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

Fo Kodjo Dzinyefa Aflagah, University of Maryland

Tanguy Bernard, University of Bordeaux and IFPRI

Angelino Viceisza, Spelman College

Abstract

Coordination is central to social interactions. Theory and conventional lab experiments suggest that cheap talk can enhance coordination. Yet, field evidence remains limited. We test this effect in a common naturally-occurring setting; one where members of agricultural cooperatives seek to jointly sell their output to secure higher unit prices. Combining theory, artefactual field experiments (LFEs), natural field experiments (RCTs), surveys, and cooperative records, we find that: (1) Revealing farmers' intended sales yields enhanced coordination in larger groups; (2) such cheap talk leads to higher incomes for small-scale farmers; (3) participation in the LFEs affects subsequent behavior in the RCTs.

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1 Introduction

The success of many activities depends on the coordinated actions of a (sometimes large) number of actors whose activities complement one another. Without prior knowledge of others' intended actions, agents may steer away from such activities. In their simplest form, coordination games illustrate this idea by allowing for multiple pure Nash equilibria where, in absence of communication, agents choose safer but Pareto dominated strategies. Given their applicability to many areas of economics, coordination games and the related concepts of strategic uncertainty and coordination failure have played a key role in the literature.[1]

With coordination failure at the heart of certain development (poverty) traps (Wydick, 2007), a key policy question is how to mitigate it (see for example Hoff, 2000). This has led to a related literature on nonbinding communication (cheap talk), as a potential mechanism for increasing coordination. Most of this literature is theoretical and/or based on conventional laboratory experiments.[2] In this paper, we revisit whether, and if so how, cheap talk communication improves coordination in a field context where strategic uncertainty has historically led to coordination failure and that represents much of the developing and developed world.

We study farmer cooperatives that seek to sell agricultural production collectively in Senegal.[3] Farmer groups have featured prominently on the policy agenda of many countries. For example, Barrett (2008) reviews several studies on market access in developing countries and concludes that interventions aimed at facilitating smallholder organization such as cooperatives, are central to stimulating market participation. Despite such potential, there is quite a bit of evidence showing that cooperatives are not always able to sell collectively (see for example Fafchamps and Hill, 2005; Aldana et al., 2007; Hellin et al., 2007; Ragasa and Golan, 2014). Our primary goal is not to promote collective commercialization, as we are agnostic regarding its effective capacity to increase farmers' income.[4] Instead, we seek to

[1]Diamond (1982), Bryant (1983), Ostrom (2010), Van Huyck et al. (1990), Battalio et al. (2001), Battaglini and Bénabou (2003), Aguiar et al. (2015), and several other references (within) discuss contexts in which the returns to a player from undertaking a particular action are affected by the (un)coordinated actions of other players.

[2]Some examples include Farrell (1987), Ostrom (1990), Van Huyck et al. (1990), Cooper et al. (1992), Farrell and Rabin (1996), Crawford (1998), Battalio et al. (2001), Ben-Porath (2003), Charness and Dufwenberg (2006), Charness and Dufwenberg (2011), and Agranov and Yariv (2017). A fairly recent exception in the field is Elfenbein et al. (2018) who study charity cheap talk on eBay – a context that is, however, different from coordination (particularly ours which occurs in an agricultural setting with pre-existing social ties). Their findings suggest that most buyers (justifiably) avoid cheap talk listings when credible quality signals are available, thus limiting the extent of cheap talk.

[3]In the remainder of the paper, we use the terms cooperatives, groups, and farmer groups interchangeably; as we do farmers, members, and players.

[4]Collective commercialization is often promoted in Sub-Saharan Africa on the grounds of imperfect competition between traders in remote and thin markets, which in turn leads to rent extraction from farmers. By aggregating farmers' production, collective commercialization offers the possibility to (1) reach distant

understand why, *in groups designed for the purpose of collective commercialization*, farmers are unwilling to sell their production collectively. In particular, because those same farmers report believing that collective commercialization would lead to higher incomes, if the group were able to aggregate a large enough quantity to be sold.

We hypothesize that strategic uncertainty, i.e. the uncertainty that other members will sell their production via the cooperative, is key to explaining the lack of coordination (collective commercialization) in these groups.[5] While a given member may collect others' "intentions" to sell through the group prior to making a decision, it is unlikely that said person will obtain such information on a sufficiently large number of members. Thus, our intervention seeks to solve a group-based coordination dilemma that manifests itself as a failure to sell through the cooperative, by revealing information on the planned actions of all members prior to commercialization.[6] We use a simple theoretical framework to show that (1) beliefs about others' contributions are key to the decision to sell through the group; (2) larger groups can have more difficulty coordinating (i.e. commercializing collectively); and (3) knowing others' intentions to sell through the group (a form of cheap talk/communication) can increase the likelihood of coordination, particularly in larger groups.

In order to test these hypotheses, we conducted a range of field experiments and surveys with a sample of 79 groundnut-producing cooperatives in Senegal. We assess whether knowing others' aggregate intentions to sell through the group impacts actual sales to the group during commercialization. For a subsample of these cooperatives, we start with a direct test of our theoretical predictions by conducting neutrally framed, high-stakes, stag-hunt-like coordination games (i.e. artefactual field experiments in the terminology of Harrison and List, 2004, hereafter, lab-in-the-field experiments/LFEs), in which we exogenously vary two main parameters of interest – i.e. the group size and coordination threshold required for achieving the high-payoff/stag equilibrium. For each group, randomly selected members were invited to play the game. In a random subset of game sessions, we also collected and confidentially revealed players' intentions, in particular the group's aggregate intentions, before they made decisions.

but larger markets or (2) negotiate better with local traders. As reviewed by Dillon and Dambro (2017), however, the empirical evidence is mixed. For example, Bergquist (2017) finds a low pass-through of price premiums from traders to farmers in Kenya, while Casaburi and Reed (2013) find a high pass-through in Sierra Leone. Bernard et al. (2008) find positive but limited impacts of cooperatives in Ethiopia and Ashraf et al. (2009) find positive, but unsustained, impacts in Kenya.

[5]There may be other reasons why cooperatives may or may not be able to sell collectively. For example, Casaburi and Macchiavello (2015a,b) and Hill et al. (2014) discuss factors such as commitment sanctions and deferred payments respectively.

[6]Bernard et al. (2014) find that among a sample of 27 groundnut-producing cooperatives in Senegal, 67% of group members believe that, if presented with the opportunity, other members would by-pass sales through the group and sell individually to a trader for a potentially lower, but more certain payoff.

Six months later, we implemented an almost identical naturally-occurring intervention (i.e. natural field experiment in the terminology of Harrison and List, 2004, hereafter, randomized controlled trial/RCT) with our full sample. Contrary to the LFEs, where we created experimental groups, in the RCT we worked with pre-existing naturally-occurring (i.e. day-to-day) farmer cooperatives. As such, the group size and coordination threshold were given.[7] Shortly after the harvest period, we collected all group members' intentions regarding their planned sales through the group (and individually) for the upcoming season. In a random subset of the groups, the aggregate intentions were revealed in a public meeting shortly before the start of the commercialization season. The LFEs and RCT complement each other, with the former providing a "cleaner" test of the theory (albeit in an artefactual setting) and the latter forming a "real-life" test, but without the possibility to exogenously vary all parameters of interest. We further supplement this spectrum of field experiments with survey and group-administrative data.

Our findings are as follows. In line with the theoretical predictions, revealing aggregate intentions improves coordination in both the LFEs and the RCT, but only if the group size is large enough. These results do not seem to be mediated by changes in farmers' preferences, in particular over risk and time, suggesting that changes in farmers' expectations of peer behavior is the main mechanism that is at play. The results are robust to a series of placebo tests, which indicate that farmers' intentions are balanced at baseline. We are also able to rule out social desirability bias as a confounding factor by comparing farmers' responses in surveys with the group's administrative data.

To the best of our knowledge, this study is the first to provide a test of cheap talk as a means to overcome coordination failure in a real life setting (recall references cited previously). This setting is arguably optimal, since we work with a relatively large number of cooperatives in which strategic complementarities and uncertainty are key, i.e. members' benefits from investing in the group depend on others' (uncertain) decisions to do so. Our results are consistent with prior findings that cheap talk facilitates coordination. However, in our context, this result primarily plays out in larger groups, suggesting that communication is particularly meaningful in shaping beliefs when it is costlier to coordinate. This stands in contrast to Feltovich and Grossman (2015) who find that the beneficial effect of cheap talk decreases as the group size increases; although their findings are based on laboratory *public*

[7]In the day-to-day context, the membership of farmer groups is likely to be driven by location characteristics (e.g. population density) and group performance. Accordingly, group size is likely to capture other attributes that are difficult to manipulate in a naturally-occurring setting. Moreover, the coordination threshold is not readily observable and thus also difficult to vary exogenously. The threshold may be farmer-specific (in that farmers can be heterogenous with respect to their commercialization capacities) or group-specific (depending on location as well as leaders' commercialization capabilities).

goods games with group sizes smaller than ours.

We further find positive welfare effects of the intervention, albeit limited to farmers with lower production. From a policy perspective, these results point to the potential for coordination-failure-based poverty traps and one way to try to solve them. In other words, collective commercialization is both more relevant (e.g. due to fixed transaction costs in commercialization) and more difficult for smaller farmers (e.g. due to a larger pool needing to coordinate in order to achieve the high-level/stag payoff). So, the type of "light-touch", group-level communication intervention we implement can be a promising approach to reduce coordination failure in larger groups that may be comprised of a greater number of smaller farmers and thus, with greater uncertainty over others' (planned) actions.

Our study also contributes to the strand of literature that links results across the spectrum of (field) experiments (see for example Levitt and List, 2007; Camerer, 2015; Viceisza, 2016, and the references within).[8] Perhaps contrary to Voors et al. (2012), we find that behavior in the "lab" (i.e. LFEs) does transfer to behavior in the "field" (i.e. RCT). We exploit the random selection of individuals in the LFE stage of our study and find that LFE participants were more likely to engage in collective commercialization than non-selected participants in the same cooperatives. In other words, there is suggestive evidence that participants "learn from the LFEs" (a finding that has anecdotally been documented by for example Cardenas and Carpenter, 2005; Hill and Viceisza, 2012; Viceisza, 2016).[9] Like others, our study suggests using LFEs in combination with RCTs (and other approaches) to better understand the mechanisms that are at play; however, researchers should also take heed that participation in LFEs in itself may affect subjects' future behavior.

The remainder of the paper proceeds as follows. Section 2 places our study within the context of the Senegalese groundnut sector/market and provides further evidence of strategic uncertainty and coordination failure among related cooperatives. Section 3 discusses our theoretical framework. Section 4 details our empirical design and estimation strategy. Section 5 covers our main findings. Finally, Section 6 concludes.

[8]Some examples include Barr et al. (2010) who correlate results from dictator games with absenteeism of school teachers in Uganda; Finan and Schechter (2012) who relate behavior in trust games to individuals' responses to vote buying in Paraguay; Stoop et al. (2012) who study cooperation among fishermen in the Netherlands; and Hoel et al. (2017) who use public goods games to relate spousal cooperation to household-level productive inefficiencies in Senegal.

[9]This is not too different from the discussions by Charles Holt and co-authors in a series of papers on (the benefits of) classroom experiments in *Journal of Economic Perspectives* in the late 1990s.

2 Strategic uncertainty in Senegalese cooperatives

Groundnut production has long constituted the backbone of the Senegalese economy. At independence, the sector employed 87% of the agriculturally active population in rural areas and took up half of the cultivated land. Groundnut processing contributed to 42% of all industrial output and groundnuts represented 80% of all export revenues (see for example Caswell, 1984). Over time, however, revenues from the sector steadily declined through a combination of: (1) external factors such as lower international prices, the end of preferential tariffs to the French market (1972), droughts (1969-1973), oil shocks (1973, 1979), and exchange rate devaluation (1994); and (2) internal factors such as mismanagement and political considerations at various levels of the value chain. These resulted in several attempts to reform the sector, which in the late 1990s culminated in the gradual privatization of all segments of the value chain.

Prior to the reforms, a dense network of state-controlled cooperatives was the exclusive interface between farmers and other actors in the groundnut value chain. Through their groups, farmers accessed credit, inputs and extension services. Each year, the producer price of groundnut was fixed at the national level and all marketed production was collected through cooperatives. Following privatization reforms, the role, functioning and capacity of producer cooperatives significantly changed; in particular for activities related to collective commercialization. Although privatized in 2001, Suneor, the principal end-buyer of groundnuts still exists, but procures groundnuts through a system in which private traders are ensured a fixed price upon delivery at the processing plant. Traders can decide to purchase from cooperatives or from farmers individually. Competitive forces were expected to support producer prices, but issues of local monopsony and potential collusion, have led to general dissatisfaction with this system. In 2010, the export-monopoly previously granted to Suneor was abolished, facilitating the entry of new players who could also procure groundnuts from cooperatives or individual farmers.

In short, reforms led to important changes in farmers' ability to coordinate within cooperatives. Previously, there were no strategic complementarities between members of a group: the price per kilogram (kg) of groundnut was the same whether the cooperative aggregated small or large quantities. At the same time, there was no uncertainty vis-à-vis other members' decision to sell through the cooperative, given the absence of alternative commercialization outlets. In the new system, group members became strategic complements since the cooperative could obtain better per-unit prices by aggregating production and accessing larger, more distant markets/processing plants, or by negotiating more strongly with local traders. However, farmers could also sell (part of) their production directly to local traders. So, this presented a tradeoff: while aggregation through the cooperative could lead to a higher unit price, receipt of such funds would usually be delayed relative to local traders who pay cash on delivery. Moreover, the price premium obtained from collective commercialization would depend on the quantity of output sold by other cooperative members to the group. However, this is uncertain at the time a given farmer is visited by a trader.

While groundnut cooperatives remain active in input and credit provision, their capacity to aggregate and sell output has weakened considerably. Bernard et al. (2014) document commercialization among members of 27 groundnut cooperatives in Senegal whose main, stated objective is collective commercialization. Their data indicate that a great majority of members sells individually to local traders in spot market transactions.[10] In fact, only 11 of the 27 groups had been able to conduct a collective sale in 2011, and for those that did, only half of the members were found to have participated. This limited involvement contrasts with farmers' perceived potential for collective commercialization: 79% of farmers cited problems when attempting to commercialize production individually, including lack of transportation to reach more lucrative markets, insufficient knowledge of current prices, and limited production to negotiate better prices; at the same time, practically all respondents were convinced that group commercialization could alleviate such constraints.

Bernard et al. (2014) also collect members' aversion to strategic uncertainty framed in an individual or collective commercialization context.[11] The results suggest that aversion to strategic uncertainty is negatively correlated with individuals' effective sales through the cooperative. This correlation clearly dominates that of other preferences such as risk, time, and social (altruism). When asked about other group members' likely response to the above strategic uncertainty question, 67% of the sample believed that, if presented with the opportunity, those members would by-pass sales through the group and sell individually to a trader for a potentially lower payoff than they would.

Overall, reforms in the Senegalese groundnut sector paved the way for issues of strategic uncertainty in associated cooperatives. This has in turn limited their capacity to aggregate members' production and successfully engage in collective commercialization. Given imperfect competition and the potential for strategic complementarities, we thus assess whether a subtle, non-costly cheap-talk intervention can mitigate the coordination failure that results from strategic uncertainty in this context.

[10] Spot market transactions refer to the fact that less than 7% of farmers deal regularly with the same trader and pre-harvest contracting exists in less than 2% of cases.

[11] Responses were elicited by means of framed, stag-hunt-like coordination games along the lines of Heinemann et al. (2009). Accordingly, subjects were presented with the choice to sell to traders at a certain price or sell through the group at higher but uncertain price that depends on how many others also sell through the group. The price offered by the trader was incrementally raised until subjects switched from selling through the cooperative to selling to the trader.

3 Theoretical framework

Consistent with the above context, our theoretical framework is based on a critical mass coordination game. There are $N \in \mathbb{N}$ players, each of whom j has a positive endowment $V_j \in \mathbb{R}_+$. All players simultaneously choose an amount $A_j \in [0, V_j]$ to send to the group (the equivalent of commercializing collectively) and keep the remainder $V_j - A_j$ (the equivalent of selling individually). Any quantity A_j that is 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. Otherwise, that is if they send a quantity $A < T$, A_j earns a low return $L < H$, which is normalized to zero (i.e. $L = 0$). Whatever a player chooses to keep individually, that is $V_j - A_j$, earns a medium return M, where $0 < M < H$.[12]

Assuming $U(w)$ is a neoclassical utility function defined over own monetary payoffs w, with positive first-order derivative ($U' > 0$) and negative second-order derivative ($U'' < 0$), there are in principle two equilibria in pure strategies in such a game.[13] If A is expected to surpass T, all players should send all of their endowment to the group ($A_j^* = V$ for all j); otherwise, they should all keep their full endowment ($A_j^* = 0$ for all j). To think through mixed-strategy equilibria in this game, consider a specific player i who compares the expected utility of sending the full endowment to the group, $EU(A_i = V)$, with the utility of keeping the full endowment, $U(A_i = 0)$. This player is indifferent across the two pure-strategy equilibria under the following condition:

$$\begin{aligned} pU(V_iH) + (1-p)U(V_i \times 0) &= U(V_iM) \\ pU(V_iH) &= U(V_iM) \end{aligned} \tag{1}$$

where $p = P\,(A \geq T)$.

Recalling that $A = \Sigma_j^N A_j$ (i.e. the sum of N independently and identically distributed random variables), p can be characterized by the so-called convolution of the N individual distribution functions. In order to make this more explicit, suppose that A_j is normally

[12] As discussed in Sections 1 and 2, there are several reasons why $L < M$ and $H > M$ may hold. In particular, changes in government policies and market conditions in Senegal led to farmer cooperatives losing bargaining power relative to traders. Yet, if able to aggregate sufficient production, such cooperatives would offer better prices (H) than traders would at the farm gate (M). If unable to aggregate, individual farmers would have to resort back to selling individually (in absence of the cooperative), which due to waiting and high fixed-transaction costs would lead to lower effective prices (L).

[13] In standard expected utility, one can think of w as the decisionmaker's terminal wealth; the same could apply here. We assume that $U(.)$ is only defined over one's own payoffs and thus, abstract from social preferences. In the empirical analysis, we do check for internal validity with regard to a range of aspects including altruistic motives. Finally, we believe that assuming $U'' < 0$ is fairly innocuous given typical assumptions of diminishing marginal utility/wealth and empirical evidence in support of risk aversion for many samples. Moreover, the latter is another validity test we perform in the analysis.

distributed with mean μ_j and variance σ_j^2.[14] Then, equation 1 can be written as:

$$\underbrace{\int_T^V \frac{1}{\sqrt{2\pi\sigma^2}} e^{-\frac{(A-\mu)^2}{2\sigma^2}} dA}_{p=P(A\geq T)=\Phi(V)-\Phi(T)} - \frac{U(V_iM)}{U(V_iH)} = 0, \text{ where} \tag{2}$$

$\Phi(.)$ represents the cumulative distribution function (CDF) of a Normal density with mean $\mu = \Sigma_j^N \mu_j$ and variance $\sigma^2 = \Sigma_j^N \sigma_j^2$, and $V = \Sigma_j^N V_j$. So, equation 2 implicitly defines p as a function of A_i and A_{-i} (i.e. a given player's action and all other players' actions) as well as N (the number of players), H (the per-unit return on A_i if the collective threshold is surpassed), and M (the per-unit return on what is kept/sold individually, $V_i - A_i$).

The above setup enables us to show the following hypotheses that form the basis for the study design (Section 4) and empirics (Section 5).

Hypothesis 1. As the group size N increases, player i does not increase its contribution to the group A_i, unless others' collective contribution to the group A_{-i} is sufficiently high.

Proof. Since N is discrete, we cannot straightforwardly apply the implicit function theorem.[15] Therefore, we appeal to Topkis's monotonicity theorem (Topkis, 1978; Milgrom and Roberts, 1994), which says that if p is submodular (or supermodular) in $(A_i; N)$, then $\frac{\Delta A_i^*}{\Delta N} \leq 0$ (or $\frac{\Delta A_i^*}{\Delta N} \geq 0$). So, we need to establish either the sub or supermodularity of p for different parts of the domain. One way to check is as follows: p is submodular on a specific part of the domain if

$$\Phi(\hat{A}; N+1) - \Phi(A; N+1) \leq \Phi(\hat{A}; N) - \Phi(A; N) \quad \forall \quad \hat{A} > A. \tag{3}$$

Otherwise, the weak inequality would be reversed and p would be supermodular. Recalling that $\Phi(.)$ is a Normal CDF with mean $\mu = \Sigma_j \mu_j$ and variance $\sigma^2 = \Sigma_j \sigma_j^2$ (i.e. forthcoming from a convolution), we note that the mean and variance of $\Phi(A; N+1)$ is strictly greater than those of $\Phi(A; N)$. From this and the standard shape (properties) of a CDF, it follows that the "rise" (of the slope) for $\Phi(A; N+1)$ is less than or equal to that of $\Phi(A; N)$ for much of the domain. In particular, inequality 3 holds for most of the domain, except for when the combined actions of the group are sufficiently high. So, p (and accordingly A_i) is

[14] We assume normality since it is a commonly used distribution. That said, one could also assume other distributions such as exponential or gamma (for which the convolution would be gamma) or Cauchy (for which the convolution would be Cauchy). Moreover, in what follows we sometimes use Σ_j to represent Σ_j^N to avoid cumbersome notation.

[15] If one were to apply the implicit function theorem, the result would be somewhat comparable. Specifically, $\frac{\partial A_i}{\partial N} < 0$ if $A < \mu$. In other words, if the collective actions are below the collective mean, player i would reduce its contribution; if above the mean, i would increase it.

submodular and thus decreasing in N, except for when A is relatively high.[16] ■

Discussion. This finding suggests that larger groups will face greater difficulties coordinating. Specifically, unless a given member (player i) believes that others will jointly contribute a relatively high amount to the group, such member will contribute less when in a larger group than when in a smaller group.

One way in which beliefs about A_{-i}, p, can be shifted is through different types of "communication".[17] For example, suppose there is a pre-stage to the above game in which all players simultaneously reveal an *intended* action towards the group A'_j prior to taking the "true" action A_j.[18] Then, whether player i has a better or worse sense of p will depend on its prior. If player i believes it has perfect foresight, it will choose to ignore A'_j. However, a more likely scenario (reasonable assumption) is that player i will substitute A'_{-i} for A_{-i} in order to more precisely assess the likelihood of surpassing the threshold. Another way to think about this is that the distribution of beliefs in the presence of intended actions A'_j will stochastically dominate the one in absence thereof. Assuming that players use intended actions as sufficient for true actions, we can derive Hypotheses 2 and 3.

Hypothesis 2. For a group of size N, player i will set $A_i = V_i$ if revealed intentions are such that $\Sigma_j A'_j = A' \geq T$.

Proof. Given the assumption that player i takes A' as sufficient for A, it will set $p = 1$ since $A = A' \geq T$. Accordingly, $U(V_iH) > U(V_iM)$ and it makes sense to go "all in" with the group, i.e. $A_i = V_i$. This was also discussed previously when setting up the model. ■

[16] This finding is somewhat comparable to that of Heinemann et al. (2009). One way to visualize the flavor of the result in our context is to plot the S-shaped CDFs for N and $N + 1$ respectively. Loosely speaking, the CDF for $N + 1$ is a more "stretched" S and thus flatter on interior parts of the domain. However, at the extremes of the distribution, equation 3 is likely to be reversed. Due to symmetry, one could potentially argue that p is also supermodular when A is very low. However, that might be "economically" unreasonable. Furthermore, in our context, very low A is likely to coincide with A being negative and tending towards $-\infty$. Since $A \geq 0$, this is practically impossible.

[17] As referenced in Section 1, there is a fairly extensive theoretical and lab-experimental literature on the impact of communication, cheap talk, and strategic information transmission in games.

[18] Some of the literature on communication has suggested that players may seek to deceive others, particularly when sending non-costly and non-binding messages. In our context, the argument would be as follows. In experimental treatments where intentions are revealed (particularly the LFEs/games), a given player would send the highest possible intention in an attempt to influence others' actual contributions to the group. While sending such an intention is likely to be rational, it should also be noted that this player has no incentive to "free ride" (so to speak) off such a message. In fact, it is not as if this individual can get a higher payoff by sending a high intention, while reducing its actual contribution to the group – as would be the case, say in a Prisoner's dilemma setting. In other words, if a player truly believes that a high signal will cause others to increase their contributions to the group, such player should align its actual contribution with the intentions since their actions are strategic complements. So there are no benefits to lying/deceiving in this context, unless players misunderstand the game or expect others to and thus, "play fictitiously".

Discussion. It is fairly strict to assume that players will take intentions A'_j as perfect predictors of A_j. So, we also introduce the following hypothesis.

Hypothesis 3. For a group of size N, player i will increase (decrease) A_i if $A > \mu$ ($A < \mu$) when revealed intentions lead to a distribution p' that second-order stochastically dominates p, i.e. $\mu = \mu'$ but $\sigma^2 > \sigma'^2$.

Proof. While this can be shown using equation 2, a more straightforward approach is to envision the S-shaped distributions for p and p' respectively. Since p' second-order stochastically dominates p, the distributions will cross each other at $A = \mu$. For any value of A above μ, $p' > p$ regardless of T. The reverse holds for any value of A below μ. ■

Discussion. So, the impact of intended actions will depend on the location of others' (expected) actions. If those are sufficiently high (i.e. above the mean), players will increase their contributions to the group. If they are below the mean, players will reduce their contributions to the group.

Our final hypothesis speaks to the combined effect of changes in group size and the presence of intended actions.

Hypothesis 4. As the group size N increases, player i does not increase its contribution to the group A_i, unless revealed intentions A'_{-i} suggest that others' collective contribution to the group A_{-i} is sufficiently high.

Proof. This can be shown by combining the proofs of Hypotheses 1 and 3.

Discussion. This hypothesis implies that the impact of intended actions varies with the group size. More precisely, if intended actions suggest that the group is more likely to surpass the threshold, players are more likely to contribute in larger groups. On the other hand, if intentions indicate that the group is less likely to surpass the threshold, players are less likely to contribute in larger groups.

4 Study design

With the above context and theory in mind, our study is mainly designed to test whether (1) knowing others' aggregate intentions about collective commercialization impacts effective sales to the group and (2) this effect is more salient in larger versus smaller groups.

As mentioned in Section 1, we are unable to manipulate two key parameters discussed in the theoretical framework – the group size N and the threshold T – in a naturally-occurring setting. As such, group-size effects could be due to unobserved group attributes that are correlated with size; for example, the fact that larger groups may offer better services to farmers to begin with. Similarly, the threshold may be unobservable and vary at both the cooperative and the individual level. At individual level, the threshold may depend on a farmer's independent commercialization capacity or marketable surplus. At the cooperative level, the threshold may depend on the group's location characteristics (e.g. distance to large markets or processors) or the leadership's experience with commercialization.

Thus, we apply a two-stage empirical strategy. We fully test our theoretical predictions through a controlled, neutrally framed LFEs in which we are able to both observe and manipulate the group size and coordination threshold. We then test for the validity of our results in a naturally-occurring RCT where we do not manipulate the group size and coordination threshold. The LFEs and RCT were conducted six months apart in order to clearly separate outcomes. The timing of these experiments and related data collection was as follows:

1. *LFEs:* From May to June 2013, randomly selected farmers from 28 cooperatives participated in variants of neutrally-framed, high-stakes, stag-hunt-like coordination games (as explained in Section 4.1). A presurvey collecting basic (behavioral) characteristics was also administered at this time.

2. *RCT:* From November 2013 to February 2014, 79 farmer groups (including the former 27) participated in variants of intention-revelation treatments (as explained in Section 4.2). In particular, intentions were revealed 2-4 weeks prior to the start of the actual commercialization period.

3. *Post-survey and administrative data:* In June 2014, a follow-up survey was administered to 10-12 randomly selected members of the cooperative (median size is 24, ranging from 4-91) in order to gauge members' sales. Administrative data on individual members' commercialization were also collected from the books/records of the farmer groups for the 2014 season.

Because some groups that were selected for the RCT did not participate in the LFEs, we are able to control for LFE participation in all RCT-related estimations. Furthermore, because individuals were randomly selected for the LFEs, we are able to assess whether participation in this set of experiments affected subsequent behavior in the RCT. Table 1 summarizes the relationship between the LFE and RCT samples.

4.1 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 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. Table 2 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, approximately 5 US dollars), the equivalent of M, if held individually. So, players were explained that at the beginning of the game 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; in Section 3 this was normalized to zero). 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 the panel to the left of the "big plus" sign 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 $-j$ on a board in 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. Apart from the BCG and the CCG, we also randomly varied N, T, H, and the presence of external uncertainty (in addition to strategic uncertainty) as separate treatments.

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 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 subjects (sessions), 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/between 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 (in addition to strategic 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.

The experiments were conducted in vacant classrooms of village schools. Each experiment session comprised the following components: (1) a presurvey collecting basic information (available from authors upon request); (2) an introduction covering issues such as the purpose of the session (that is, to present participants with different decision-making scenarios) and the fact that participants would be paid for the decisions made during the session; (3) four rounds of decisions with no feedback, followed by debriefing; (4) a postsurvey collecting other information (available from authors 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 this region of 5,000 CFA. So, these LFEs could be considered relatively "high stakes".

4.2 RCT

We worked with 79 groundnut-producing cooperatives – including the 28 with which we implemented the LFEs – that are part of two umbrella federations (i.e. conglomerates of farmer cooperatives) in the Bassin Arachidier (the main groundnut production zone of Senegal). From November to December 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 briefing meeting with all cooperative members upon returning to their village, to report on 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 January 2014, 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 had to split their anticipated harvest into (1) individual commercialization, (2) collective (cooperative) commercialization, (3) inventories, and (4) other uses. 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 then allocated to one of four conditions, depending on the information that would be disclosed in the subsequent meeting (Table 3 summarizes the RCT design):

- In Group A (the control group), members' intentions were not revealed. Enumerators announced that a follow-up survey would be conducted after the end of the commercialization period. This announcement was also made in Groups B, C, and D.

- In Group B, members' aggregate intentions were revealed.

- In Group 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 Group D, the same information as in C was revealed, but the distribution of intentions was disaggregated by ordinary members versus cooperative leaders (defined as all members of the management committee which typically comprises 8 to 12 individuals). This treatment was inspired by the literature on leadership, which emphasizes that leaders' actions can have larger effects than that of regular members, in part because they are perceived as having privileged access to information (see for example Hermalin, 1998; Potters et al., 2007; Jack and Recalde, 2015).

Table 4 describes the characteristics of individuals who responded to the RCT-related survey and compares means across groups that received no information about others' intentions and groups that did. Consistent with the context described in Section 2, 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% 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% 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% of farmers' production.

Overall, Table 4 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. As discussed below, all RCT-related estimations will control for this – and other – individual characteristics that may be unbalanced at baseline. Table 4 also indicates that a greater number of farmers whose group was selected to participate in the LFEs were in the control group. To the extent that participation in the LFEs affects naturally-occurring behavior – which we assess later on – this difference could also introduce some bias. Our empirical strategy therefore includes this variable among the controls.[19]

4.3 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 production/resources 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:

$$A_{ig} = \alpha + \beta I_g + \varepsilon_{ig} \tag{4}$$

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 the theoretical framework), and the associated parameter β measures its effect on the dependent variable of interest (more below). ε_{ig} is a

[19]Table A3 reports the same set of tests across conditions A, B, C, and D. The results are similar.

composite error term defined as:[20]

$$\varepsilon_{ig} = \mu_g + \xi_{ig}$$

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.[21]

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% of group members attended the meeting in which aggregated intentions were revealed (see last row of Table 4). Without exogenous variation in attendance to the meeting, we estimate equation 4 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.[22]

Although we do not find evidence of major imbalances across conditions, we augment equation 4 with a vector of individual characteristics, 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 the individual's age, gender, 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).[23] In RCT-related estimates, this includes the individual's age, gender, 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:

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

[21]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.

[22]One could attempt to recover the impact on those who participated in the meeting by dividing the β estimate by the meeting participation rate. However, this would implicitly assume that the information provided in the meeting was not shared with non-attending group members, which we cannot guarantee.

[23]As mentioned previously, 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).

$$A_{ig} = \alpha + \beta I_{ig} + X_{ig}^{'}\rho + \varepsilon_{ig} \tag{5}$$

Finally, in line with our theoretical framework, we test for heterogenous treatment effects with respect to group size by estimating equation 6, 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 among all individuals in the sample; and δ captures the additional effect of this exposure for individuals in larger groups.

$$A_{ig} = \alpha + \beta I_g + \gamma S_g + \delta I_g \times S_g + X_{ig}^{'}\rho + \varepsilon_{ig} \tag{6}$$

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 theoretical framework (and notation) discussed in Section 3.

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 booklets discussed in Section 4.2); although such data do not include individual characteristics, X_{ig}. Finally, we also run some Tobit (to account for null reports) and inverse hyperbolic sine transformations specifications.

5 Results

5.1 Cheap talk and coordination

Tables 5 and 6 present our main results based on specifications 5 and 6. 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 (recalling there are no controls in this case).

In Table 5 we assess the extensive margin – i.e. the effect of revealing others' intentions on an individual's decision to contribute part of one's resources (i.e. number of chips in the LFEs, kilograms/kgs of groundnuts in the RCT) or not to the group. We find no direct effect of revealing aggregate intentions, be it in the LFEs or in the RCT (columns 1 and 5); however, interacting with group size yields support for hypothesis 4. In the LFEs, cheap talk (intentions) led to an 11 percentage point increase in the probability that one invests chips collectively, albeit limited to those sessions with groups of size 20 (as opposed to 10). The same is true in column 6, where revealing intentions increases the likelihood that one sold part of one's production through the cooperative by one percentage point for each additional group member. While insignificant, the direct effect of intentions is negative (-0.04 for LFEs and -0.08 for RCT), suggesting that the cheap-talk effect on increased coordination is limited to larger groups.[24] 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 very similar when using administrative data (column 8).

Results on the intensive margin are presented in Table 6 and show a similar pattern. While we do find a direct effect of cheap talk on the quantity of chips (column 1) and groundnuts (column 5) contributed to the group, this effect is only present in larger groups for both the LFEs (column 2) and the RCT (column 6). No such relationships are found in our placebo tests (columns 3 and 4) and the administrative data yield results comparable to those based on the individual survey data (columns 7 and 8).[25]

Overall, results from Tables 5 and 6 provide strong support for our theoretical predictions (in particular hypothesis 4): Cheap talk (intentions) can enhance coordination in situations of strategic uncertainty and this mainly seems to occur in larger groups, where coordination is more difficult to start with.[26] While consistent with our theoretical framework and prior work on the direct effect of cheap talk on coordination (recall theoretical and lab findings referenced in Section 1), these results do not line up with Feltovich and Grossman (2015) who find that the beneficial effect of cheap talk decreases as the group size increases. This said, their findings come from *public goods* games, which have a different payoff structure than coordination games (recall footnote 18). In addition, their group sizes are significantly

[24] As previously discussed, we are unable to exogenously vary group size in the RCT. Therefore, larger groups may be correlated with other characteristics and one should be cautious in interpreting the interacted effect as solely driven by group size. The fact that the RCT results align with those of the LFEs nevertheless support this interpretation. Further, in Appendix Table A4 we compare group characteristics between above- and below-median group size. We find limited evidence that these groups systematically differ from one another, except for being part of different umbrella federations, which we control for in all specifications.

[25] These results are robust to a Tobit specification (Table A6) and inverse hyperbolic sine transformation (Table A7).

[26] In the upper panel of appendix Table A5, we show that these results cannot be attributed to changes in individuals' risk, time, and social preferences, since those are unaffected by the RCT intervention.

smaller. In our LFEs, 10 is small and 20 is large, while in the RCT small constitutes less than 24 and large is 24 to 91. This compares to 2 (small) and 15 (large) in their experiments.

5.2 Collective commercialization and farmers' income

With the increased level of collective commercialization induced by the RCT intervention, we turn to its potential welfare effect. Specifically, we assess its impact on farmers' total income from the sale of groundnuts. This includes individual sales as well as those conducted through the cooperative. Since the intervention started shortly before the harvest, it did not affect farmers' production choices. Therefore, any effect of the intervention is mediated by a combination of (1) changes in the way farmers allocate their marketable surplus across individual versus collective sales and (2) changes in the price that may be obtained from collective sales due to greater aggregation of the product.[27]

The results are presented in Table 7. In column 1, we find no evidence of a direct effect of the intervention on farmers' income from groundnuts. In column 2 we extend the model to investigate whether the intervention differentially affected smaller versus larger farmers. We interact the treatment variable with the farmer's reported area of land that is dedicated to the production of groundnuts. While the direct effect of the intervention is large and positive, the coefficient associated with the interaction term is negative. In other words, larger farmers gain less from collective commercialization, in line with the existence of economies of scale in commercialization albeit with decreasing marginal returns. These results, combined with those in Section 5.1, possibly point to a "coordination-based poverty trap", characterized by: (1) smaller (poorer) farmers having more to gain from coordinated actions (collective commercialization); (2) successful coordination requiring a sufficient level of product aggregation, which in turn means a larger number of small farmers coordinating; and (3) coordinated actions being more difficult to achieve in larger groups.

Lastly, column 4 assesses the robustness of these findings by controlling for various individual and group characteristics. While the sign of coefficients remain, they are no longer statistically significant. Nevertheless, the coefficient on the direct effect of the intervention does indicate that providing information on aggregate intentions led to a meaningful increase in groundnut revenues for farmers in the treatment group.

[27]We are unable to use the per-unit price as our dependent variable. In fact, farmers tend not to negotiate price on a per-kg basis, but on the total price for a given quantity. Field work revealed that quantities collected from farmers were quite noisy and imprecise due to the lack of weighting scales for transactions that occur at the farm gate or at local markets. As such, we are unable to recover a per-unit price for those transactions conducted outside the group. Farmers are, however, able to recall how much they earned in total for each transaction, which is what we rely on here.

5.3 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 8). 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. One caveat to the analysis is that individual intentions and thus the aggregate, are endogenous, potentially reflecting other characteristics of the groups in question. Results are however robust to the inclusion of various individual and group characteristics in columns 3 and 4.

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 3). Table 9 presents the disaggregated impact of providing information on groundnut contributions to the cooperative, by informational treatment arm. 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 treatment groups, although with significant differences in magnitude. In particular, while knowing others' aggregate intention leads to higher contributions (Group B), the effect is more than two times larger in magnitude if one is also provided with the distribution of others' intentions (Group C).[28]

Two potential mechanisms may be at play here. First, one may expect a given aggregate to be more trustworthy when originating from a large 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 for them to expect that such individual would be better off selling alone. So, revealing the distribution of intentions may help individuals refine their expectations (consistent with our theoretical framework). Moreover, since most individuals intend to contribute in this setting, this might induce individuals to further revise their expectations about the final aggregate upward. Second, individuals in the group may ex-

[28]In addition to the information that was revealed, our treatments differed from one another in the way they were implemented. Since RCT Groups C and D by design revealed "distributions of intentions" also, their meetings were longer than those of RCT Groups B and A (the control group). One could argue that this extra time spent with farmers made the revealed intentions (or collective commercialization generally) more salient; thus confounding the observed impacts (e.g. Zwane et al., 2011). However, the large effect for RCT Group B (relative to A) suggests that there is a direct effect of the information that is provided. In practice, the meetings for B were less than two minutes longer than those for A. Moreover, if time spent with subjects were the main explanatory variable here, we would expect the impacts for C and D to be similar and if at all, for D to have a greater impact than C, which is not the case (see further below).

hibit 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" (see for example Bernheim, 1994). Some examples of this type of norm and information signaling are discussed by Vesterlund (2003), Gächter et al. (2010), and Hill et al. (2012).[29]

We find a smaller effect of revealing the distribution of intentions separately for leaders versus ordinary members (Group D) in comparison to the overall distribution (Group C). As mentioned previously, leaders may be perceived as having superior information relative to ordinary members.[30] In this case, that could mean that the leader for example has better knowledge of current market conditions.[31] So, it may be somewhat surprising that knowing leaders' intentions leads to positive, but lower contributions to the cooperative relative to knowing the overall distribution (Group C).

To further unpack this finding, we conduct an analysis comparable to that in Table 8, but now by informational treatment arm (see Table A8). 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. In other words, one reason why the impact of Group D may be smaller than that of Group 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 A8 suggests that the results are inconsistent with the above – providing information about aggregate intentions, the overall distribution, and the distribution by leaders versus ordinary members matters, however the actual quantities revealed do not seem to matter. In other words, the findings do not support the idea that members in Group D may have been deceived by the intentions provided/revealed by leaders.

Overall, while generally positive, we find differential effects depending on the type of information that is revealed. The quantity of intended actions revealed does not seem to matter.

[29] The latter study in particular discusses how a norm of reciprocity can unravel in the presence of peer-players' actions (as opposed to signals) in a rural context similar to the one discussed here.

[30] See for example Hermalin (1998), Potters et al. (2007), and Jack and Recalde (2015).

[31] Using data from 27 groundnut cooperatives in Senegal, Bernard et al. (2014) find that 75% of members believed that the management committee (i.e. leaders) had better knowledge and information than ordinary members on where to find good traders and/or current market and farm-gate prices for the region.

5.4 Participation in games and naturally-occurring behavior

Our design further enables us to assess how participation in the LFE coordination games affected later behavior in the naturally-occurring RCT. After the LFEs, all participants were asked to provide feedback regarding the game they had played. We had made sure that the game was neutrally framed and that there was no reference to collective marketing at any point. Still, many participants indicated that the coordination setting they had been faced with closely aligned with the situation they faced when deciding whether or not to sell their groundnut harvest through the cooperative. 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.[32] 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 10 where, in addition to the RCT treatment, we assess the impact of one's participation in the LFEs. We find no such effect in columns 1 and 2, where the dependent variable is one's intentions to commercialize collectively. In columns 3 and 4, however, 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 of the treatment: 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 theoretical framework in a highly controlled environment, researchers should take heed that they may be more than just that. In our setting, participation in the LFEs seems to improve farmers' understanding of the strategic complementarities that exist, which in turn impacts collective commercialization. Alternatively, participation in the games may have "fine-tuned" selected

[32] This decision was mainly driven by the fact that cooperatives are typically defined at the village level. With villages sometimes being far apart, we thus chose to (1) organize each session in the village school and (2) avoid further complications such as communicating decisions by phone as done by Hill et al. (2012). In appendix Table A9 we report tests for equality of characteristics between LFE participants and non-participants within groups that were selected for the LFEs. Overall, the results show that the two samples are balanced on most characteristics, except two which we control for in all related estimations.

farmers' expectations about fellow group members' subsequent behavior. These findings are consistent with anecdotes reported by for example Cardenas and Carpenter (2005), Hill and Viceisza (2012), and Viceisza (2016) as well as arguments in favor of classroom experiments (see for example a series of papers by Charles Holt and co-authors in *Journal of Economic Perspectives* in the late 1990s), all of whom have made the potential case for "learning through lab experiments/LFEs". They also contribute to the literature on generalizability of (lab) experiments (see for example Levitt and List, 2007; Al-Ubaydli and List, 2013; Camerer, 2015; Viceisza, 2016, and the references within) and a growing set of studies that combine findings from a spectrum of field experiments (see for example Barr et al., 2010; Finan and Schechter, 2012; Stoop et al., 2012; Hoel et al., 2017).

6 Conclusions

This paper reports a two-tier study on coordination between members of farmer cooperatives in rural Senegal. We rely on coordination games (i.e. artefactual lab-in-the-field experiments in which we can exogenously vary all parameters of interest) played by a subset of farmers and a naturally-occurring randomized controlled trial (i.e. natural field experiment) where we cannot exogenously manipulate all parameters, but have more immediate validity for policy. In both experiments, we test whether a "cheap talk" intervention, in which we reveal the group's aggregate intentions before individuals decide on their actual moves, can foster coordination. To the best of our knowledge, we are the first to create and test the impact of a subtle, low-cost "cheap talk/communication" institution in a (rural) field context, where strategic uncertainty and coordination failure play a primordial role. In line with our theoretical framework, we find across-the-board, consistent positive effects of cheap talk on coordination, but only in larger groups. These results are robust to the use of different datasets, i.e. farmer self-reports *and* administrative booklets, as well as several falsification tests that assess the effect of the intervention on individuals' intentions collected *prior to* such intervention. We also find evidence that revealing intentions led to higher revenues from groundnut sales during commercialization for smaller farmers. This is in line with current policies to promote collective commercialization in the presence of imperfect (market) competition, particularly when it concerns smaller farmers who tend to be less able to negotiate better prices in such environments.

Our findings also point at a number of avenues for future research. First, we investigate which type of information yields higher coordination, by varying how intentions are revealed to respondents in the RCT. We find that knowing the distribution of intentions is associated with higher coordination than the mere sum of intentions, possibly in line with the literature on conformity and interdependent preferences (see for example Bernheim, 1994; Manski, 2000; Sobel, 2005; Hill et al., 2012). Our results also suggest that knowing the distribution of leaders' intentions (versus that of ordinary members) yields somewhat lower effects than knowing the overall distribution.

Second, we find that participation in the LFEs (lab) impacts subsequent behavior in the RCT (naturally-occurring context). Those randomly selected to participate in the games were more likely to engage in collective commercialization, even though the games were neutrally framed. Consistent with Cooper and Van Huyck (2016) and anecdotes reported by, for example, Cardenas and Carpenter (2005) and Viceisza (2016), this suggests that such games can lead to learning, which is transferred to naturally-occurring behavior. We hypothesize that this may work through one or both of 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 expectations vis-à-vis group members when engaged in situations where strategic complementarity matters, e.g. collective commercialization. These findings contribute to the literature on generalizability of (lab) experiments and a growing set of studies that combine findings from a range of field experiments (e.g. Levitt and List, 2007; Al-Ubaydli and List, 2013; Camerer, 2015).

We close with an institutional-level, policy-design question: Given the simplicity of this intervention – i.e. merely collecting intentions and publicly revealing aggregates – why did these cooperatives, and arguably many others in Sub-Saharan Africa and elsewhere, not already come up with this solution? As put by Matsuyama (1997), page 135:

> *"If the coordination problem were simple enough that even the outsider, such as the economist and the bureaucrat, would know how to solve it, it would have been taken care of a long time ago by those directly involved with the problem."*

Of course, if our intervention had missed the mark by reinventing the wheel (so to speak), we should not have observed such clear and consistent impacts. One possible explanation is the fact that the study was carried out by "strangers" in an environment that is otherwise characterized by important interpersonal ties. Accordingly, participants may have been more willing to reveal their intended actions than if they were asked by peers. Similarly, participants may have found the aggregate intentions more credible when revealed by strangers than by group members who in principle could have "hidden agendas". We are, however, unaware of farmer cooperatives in this context, that failed at instituting a coordination mechanism of this type. Future research could investigate related issues.

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Tables

Table 1: 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 Group A	10	7	114	80	320	207
RCT Group B	15	6	173	68	640	201
RCT Group C	14	6	160	59	360	276
RCT Group D	14	7	156	72	418	331
Total	53	26	603	279	1738	1015

Group A is the control group in the RCT

Groups B, C, and D are treatment variations (see Section 4.2 or Table 3)

Table 2: Distribution of BCG and CCG variations

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

BCG = Baseline Coordination Game

CCG = Communication Coordination Game

Table 3: RCT treatments

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

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

Table 4: 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

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 5: 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:* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Standard errors are clustered at the group level. Columns 1 and 2 use data from the LFEs. 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 intentions collected before the RCT interventions. Columns 5 and 6 use the collective commercialization self-reported. Columns 7 and 8 use administrative data obtained from the cooperatives on collective commercialization. In columns 3–8, size is the actual group size ranging from 4-91 (with median size being 24). The coefficients are estimated using a linear probability model. Controls in the LFE regressions include age, gender, 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 (recall Table 4), 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 6: Intensive margin: treatment effect on quantities contributed to the group

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)
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)**
Control group mean	3.03	3.03	830.39	830.39	39.58	39.58	123.79	123.79
N	3,316	3,316	873	873	873	873	2,752	2,752

Notes: $***p < 0.01, **p < 0.05, *p < 0.1$. 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 intentions collected before the RCT interventions. Columns 3 and 4 use intentions collected before the RCT interventions. Columns 5 and 6 use the self-reported collective commercialization. Columns 7 and 8 use administrative data obtained from the cooperatives on collective commercialization. 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, gender, 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 (recall Table 4), 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 7: Effect on revenues from groundnut sales

	1	2	3	4
PO received some info (1= yes, 0 = no)	15.57	185.11	7.91	61.05
	(73.25)	(56.22)***	(51.46)	(39.07)
Size of land (ha)	3.48	45.62	0.85	14.28
	(3.55)	(20.68)**	(1.16)	(16.38)
Info * Land size (ha)		-43.33		-13.70
		(20.83)**		(16.47)
Mean control group		233.66		
R^2	0.01	0.06	0.34	0.34
N	868	868	868	868
Controls	No	No	Yes	Yes

Notes: $*p < 0.1; **p < 0.05; ***p < 0.01$. Standard errors are clustered at the group level. The dependent variable in all the regressions is the total revenue from selling groundnuts in thousands of fcfa (1000 fcfa was approximately equivalent to US$2 at the time of the survey). The last column adds as control variables age, gender, land size, generosity, risk aversion and time preference (recall Table 4), cooperative size, dummies for the federation, whether the farmer is a leader, and whether the farmer produced crops other than groundnuts, all measured pre-intervention, as well as a dummy for whether the farmer indicated positive intentions to sell through the group.

Table 8: Collective commercialization and aggregate intentions

	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)**
R^2	0.08	0.10	0.17	0.18
N	873	873	873	873
Controls?	No	No	Yes	Yes

Notes: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.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, gender, land size, generosity, risk aversion measures and time preferences elicited through hypothetical questions (recall Table 4), 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 9: Effect on quantity sold by treatment arms

Dependent variable	Contribution intentions (kg)		Quantity contributed (kg)		Quantity contributed (kg)	
Source	RCT - survey data		RCT - survey data		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)
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)*
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: $***p < 0.01, **p < 0.05, *p < 0.1$. Standard errors are clustered at the group level. 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. Controls in the regression include age, gender, land size, generosity, risk aversion, and time preferences (recall Table 4), 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 10: 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
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: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. 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, gender, land size, generosity, risk aversion, and time preferences (recall Table 4), 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 aids

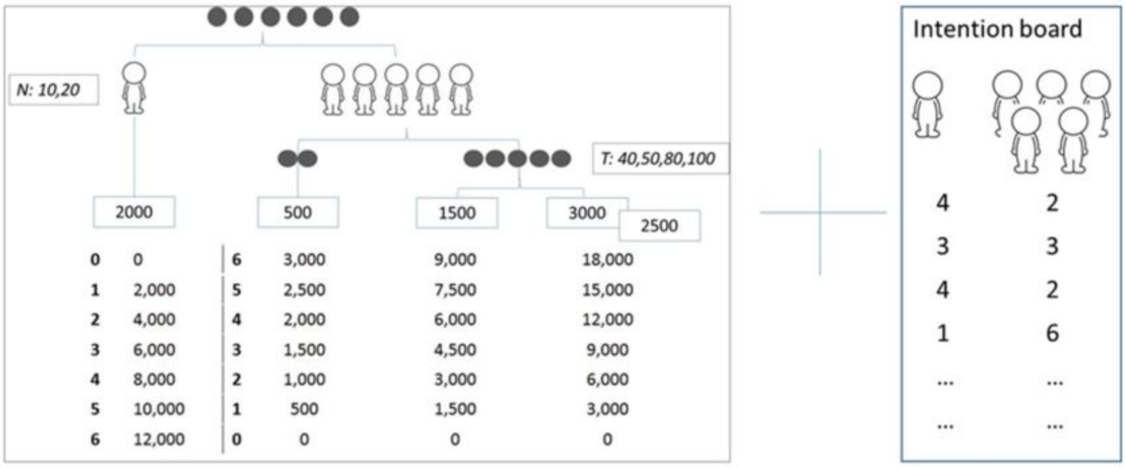

Table A1: Distribution of N, T, H and uncertainty variations in LFEs

Variable	N		T				H		Uncertainty	
	10	20	40	50	80	100	2,500	3,000	yes	no
Variation at	Session level		Round level				Round level		Session level	
Sessions	28	28	56	56	56	56	56	56	28	28
Rounds	112	110	86	86	24	26	111	111	108	114
Observations	1,120	2,196	1,160	1,160	478	518	1,658	1,658	1,720	1,596

Table A2: Correlation between parameter variations in LFEs

Variable	CCG	N	T	H	Uncertainty
CCG	1.00				
N	-0.08	1.00			
T	-0.05	0.51*	1.00		
H	0.00	0.00	0.00	1.00	
Uncertainty	0.04	-0.02	-0.01	-0.00	1.00

CCG = communication coordination game.

* Significantly different from 0 at the 10% level.

Table A3: Balance tests with respect to the treatment arms - 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**
Gender (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

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 A4: 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
Gender (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

$***p < .01, **p < .05, *p < .1$ 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. Recall Table 4 for definitions of generosity/social preferences, risk preferences, and patience/time preferences.

Table A5: Robustness check: effect of the treatment on patience, generosity and risk

	Patience	Patience	Generosity	Generosity	Risk	Risk
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:* $p < 0.1$; ** $p < 0.05$; *** $p < 0$. Standard errors are clustered at the group level. Recall Table 4 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, gender, 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 A6: Effect on collective commercialization - Tobit model

	1	2	3	4
Treatment	278.60	-622.40	460.23	-88.53
	(295.31)	(550.79)	(258.33)*	(512.10)
Size of PO	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)
Pseudo R^2	0.03	0.03	0.04	0.04
N	882	873	873	873
Control	No	No	Yes	Yes

Notes: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. 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, gender, land size, generosity, risk aversion measures and time preferences elicited through hypothetical questions (recall Table 4), 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 A7: Effect on collective commercialization - inverse hyperbolic sine transformation

	1	2	3	4
Treatment	0.42	-1.13	0.64	-0.52
	(0.39)	(0.68)	(0.34)*	(0.63)
Size of PO	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)*
R^2	0.10	0.11	0.17	0.17
N	882	873	873	873
Control	No	No	Yes	Yes

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Notes: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. 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, gender, land size, generosity, risk aversion measures and time preferences elicited through hypothetical questions (recall Table 4), 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 A8: Collective commercialization and leader's intentions

	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 simple 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 simple member	-0.10	-0.06		
	(0.07)	(0.08)		
Difference intentions leader - simple member ≥ 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)***
R^2	0.15	0.19	0.14	0.18
N	868	868	868	868
Control	No	Yes	No	Yes

Notes: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. 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, gender, land size, generosity, risk aversion measures and time preferences elicited through hypothetical questions (recall Table 4), 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: Comparing farmers attending games to farmers not attending, within LFE coops

	N	All	Farmers in games No	Farmers in games Yes	P-value of diff.
Age	279	45.05	46.85	44.44	0.07*
Gender (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: $***p < .01, **p < .05, *p < .1$ 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. Recall Table 4 for definitions of generosity/social preferences, risk preferences, and patience/time preferences.