	873	873	873	873	873
Treatment arms						
B	-0.12	-0.04	0.11	0.16	0.18	0.44
	(0.13)	(0.32)	(0.12)	(0.26)	(0.13)	(0.35)
C	-0.29	-0.22	0.04	-0.03	0.08	0.30
	(0.14)	(0.32)	(0.12)	(0.25)	(0.16)	(0.37)
D	-0.04	-0.15	0.08	0.16	0.23	0.48
	(0.14)	(0.32)	(0.13)	(0.24)	(0.14)	(0.38)
Size of Group	-0.00	-0.00	0.00	0.00	-0.01	0.00
	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)
B X Size		-0.00		-0.00		-0.01
		(0.01)		(0.01)		(0.01)
C X Size		-0.00		0.00		-0.01
		(0.01)		(0.01)		(0.01)
D X Size		0.00		-0.00		-0.01
		(0.01)		(0.01)		(0.01)
Control group mean	1.84	1.84	3.10	3.10	2.15	2.15
R^2	0.07	0.07	0.08	0.08	0.04	0.04
N	873	873	873	873	873	873

Notes: Standard errors are clustered at the group level. See Table A4 for definitions of generosity/social preferences, risk preferences, and patience/time preferences. All dependent variables were measured after the intervention and in the same way they were measured before the intervention. Their values pre-intervention are used as controls in the regressions. Additional controls include age, sex, land size, and dummies for the federation, whether the farmer is a leader, and whether the farmer produced crops other than groundnuts, all measured pre-intervention.

Table A12: Comparing large and small cooperatives

	N	All	Small Cooperatives	Large Cooperatives	P-value of diff.
Age	882	46.27	46.59	45.99	0.60
Sex (1=male; 0=female)	873	0.67	0.71	0.64	0.37
Leader (1=yes, 0=no)	873	0.18	0.18	0.19	0.83
Size of land (ha)	873	4.25	4.13	4.36	0.81
Risk (1 to 5)	873	2.80	2.74	2.86	0.37
Generosity (1 to 7)	873	2.92	2.89	2.94	0.77
Patience (1 to 5)	873	2.52	2.56	2.48	0.61
Federation (1=CCPA, 0=FEGPAB)	873	0.52	0.33	0.69	0.00
Coop exposed to lablike exp.: 1=yes; 0=no	882	0.32	0.27	0.36	0.39
2013 harvest (kg)	873	1,680.85	1,780.21	1,592.46	0.54
Expected 2014 harvest (kg)	873	1,628.10	1,663.21	1,596.86	0.87
Intended to coll. com. : 1=yes, 0=no	873	0.84	0.81	0.87	0.08
Intentions coll. com. (kg)	873	966.43	972.34	961.17	0.96
Intentions indiv. com. (kg)	873	148.05	170.02	128.50	0.58
Farmed other crops : 1=yes, 0=no	873	0.57	0.59	0.56	0.55
Attended int. revelation meeting: 1=yes; 0=no	882	0.59	0.62	0.55	0.11

Notes: The p-values in the last column are reported from two-tailed t-tests and the standard errors are clustered at the group level. Cooperatives are divided at the median size (24 members) into large and small. All the variables were measured before the intervention, that is before revealing intentions. See Table A4 for definitions of generosity/social preferences, risk preferences, and patience/time preferences.

Table A13: Collective commercialization and aggregate intentions

Dependent variable Source	Contributed to group RCT - survey data			
	1	2	3	4
Treatment	91.90	-16.87	121.66	50.55
	(37.79)	(36.80)	(34.92)	(35.13)
Aggregate intentions (tonnes)	3.41	-0.49	0.58	-1.83
	(1.08)	(0.51)	(1.46)	(1.29)
Treatment X Aggregate intentions		4.59		2.96
		(1.42)		(1.44)
Control group mean	39.58			
R^2	0.08	0.10	0.17	0.18
N	873	873	873	873
Controls?	No	No	Yes	Yes

Notes: Standard errors are clustered at the group level. The dependent variable is the quantity of groundnuts sold through the group. Aggregate intentions (in tonnes) are obtained as the sum of the individual intentions reported by farmers. Controls include age, sex, land size, generosity, risk aversion measures and time preferences elicited through hypothetical questions (see Table A4), cooperative size, and dummies for the federation, whether the farmer is a leader, and whether the farmer produced crops other than groundnuts, all measured pre-intervention. A dummy for whether the individual indicated positive intentions to sell through the group is also added.

Table A14: Collective commercialization and leader's intentions

Dependent variable Source	Contributed to group RCT - survey data			
	1	2	3	4
B	73.39	108.56	57.59	88.06
	(48.97)	(43.60)	(44.72)	(40.47)
C	133.66	169.72	148.73	181.64
	(56.57)	(54.83)	(60.85)	(58.72)
D	125.63	144.68	70.84	117.12
	(63.79)	(59.99)	(43.04)	(45.77)
Average intentions leader	0.06	0.07		
	(0.04)	(0.04)		
Average intentions member	-0.04	-0.06		
	(0.03)	(0.03)		
D X Average intentions leader	0.06	0.03		
	(0.07)	(0.08)		
D X Average intentions member	-0.10	-0.06		
	(0.07)	(0.08)		
Leader's intentions -/- member's ≥ 0			46.66	49.67
			(49.42)	(45.38)
D X difference ≥ 0			43.34	6.62
			(72.45)	(71.19)
Size of Group	7.14	6.97	7.52	7.19
	(2.03)	(2.18)	(1.96)	(1.92)
Control group mean	39.58			
R^2	0.15	0.19	0.14	0.18
N	868	868	868	868
Control	No	Yes	No	Yes

Notes: Standard errors are clustered at the group level. The dependent variable is the quantity of groundnuts sold through the group. Average intentions are used as an explanatory variable. This information was only revealed in the RCT Group D. Controls include age, sex, land size, generosity, risk aversion measures and time preferences elicited through hypothetical questions (see Table A4), and dummies for the federation, whether the farmer is a leader, and whether the farmer produced crops other than groundnuts, all measured pre-intervention. A dummy for whether the individual indicated positive intentions to sell through the group is also added.

Table A15: Comparing farmers attending games to farmers not attending, within LFE coops

	N	All	Farmers in games No	Yes	P-value of diff.
Age	279	45.05	46.85	44.44	0.07
Sex (1=male; 0=female)	279	0.65	0.61	0.66	0.45
Leader (1=yes, 0=no)	279	0.19	0.10	0.23	0.00
Size of land (ha)	279	3.96	3.36	4.17	0.26
Risk (1 to 5)	279	2.96	2.79	3.02	0.19
Generosity (1 to 7)	279	3.16	3.01	3.21	0.30
Patience (1 to 5)	279	2.44	2.46	2.43	0.87
Federation (1=CCPA, 0=FEGPAB)	279	0.40	0.52	0.36	0.14
2013 harvest (kg)	279	1,807.42	1,537.59	1,899.52	0.40
Expected 2014 harvest (kg)	279	1,432.58	1,585.80	1,380.28	0.65
Intended to coll. com. : 1=yes, 0=no	279	0.86	0.86	0.87	0.91
Intentions coll. com. (kg)	279	916.64	1,052.35	870.32	0.61
Intentions indiv. com. (kg)	279	71.65	54.79	77.40	0.56
Farmed other crops : 1=yes, 0=no	279	0.66	0.68	0.66	0.80
Attended int. revelation meeting: 1=yes; 0=no	279	0.72	0.59	0.77	0.00

Notes: The p-values in the last column are reported from two-tailed t-tests, and the standard errors are clustered at group level. Only cooperatives that took part in the LFEs are considered here. All members did not take part, and those who took part were chosen randomly. The table compares those two categories of farmers, within cooperatives exposed to LFEs. All the variables were measured before the intervention, that is before revealing intentions. See Table A4 for definitions of generosity/social preferences, risk preferences, and patience/time preferences.