Communication and coordination: Experimental evidence from farmer groups in Senegal

Fo Kodjo Dzinyefa Aflagah[1], Tanguy Bernard[2,3], Angelino Viceisza[4]

June 2015

Abstract

Coordination failures are at the heart of development traps. While communication can reduce such failures, to date, experimental evidence has primarily been lab-based. This paper studies the impact of communication in stag-hunt coordination games played by members of Senegalese farmer groups – a setting where coordination failure towards collective commercialization has prevailed, as in many rural contexts. We find that communication only increases coordination in larger experimental groups, where it matters most from the standpoint of poverty traps. We also find that these effects are driven by communication's impact on perceptions of strategic uncertainty. Some policy implications are discussed.

Keywords: Coordination, strategic uncertainty, communication, cooperatives, field experiments, development

JEL Codes: C72, C93, D71, O12, Q13

[1] University of Maryland, College Park, MD 20742, aflagah@econ.umd.edu.
[2] University of Bordeaux, Avenue Léon Duguit, LAREFI - Bureau R 190, 33608 Pessac, tanguy.bernard@u-bordeaux.fr.
[3] IFPRI, 2033 K Street NW, Washington, DC 20006, t.bernard@cgiar.org.
[4] Spelman College, 350 Spelman Lane SW, Atlanta, GA 30314, aviceisz@spelman.edu.

1. Introduction

Economic growth and development depend on production, which requires coordination. As Wydick (2007) discusses, strategic interdependence and coordination among the economy's different players are central to the Big Push idea originally conceived by Rosenstein-Rodan (1943). For example, the American Big Push came about due to, among other factors, complementary investments in key industries, which in turn generated the economic momentum necessary for economic growth. Given their applicability to many areas of economics, coordination games and the related concepts of "strategic uncertainty" and "coordination failure" have featured prominently in the literature.[5]

Consider the two-player coordination game in Figure 1 owing to Cooper et al. (1992, page 741).[6] There is strategic uncertainty as to how the opponent will play the game and given such uncertainty, strategy 1 is deemed "safe" since a player always receives 800. Coordination failure occurs when both players choose the safe strategy and end up in the Pareto-inferior Nash equilibrium (1,1).

Figure 1: Coordination game

		Column Player's Strategy	
		1	2
Row Player's Strategy	1	(800,800)	(800,0)
	2	(0,800)	(1000,1000)

With coordination failure at the heart of certain development (poverty) traps, a key policy question is *'how to mitigate it'*. This has led to a related literature on

[5] Diamond (1982), Bryant (1983), van Huyck et al. (1990), Battalio et al. (2001), Battaglini and Bénabou (2003), Heinemann et al. (2009), Ostrom (2010), Aguiar et al. (2015), and several other references discuss contexts in which the returns to a player from undertaking a particular action are impacted by the (un)coordinated actions of other players.

[6] As Crawford (1991) highlights, the basic structure of this game is similar to that of the "stag hunt" parable discussed by Rousseau (1973). This game is different from a typical Prisoner's dilemma (PD), because one player cannot free ride off the other. In other words, a given player can do NO better by choosing the "safe" strategy, 1; whereas, that IS the case in the PD game (by "ratting out" the other player).

communication (cheap talk)—in particular, as a potential mechanism for increasing coordination. Most of this literature is theoretical and/or based on conventional laboratory experiments.[7]

In this paper, we revisit whether and if so how communication improves coordination by addressing these questions in a field context where strategic uncertainty has historically led to coordination failure and that represents much of the rural developing world. We study farmer groups in Senegal that seek to sell agricultural production collectively. To understand the relevance of coordination and strategic uncertainty in this context, consider the following.

Figure 2: Scale-dependent profit function

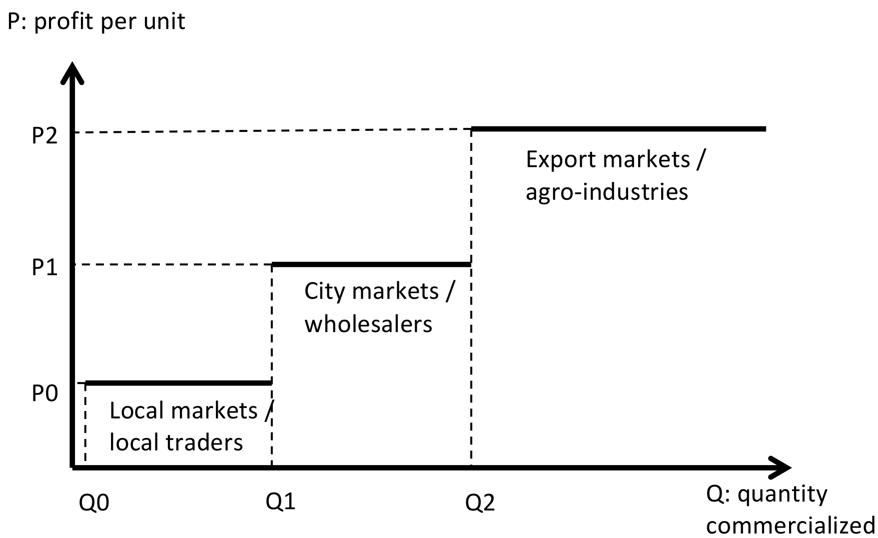


Figure 2 shows the scale-dependent profit function that farmers typically face, due to the presence of fixed transaction costs.[8] Unable to spread the fixed costs of

[7] See for example Crawford and Sobel (1982), Farrell (1987, 1988), Cooper et al. (1992), Farrell and Rabin (1996), Crawford (1998), Ben-Porath (2003), Charness and Grosskopf (2004), Charness and Dufwenberg (2006, 2010, 2011), Levy and Razin (2007), Chen et al. (2008), Kartik (2009), Ganguly and Ray (2010), Agranov and Yariv (2015), and Feltovich and Grossman (2015).

[8] See for example Key et al. (2000) as well as some of the claims made by Ashraf et al. (2009).

reaching wholesale markets on a sufficiently large quantity of output, small-scale farmers (Q0 to Q1) tend to sell in local markets that are characterized by spot transactions with local traders. Medium-scale farmers (Q1 to Q2) tend to sell in city markets that are more often characterized by contract transactions. Finally, large-scale farmers (beyond Q2) also tend to sell by contract, but typically do so to export markets or local agro-processors. Given this hierarchy, it is customary for small-scale farmers to get lower profits per unit (P0) than medium-scale (P1) and for medium-scale farmers to get lower profits than large-scale (P2).

Coordination is important because—in this and many rural contexts—farmers tend to be small-scale. So, if a farmer were to sell independently, she would obtain P0. However, by selling collectively, the farmer can behave as if she were a medium- to large-scale producer, spread fixed costs over a larger quantity of (aggregated) output, and reap higher profits per unit. Like the Big Push, this collective marketing requires coordination. Thus, the payoffs to an individual farmer from selling through the group are strategically uncertain—they depend what other farmers do (similarly to strategy 2 in Figure 1).

Previous data, in particular those by Bernard et al. (2014), show that more than half of the farmer groups in this sample have been unable to commercialize collectively despite their intent to do so.[9] One of the principle reasons cited by group members is the uncertainty that other members will actually sell through the group when the time comes. Two thirds 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.[10] So, the question remains: How to mitigate coordination failure?

[9] Similar evidence is found in Uganda (Fafchamps and Hill 2005), in Central America (Hellin et al. 2007), in Asia (Aldana et al. 2007), in Burkina Faso (Bernard et al. 2010), in Ethiopia (Bernard and Taffesse 2012), and in the Democratic Republic of Congo (Ragasa and Golan 2014).

[10] This is likely reinforced by the fact that farmer groups seldom sanction members who engage in side selling. In fact, usually no formal contract is established between members and the group. So,

This study tests for the relevance of N-way pre-play communication as a means to foster coordination in existing farmer groups by conducting artefactual field experiments with randomly selected members of such groups. In our baseline treatments, farmers play neutrally framed, high-stakes stag-hunt coordination games in experimental groups of size N equal to 10 or 20. In the communication treatments, farmers play these same games, but prior to doing so, they engage in N-way structured, preplay communication—farmers are asked to reveal their intended actions to the group.

Like previous lab studies, we find that communication significantly reduces coordination failure. However, this finding is context-specific. Coordination is more difficult in groups of size 20 (versus 10) and N-way communication contributes to overcoming this group-size constraint. These lab findings have an important policy implication. While collective action may generate the type of economies of scale needed for small-scale farmers to access more profitable markets, seizing such market opportunities also requires coordination by larger groups (that is, groups with more "small" members). But, it is exactly larger groups that have difficulty coordinating, making their members more susceptible to poverty-driven coordination traps. Since we find that communication works in groups of size 20 (versus 10), the results imply that communication has an impact exactly where it is needed—in larger groups that potentially comprise the smallest-scale farmers. Using treatment variation and additional survey data, we find that communication works through a primary mechanism: It plays a reassurance role by increasing (decreasing) coordination due to reduced uncertainty around other players' actions.

Despite its potential to inform policy, to the best of our knowledge, there is limited evidence on the impact of communication on coordination in the field,

there is no mechanism to appeal to a court of law. Further, as these groups are located in villages with dense social ties, they are rarely able to exclude anyone from continued membership.

particularly when the setting has historically been conducive to coordination failure.[11] We thus designed these artefactual field experiments in order to assess whether structured, N-way communication has the potential to improve coordination in such contexts. Despite their artefactual nature, these experiments also shed light on what a naturally occurring communication institution could look like. In fact, as a follow-up to these experiments, we designed natural field experiments that implemented communication in a similar manner (see for example Barr et al. (2014) and de Arcangelis et al. (2015) for comparable approaches).

The remainder of the paper is organized as follows. Section 2 develops testable hypotheses in order to discuss the experimental design, protocol, and empirical strategy. Section 3 presents the main results. Finally, Section 4 concludes.

2. The experiments

Given we are primarily interested in assessing whether and if so, how communication affects coordination, our experimental design rests on two main treatments: (1) a baseline coordination game (BCG) and (2) a pre-play communication coordination game (CCG). These treatments, which will be discussed in greater detail below, were randomly assigned across groups of subjects. These experimental groups were created by randomly drawing members of existing farmer groups to form sets of players of size N equal to 10 or 20, with all players in a given session originating from the same real-life farmer group.[12] We also conducted supplementary treatments that varied some of the parameters in the BCG and the CCG (we discuss those further below).

[11] See Alzua et al. (2014) for a study on cooperation (public goods games) and communication.

[12] A variation could have been to create experimental groups comprising farmers who were not members of (1) the same real-life group or (2) a group at all. However, since we are interested in the potential effect of communication in existing real-life farmer groups, we created experimental groups by randomly selecting members from the same real-life group. Dividers separated subjects in order to maintain the simultaneous nature of the games and mitigate peer effects (Figure 2).

2.1. The BCG, the CCG, and variations

In the BCG, each player i has an endowment, $E \in \mathbb{N}$. Player i chooses to send $A_i \in \{0,1,\dots,E\}$ to the N-player pool (the equivalent of selling through the group, strategy 2 in Figure 1) and keep the remainder $E - A_i$ for herself (the equivalent of selling individually, strategy 1). The player's monetary payoff Π_i is determined as follows. A_i earns a high return of H if all players (including player i) jointly send an amount $A \geq T$ (where T represents some threshold) to the N-player pool. If not, that is if they send an amount $A < T$, A_i earns a low return of $L < H$. Whatever the player chooses to keep individually, that is $E - A_i$, earns a medium return of M, where $L < M < H$.[13]

Player i's expected payoff is thus represented by the following expression:

$$\mathrm{E}\Pi_i = A_i(pU(H) + (1-p)U(L)) + (E - A_i)U(M)$$

where p is the probability that all other players $-i$ jointly send at least $T - A_i$ to the N-player pool. So, we have: $p = P(\Sigma_{-i=1}^{N-1} A_{-i} \geq T - A_i)$.

The Nash equilibrium of this game requires that each player i maximize $E\Pi_i$, giving rise to the following first-order condition (FOC):

$$\frac{\partial E\Pi_i}{\partial A_i} = 0 \Rightarrow p[U(H) - U(L)] + U(L) - U(M) = 0 \Rightarrow$$

$$P(\Sigma_{-i=1}^{N-1} A_{-i} \geq T - A_i)[U(H) - U(L)] + U(L) - U(M) = 0$$

which implicitly defines the function $G(A_i, A_{-i}, N, T, H) = 0$.

Thus, in equilibrium, a given player i's action A_i depends on (1) beliefs (P) about other players' actions (A_{-i}) relative to the net threshold ($T - A_i$); (2) the number of players (N); (3) the payoff trade-off between sending to the group, which pays

---

[13] $L < M$ if $A < T$ because this compares to a scenario where the group contracts with a wholesaler or agro-processor, but breaches the contract because an insufficient number of farmers comply when the time comes. This costs those who *have* committed in at least three ways, compared to had they sold individually to begin with. First, they need to find alternative buyers (likely locally) when the market is already saturated. Second, these profits are reaped later and thus are less valuable from a time value standpoint. Finally, this breach of contract has reputational costs in the long run, since the buyer is unlikely to contract with them again.

H or L depending on other players' actions (strategic uncertainty), and keeping individually, which pays M regardless of other players' actions; and (4) the properties of the utility function U (in the empirical analysis, we will control for risk, time, and social preferences as well as other characteristics). We formalize these claims when discussing the comparative statics in Section 2.2.

The CCG is identical to the BCG with one exception: Players have the ability to communicate in a very structured manner. Prior to choosing and committing to A_i, each player sends a nonbinding signal $S_i \in \{0,1,\dots,E\}$ of how much she plans to send to the N-player pool. This signal, which is revealed to all other players $-i$, indicates a respective player's likely action. However, it is not a binding commitment and as such, other players cannot know with full certainty that A_i will be the same as S_i. Furthermore, the player's identity is not revealed with the signal. In other words, all other players know that *some* player sent signal S_i, but they do not know who i actually is. This is the only form of communication that is allowed in the CCG.

Apart from the BCG and the CCG, we also varied N, T, H, and the presence of external uncertainty in addition to strategic uncertainty. We discuss the exact parameterizations when detailing the protocol.

2.2. Hypotheses and mechanisms

We are primarily interested in whether and if so, under what conditions a given player's action A_i varies with the presence of communication. However, prior to hypothesizing the potential effect of communication, we start with basic comparative statics relative to N, T, H, and external uncertainty.

As derived above, in the BCG, players' actions (A_i) are driven by the following FOC: $G \equiv P(\Sigma_{-i=1}^{N-1} A_{-i} \geq T - A_i)[U(H) - U(L)] + U(L) - U(M) = 0$. Under reasonable assumptions on the probability mass function (PMF, $P(.)$), its associated cumulative distribution function (CDF, $F(.)$), and player i's location in the action space (that is, whether A_i is low or high), we can use this FOC to prove the following hypotheses (see appendix for proofs of all hypotheses):

H1: As the group size (N) increases, player i decreases the number of chips sent to the group (A_i).

H2: As the threshold (T) increases, player i increases A_i.

H3: As the premium (H) increases, player i increases A_i.

H4: As external uncertainty surrounding the premium increases, player i decreases A_i.

Turning to the hypotheses surrounding communication, we note that in the CCG, player i uses the signals sent by others S_{-i} to inform her action A_i. Specifically, player i substitutes S_{-i} into the FOC above to get $G^* \equiv P(\Sigma_{-i=1}^{N-1} S_{-i} \geq T - A_i)[U(H) - U(L)] + U(L) - U(M) = 0$. Under the same assumptions on $P(.)$, its associated CDF, $F(.)$, and player i's location in the action space, we can use this FOC to prove the following hypothesis:

H5: Player i's action A_i is impacted by the presence of N-way communication in the form of S_{-i}. See H9 and H10 for more on the direction and mechanism.

We can also show that the communication effect depends on other experimental variations, leading to the following hypotheses:

H6; H7; H8: A_i is impacted differentially by the presence of N-way communication in (*) groups of different sizes; (*) when facing different thresholds; (*) when the premium is low or uncertain.

The above comparative statics explore conditions under which communication may matter. We are also interested in exploring some mechanisms by which communication affects players' actions. In order to develop these hypotheses, we turn to some of the existing literature on cheap talk, communication, and social interactions.[14]

First, as reviewed by Farrell and Rabin (1996) and Crawford (1998), partly based on the findings of Farrell (1987, 1990) and Cooper et al. (1992), the role of communication depends on (a) the type of game under consideration and (b) whether communication is structured and/or N-sided. The BCG and the CCG are stag-hunt coordination games with multiple and asymmetric equilibria. Furthermore, in the CCG, communication is structured and N-sided. Noting these characteristics, the previous references suggest that communication should primarily play a *reassurance* role. This means that communication should drive farmer subjects to coordinate on more efficient equilibria due to reduced uncertainty about other players' actions.

Strictly speaking, this means that communication should on average increase (reduce) amounts sent to the group (kept individually). However, reassurance can either increase or decrease contributions to the group. To formalize this argument, consider the set of signals sent by all players, $\{S_1, \ldots, S_i, \ldots, S_N\}$. Under the assumption that the sum of these signals across all players, that is, $S = \Sigma_i S_i$, is a decent approximation of $A = \Sigma_i A_i$, a given player will compare S to the threshold T that needs to be surpassed in order for amounts sent to the group (A_i) to earn a high return (H). So, the player now has a more accurate way of informing her belief about p. If S is sufficiently close to T from below or surpasses it, the player should send her whole endowment to the group in order to maximize her payoffs. On the other hand, if S is well below the threshold—signaling that there is 'no hope' for coordination on good equilibria—the player should keep her whole

[14] Some reviews on communication and cheap talk were cited previously (see Farrell and Rabin 1996; Crawford 1998; and Ganguly and Ray 2010). For some discussions of social interactions and interdependent preferences, see Manski (2000) and Sobel (2005).

endowment. This intuition leads to our ninth hypothesis, which expands on H5 and focuses on *strategic uncertainty and reassurance*:

H9: Player i's action is impacted due to changes in strategic uncertainty resulting from N-way communication in the following way:

(a) If S is close to (from below), equal to, or greater than T, then A_i should be equal to E.

(b) If S is well below T, then A_i should be equal to zero.

Communication should also impact players' beliefs (about others' beliefs) about strategic uncertainty. Theoretically, the role of second- and higher-order beliefs has primarily been formalized by the psychological games literature (see for example Geanakoplos et al. 1989 and Battigalli and Dufwenberg 2009). Some recent empirical references are Charness and Dufwenberg (2006, 2010). While we did not explicitly elicit these types of beliefs (due to the complexity/time these would have added to the experiment protocol), H2, H3, and H5 can be seen as indirect tests of beliefs. When discussing the results, we also explore robustness of our treatment effects with regard to survey proxies for pre-existing beliefs (trust) towards one's group members.

Second, as reviewed by Manski (2000) and Sobel (2005), when engaging in social interactions, agents (in particular, players in a game) may exhibit interdependent (social) preferences. Thus far, we have ignored such complications by assuming that a given player maximizes her own expected monetary payoff $E\Pi_i$. However, if a player exhibits interdependent preferences V_i over her and others' monetary payoffs, that is $V_i = f(E\Pi_i, E\Pi_{-i})$ (with nonzero first-order derivatives), this can give rise to social norms of equity and fairness. While these effects may exist even in the absence of communication, they may be particularly salient in the CCG since communication can be interpreted as signaling what other players consider 'the right thing to do'. Some examples of this type of 'norm and information' signaling are discussed by Vesterlund (2003), Gaechter et al. (2010), and Hill et al. (2012). 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.

In the CCG, players were exposed to the set (distribution) of signals that other players sent to the group. If a given player sees the set of signals as a norm for how to behave, she may adapt the amount sent to the group A_i to conform to the typical expected behavior of the group (see for example Bernheim 1994). Under the assumption that S_{-i} was a good approximation for A_{-i}, we have the following hypothesis:

H10: Player i's action is impacted by N-way communication in the following way: A_i may approximate the median of others' signals S_{-i}.[15]

Finally, some of the literature on communication has suggested that players may seek to deceive others when sending signals. In our context, the argument would be as follows. In the CCG, a player would send the highest possible signal in an attempt to influence others' contributions to the group. While sending such a signal is likely to be rational, we also note that a player has no incentive to "free ride" off such a signal. Given the stag-hunt nature of the BCG and the CCG, it is likely that a player who sends such a signal recognizes the *reassurance* role that communication can have (see previous discussion). This said, it is not as if this player can get a higher payoff by sending a high signal and reducing her actual contribution to the group – as would indeed be the case in a Prisoner's Dilemma setting. In fact, if she truly believes that a high signal will cause other players to increase their contributions to the group and thus surpass T, she should actually send all her endowment to the group. So, there are no incentives to lying/deceiving in this context. Even if there were any such incentives, we note two things. First, the empirical evidence seems to suggest that players are averse

[15] We also explore robustness of our treatment effects to a proxy for social preferences (hypothetical dictator game).

to lying/deceiving (see for example Gneezy et al. 2013). Second, some of our analysis looks at the difference between players' signals and their actions (that is, revisions from signals). This controls for such confounds.

2.3. The protocol

The experiments were conducted using pencil and paper in vacant classrooms of village schools. Each experiment session comprised the following components:

1) A pre questionnaire collecting basic information (available upon request).
2) An introduction covering (a) the mission of the International Food Policy Research Institute, (b) the purpose of the session (that is, to present participants with different decision-making scenarios), and (c) the fact that participants would be paid for the decisions made during the session.
3) Detailed instructions (available on the authors' websites).
4) Four rounds of decisions with no feedback, followed by debriefing.[16]
5) A post questionnaire collecting other information (available upon request).
6) Payment in private based on one randomly selected round.

The sessions lasted two and one half to three hours and average earnings were 9500 West African francs (CFA, approximately equivalent to 20 United States dollars), relative to a daily wage equivalent in this area of 5000 CFA. So, these experiments could be considered relatively "high stakes".

During the instruction phase, action and payoff sets were explained using several visual aids at the front of the room (see Figure 3). The visual aids were introduced systematically as the instructions moved along and varied depending on the treatment protocol under consideration. We start with an explanation of the BCG protocol. Then, we elaborate on the CCG and other treatment protocols.

[16] There was one session in which only two of the four rounds could be conducted due to subject time constraints.

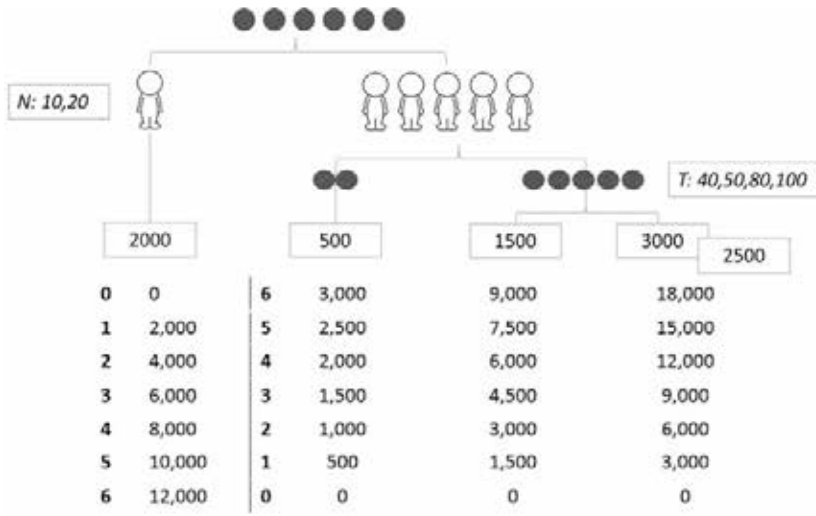
Figure 3: Sample session

In the BCG, each player had an endowment E of six chips.[17] Each chip was worth 2000 CFA (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 presented as payment for the pre-survey.[18] The payoff for each chip sent to the group was dependent on whether or not the threshold (T) was reached/surpassed. If $A \geq T$, each chip was worth 3000 CFA (H); if not, each chip was worth 500 CFA (L). So, each player had to decide how many of the six chips to send to the group (A_i) and how many to keep individually ($6 - A_i$), as shown in Figure 4.

[17] We did not vary E across individuals in order to keep the protocol simple and mitigate potential feelings of unfairness among subjects. We also did not vary E across treatments since we already had several variations across subjects and/or rounds. While in reality, farmers face different "endowments" (production quantities) due to factors such as land size, ability, and access to inputs, what primarily matters here is that we varied the group-level thresholds required for coordination.

[18] Even if such effects exist, we note that our main comparisons are across treatments.

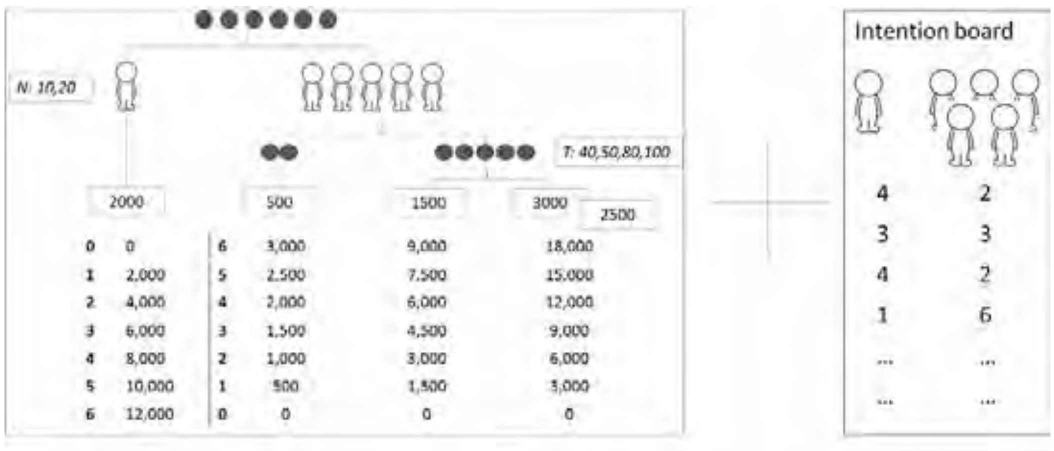
Figure 4: Visual of BCG protocol



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 (see bottom-left picture in Figure 3). Second, many hypothetical examples were used. Among these were situations in which the experimenter and his assistant acted through several payoff scenarios. We further tested subject understanding by periodically asking specific players to calculate such payoffs. A substantial part of the experiment session was dedicated to the instruction phase.

In the CCG, the exact same procedure was followed except that prior to the subjects making their actual decisions (A_i), the experimenter went around the room and asked players to reveal their intended actions/signals (S_i). Subjects were explained that this information would be collected by the experimenter and displayed on a separate board at the front of the room. The order of signals was random such that players did not know who sent which signal. It was made clear that this was an intended, but non-binding action. Figure 5 shows the logic behind the CCG. It was identical to the BCG, except for an additional board, which contained randomly ordered signals S_i and the aggregate S. The bottom-right panel of Figure 3 shows an example of the completed intention board in a session.

Figure 5: Visual of CCG protocol



The BCG and the CCG were our main treatment variations. They were implemented between subjects (sessions), since introducing communication mid-session would have complicated the protocol. We varied the following:[19]

1. The (experimental) group size, N, was fixed at either 10 or 20 during a session. So, N was varied across sessions/between subjects. In the naturally occurring environment, groups also vary in size.
2. The threshold, T, was 40 or 50 in 10-person groups and 40, 50, 80, or 100 in 20-person groups. T was varied across rounds. In the day-to-day environment, a minimum quantity is typically required to satisfy a contract and make it economically worthwhile to incur transportation and storage costs. In particular, larger groups may enter into larger contracts, since groups consider per-capita contributions.
3. The premium, H, was either 3000 or 2500 CFA per chip. H was varied across rounds. In the everyday environment, the premium to collective marketing (benefit from a contract) tends to vary with market conditions.

[19] These were chosen to mimic existing constraints in the commercialization environment, although never presented as such during the experiment sessions in order to preserve the neutral framing. Post-session debriefing however indicated that many participants related to the experiment's features to their groups' issues at time of collective commercialization.

4. Whether or not there was external uncertainty in addition to strategic uncertainty. This was implemented as follows. Subjects were informed that there was a 50 percent chance that due to bad luck the premium H would be 1500 CFA per chip (instead of 2500 or 3000). This was varied across rounds by flipping a coin. In the day-to-day environment, exogenous shocks impact group-level prices, even when the group manages to negotiate a contract.

Tables 1, 2, and 3 show the number of observations per treatment and assess the extent to which the treatments are orthogonal to each other. By design, there is significant correlation between the N and T treatments, since only large groups were exposed to thresholds of 80 and 100.

Table 1: Distribution of BCG and CCG

	CCG	BCG	Total
# sessions (s)	28	28	56
# rounds (r)	110	112	222
# players (i)	410	429	839
# observations	1600	1716	3316

Table 2: Distribution of N, T, H, Uncertainty treatments

	N		T				H		Uncertainty	
	10	20	40	50	80	100	2500	3000	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	1120	2196	1160	1160	478	518	1658	1658	1720	1596

Table 3: Correlation between treatments

	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

** Significantly different from 0 at the 5% level.

2.4. The sample

The experimental groups were randomly drawn from a sample of 28 Senegalese farmer groups that primarily produce groundnuts and seek to sell those collectively.[20] From each farmer group, two experimental groups—one of size N=10 and the other of size N=20—were randomly drawn.[21]

These experimental groups were selected from an up-to-date and complete list of farmer group members, which was collected during a previous field visit. The list included farmers' cell phone numbers such that selected farmers could be called/invited directly. This was an important part of the recruitment protocol, since contacting farmers via their day-to-day groups or leaders was likely to bias their behavior in the experiment towards "collectivism". In all cases, we randomly selected/invited extra participants in case some subjects could not make it. On the day of the experiment, participants were admitted to the session on a first-come-first-serve basis. Upon arrival, subjects drew a random number from a bag, which determined their seat during the experiment session.

Some basic characteristics of our sample, particularly across the BCG and the CCG, are included in Table 4. As the table suggests, 53% of the sample is female; 61% went to Koranic school; and the sample holds an average of 4.81 hectares of land. Overall, the BCG and CCG samples do not seem to be significantly different from each other based on this set of observable characteristics. The only exception is that there are more women in the BCG than in the CCG. To control for this and any other possible confounds, we typically include a set of covariates in our regression analysis, as explained next.

[20] Bernard et al. (2014) discuss additional details and background history on these farmer groups

[21] Four farmer groups were not large enough to accommodate two experimental groups. So, additional experimental groups were drawn from other farmer groups that were sufficiently large.

2.5. Empirical strategy

Our first set of estimations tests H1-H4 and takes the following form:

$$A_{ir\tau} = \alpha + D'_{r\tau}\delta + X'_{ir\tau}\gamma + \rho R_r + \mu_\tau + \varepsilon_{ir\tau} \quad (1)$$

The main outcome variable in this specification is the number of chips that individual i sent to the group in round r of session τ, $A_{ir\tau}$. The independent variables are: $D_{r\tau}$, which is a set of dummies for the other treatment variations (N, T, H, Uncertainty); X, which is the set of individual-level covariates; and R, which stands for a set of round dummies. μ_τ is a session-level error term, which we account for by clustering standard errors at session level, while $\varepsilon_{ir\tau}$ is an independent, individual-specific error term.

Our second set of estimations tests H5-H8 and takes the following form:

$$A_{ir\tau} = \alpha + \beta_1 C_\tau + D'_{r\tau}\delta + C_t D'_{rt}\beta_2 + X'_{ir\tau}\gamma + \rho R_r + \mu_\tau + \varepsilon_{ir\tau} \quad (2)$$

where all is as defined previously, except that we now include C_τ, which is a dummy indicating whether the session was CCG, and interact C with the other treatment dummies D.

Finally, our third set of estimations tests H9 and H10. Given the purpose is to assess the mechanisms by which communication is occurring, we focus our attention on data from the CCG sessions only. This set of specifications primarily takes the following form:

$$A_{ir\tau} - S_{ir\tau} = \alpha + M'_{ir\tau}\beta + S'_{ir\tau}\lambda + D'_{r\tau}\delta + X'_{ir\tau}\gamma + \rho R_r + \mu_\tau + \varepsilon_{ir\tau} \quad (3)$$

The main outcome variable in this specification is the difference between the actual amount a player sent to the group, $A_{i\tau r}$, and her signal, $S_{ir\tau}$. The main independent variables are as in specification (1), except for $S_{ir\tau}$ and $M_{ir\tau}$, which represents (1) the per-capita distance between the threshold T and the aggregate signals $S = \Sigma_i S_i$ (as a test of H9) and (2) the distance between an individual's signal and the median signal sent by the group (as a test of H10).

Table 4: Average sample characteristics (overall, BCG, CCG, difference)

	Overall	BCG	CCG	Difference
Gender (1=male, 2=female)	0.53	0.57	0.48	0.10**
	(0.50)	(0.02)	(0.03)	(0.03)
Land size (hectares)	4.81	4.52	5.11	-0.60
	(5.42)	(0.26)	(0.27)	(0.37)
French school (0=no, 1=yes)	0.16	0.15	0.17	-0.02
	(0.36)	(0.02)	(0.02)	(0.02)
Koranic school (0=no, 1=yes)	0.61	0.61	0.60	0.01
	(0.49)	(0.03)	(0.03)	(0.05)
Groundnut harvest (kg)	1487.48	1400.39	1576.32	-175.93
	(2425.96)	(129.70)	(111.87)	(171.54)
Trust	2.69	2.66	2.72	-0.07
	(1.44)	(0.07)	(0.07)	(0.10)
Generosity	1.40	1.42	1.37	0.05
	(0.61)	(0.03)	(0.03)	(0.04)
Risk aversion	3.10	3.14	3.07	0.07
	(1.45)	(0.07)	(0.07)	(0.10)
Patience	1.53	1.58	1.47	0.11
	(1.75)	(0.09)	(0.09)	(0.12)
Number of observations	839	429	410	839

** Significantly different from 0 at the 5% level based on two-sided t-test. Trust is a survey-based measure asking about trust towards a random group member; generosity is based on a hypothetical dictator game; risk aversion is based on a hypothetical Binswanger-style (1980) lottery choice; and patience is based on a hypothetical multiple price list between 100,000 CFA tomorrow and an equal or higher (increasing) amount three months from today.

3. Results

3.1. BCG: Coordination without communication (H1 through H4)

We first investigate the extent of coordination success or failure in the absence of pre-play communication, restraining the sample to those 28 sessions where no intentions were revealed. Overall, groups were able to achieve the coordination threshold in 34% of the cases. This statistic in itself is not informative: one would have likely found different results with different initial endowments and different threshold levels. It is more interesting to investigate the treatment effects (N, T, H, and Uncertainty) given individual- and round/session-level determinants of investments through the group. Table 5 shows the estimates according to specification (1).

Table 5. BCG: coordination without communication
Dependent variable: Number of chips sent to the group

	(1)	(2)	(3)	(4)
Size (*N*)	-0.408	-0.412	-0.410	-0.848
	(0.342)	(0.262)	(0.263)	(0.363)**
Threshold (*T*)	0.000	-0.001	-0.001	0.009
	(0.007)	(0.006)	(0.006)	(0.003)***
Premium (*H*)	0.156	0.152	0.147	0.192
	(0.070)**	(0.070)**	(0.066)**	(0.079)**
Uncertainty	0.117	0.007	0.007	0.001
	(0.300)	(0.233)	(0.233)	(0.331)
Age		0.010	0.010	0.010
		(0.004)**	(0.004)**	(0.006)
Female		0.011	0.011	-0.139
		(0.215)	(0.214)	(0.291)
Land size		0.028	0.028	0.033
		(0.014)*	(0.014)*	(0.017)*
Schooling		-0.068	-0.068	-0.334
		(0.245)	(0.246)	(0.277)
Trust		0.123	0.123	0.120
		(0.194)	(0.195)	(0.227)
Generosity		0.272	0.271	0.303
		(0.051)***	(0.051)***	(0.068)***
Risk aversion		-0.084	-0.084	-0.115
		(0.046)*	(0.046)*	(0.057)*
Patience		0.054	0.054	0.019
		(0.042)	(0.042)	(0.054)
Round dummies	No	No	Yes	Yes
R^2	0.01	0.09	0.09	0.10
N	1,716	1,712	1,712	1,072

* *p*<0.1; ** *p*<0.05; *** *p*<0.01. Robust standard errors clustered at session-level in parentheses. Trust, generosity, risk aversion, and patience as defined in previous table.

In columns (1) to (3), we introduce individual- and round/session-level effects stepwise in order to assess the robustness of the treatment estimates. Introducing such effects does not impact the point estimates of the treatment estimates. Although imprecisely estimated, we find a large point estimate associated with the group-size parameter (N). Individuals in groups of size 20 tend to invest 0.4 chips less through the group – 800 CFA or 1.6 USD – than their counterparts in groups of 10. We also find that changing the level of the premium (H) from 2500 to 3000 CFA per chip invested in the group is associated with an increase of 0.15 chips played through the group – approximately, 60 cents of a dollar. The economic and statistical significance of the threshold (T) and uncertainty treatments, in comparison, are limited.

In column (2), we control for individual characteristics measured before the game started. Results indicate that age and land size contribute to explaining player behavior, while schooling and gender do not. Turning to attitudinal measures, we find generosity and risk aversion to clearly correlate with higher investment through the group. As one would expect, higher risk aversion is associated with lower investment in the uncertain strategy (investing through the group). We also find that generosity correlates positively with investment through the group; suggesting that individuals perceive group investments as a collective endeavor above and beyond the individual gains they receive from it. In column (3), we control for round dummies. While players were not provided with their actual gains after each round (only at the end of the entire session was one round selected for payment), there can always be some form of (fictitious) learning (updating) across rounds. Controlling for rounds does not significantly affect the results obtained in columns (1) and (2).

In column (4) we further investigate the effect of the group-size treatment on individual decisions. As described in Section 2.3, groups of 20 individuals were either exposed to lower (40 or 50 chips) or higher (80 or 100) threshold ranges, while smaller groups were only exposed to lower ones (40 or 50). Therefore, two opposite effects may give rise to the results: (i) coordination may be more difficult in larger groups, and (ii) the level of individual effort needed to reach a low threshold is lower in larger groups. To distinguish between these two effects, we restrict the sample to include, among the larger groups, only those exposed to high threshold ranges. This way, we are able to assess the effect of group size for the same level of individual effort – the contribution necessary from each individual to reach the threshold is now 4 or 5 chips per individual regardless of the group size. The results are rather clear: for the same level of effort required to reach the threshold, individuals in larger groups of size 20 tend to contribute 0.8 chips less (that is 1600 CFA or 3.2 USD) through the group than their counterparts in smaller groups of size 10.

Overall, results from the BCG point towards a mixed story. The premium level (H), the group size (N, holding the level of individual effort constant), and risk aversion are strong drivers of coordination. These suggest that individuals do react to variation in the expected benefits from coordinating with others and the corresponding likelihood of success. Yet, we also find evidence of a generosity-driven investment through the group, suggesting a type of norm of group cooperation that we will further investigate below.

3.2. CCG: Coordination with communication (H5 through H8)

Table 6 shows the main estimation results for specification (2). Results indicate a clear effect of pre-play communication, leading to 0.4 additional chips invested through the group (800 CFA or 1.6 USD) as compared to sessions without pre-play communication. Thus, communication reduces coordination failure (H5), confirming what several conventional lab experiments have found (see previously mentioned references; in particular, those reviewed by Crawford 1998 and Ganguly and Ray 2010).[22]

Table 6. Main regression estimates testing H5 through H8
Dependent variable: Number of chips sent to the group

	(1)	(2)	(3)
C	0.401	-0.373	-0.778
	(0.191)**	(0.678)	(0.632)
N	0.015	-0.382	-0.844
	(0.231)	(0.259)	(0.338)**
T	-0.005	-0.001	0.008
	(0.004)	(0.005)	(0.003)***
H	0.148	0.156	0.196
	(0.050)***	(0.068)**	(0.084)**
Uncertainty	-0.119	-0.003	0.012
	(0.187)	(0.231)	(0.328)
$C * N$		0.789	1.687
		(0.438)*	(0.592)***
$C * T$		-0.007	-0.026
		(0.009)	(0.009)***
$C * H$		-0.016	-0.009
		(0.098)	(0.135)
C *Uncertainty		-0.188	0.414
		(0.366)	(0.418)
R^2	0.10	0.11	0.10
N	3,312	3,312	2,112

* $p<0.1$; ** $p<0.05$; *** $p<0.01$. Controls for age, gender, land-size, schooling, trust, generosity, risk aversion and patience included in all specifications, along with round-level fixed effects. Robust standard errors clustered at session-level in parentheses.

Further, results presented in column (1) broadly confirm those obtained in column (3) of Table 5. We find little effect of threshold (T) or uncertainty on one's

[22] The communication effect still holds if we restrict the data to just the first round.

willingness to invest through the group and the estimate associated with the premium (H) has the same magnitude as before. Together, these results confirm the relative independence of all experimental treatments on individual behavior. However, we do find a drastic reduction in the point estimate for the group size (N). This suggests complementarity between group size and communication.

To test H6 through H8, column (2) adds interaction effects between the communication treatment and the remaining treatments (T, H, Uncertainty, and N). Results show that there are interactions between communication and group size (H6). We do not, however, uncover clear interaction effects between communication and the threshold level as specified in H7 (more on this below). Further, neither uncertainty nor the premium seems to interact with communication (H8).

In column (3), we further investigate the relationship between communication and size, keeping the individual level of effort constant, that is removing from the sample larger groups (size 20) exposed to lower threshold ranges (40 and 50 chips). We find that for a required effort of 4 or 5 chips per individual, communication essentially matters in larger groups leading to an increase in investment through the group of 1.7 chips (3400 CFA or 6.8 USD). These findings suggest that communication is really only working in large groups. There does not appear to be any reaction in small groups (size 10), as suggested by the direct effect of communication, which is no longer significant. The estimates also suggest that communication more than overcomes the coordination constraint faced by larger groups, since the direct effect of size (N) is about half of the interacted effect of size and communication (CCG*N).

Lastly, we do find small but significant evidence that, when facing higher thresholds, communication lowers chips sent to the group (CCG*T; recall H7). Thus, information about others' intentions helps an individual revise her belief about the likelihood of reaching the threshold. In particular, the direction of the effect suggests that when facing a higher threshold, this information may reveal a greater distance to the threshold than when facing a lower threshold. This in turn leads the individual to revise her actual play (chips sent to the group) downward.

Overall, the results suggest that communication helps overcome the constraint of group size. This result carries significant implication for producers' organizations in a day-to-day setting. Typically, a large number of small-scale producers need to aggregate their output in order to reap the benefits of economies of scale. Absent N-way communication, coordination of such large groups may be infeasible, contributing to coordination failure as described in the introduction.

3.3. Mechanisms of communication (H9 and H10)

Lastly, Figures 6 and 7 explore the mechanisms by which communication effects are occurring. These figures present point estimates from conditional regressions as in specification (3).

Consistent with H9, Figure 6 shows that, controlling for distance to the median signal, the closer the aggregate signal is to the threshold, the more likely players are to revise their actions upward from their initial signals. So, indeed, players are using the aggregate signal as a way to assess strategic uncertainty and thus, their likely payoffs. This is consistent with findings in the previous section.

Figure 7, however, shows that, controlling for the distance to the threshold, there is no evidence of players revising their actions toward the median signal sent by the group. So, there seems to be limited evidence of conformity to a norm of coordination, as established by other players' signals.

These effects hold even after controlling for altruistic/other-regarding motives and trust attitudes towards a random group member.

Figure 6: Effect of per-capita distance to threshold on revisions from intentions

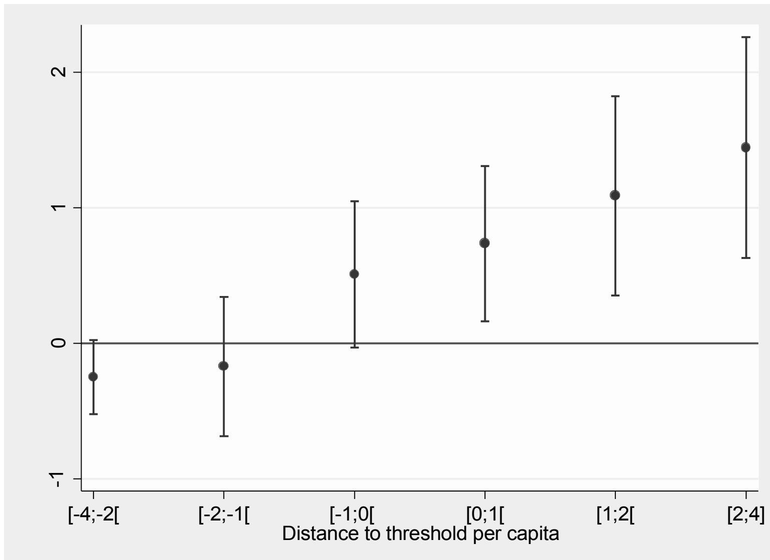
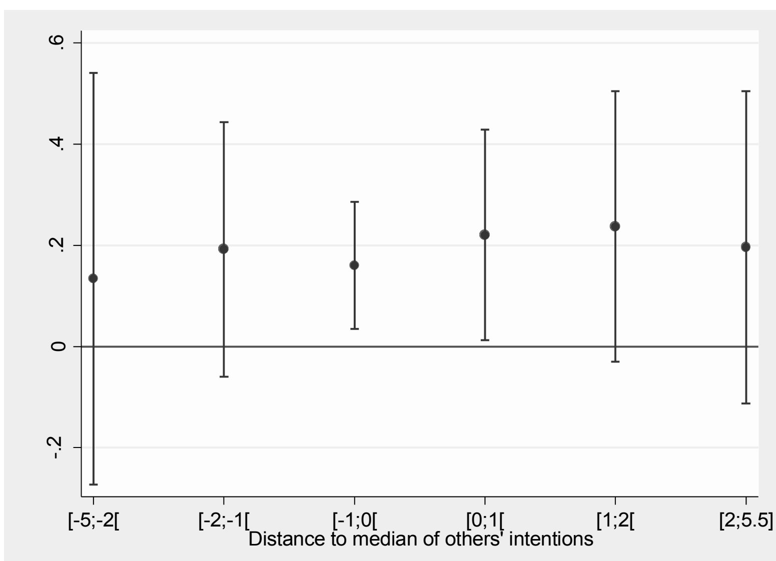


Figure 7: Effect of distance to median on revisions from intentions



4. Conclusion and discussion

Economic growth and development depend on production, which requires coordination. As such, coordination failure is at the heart of certain development (poverty) traps. This has led to a literature on communication (cheap talk) as a potential mechanism for increasing coordination. This paper focuses on a microeconomic developing country field context where strategic uncertainty has historically led to coordination failure – farmer groups in Senegal that seek to sell agricultural production collectively. We explore the role of communication in high-stakes, neutrally framed, stag-hunt coordination games with randomly selected members of those farmer groups.

Like previous lab studies, we find that communication significantly reduces coordination failure through reduced strategic uncertainty (as theory would predict). However, this finding is context-specific. Using treatment variation, we find that (1) large groups have greater difficulty coordinating on "good" equilibria and (2) communication helps them overcome this constraint. In fact, communication only works in large groups.

These findings have an important policy implication: While collective action may generate the type of economies of scale needed for small-scale farmers to access more profitable markets, seizing such market opportunities also requires coordination by larger groups (that is, groups with more "small" members). According to our findings, it is exactly larger groups that have difficulty coordinating, making their members more susceptible to poverty-driven coordination traps. Since communication works in larger groups, our results imply that communication has an impact exactly where it is needed—in larger groups that potentially comprise the smallest-scale farmers.

From a policy standpoint, communication is a desirable mechanism for reducing coordination failure since it is relatively non-costly to implement. After all, people communicate in free form all the time. However, if so, why would we find an effect of communication? Two main features distinguish this communication institution from everyday "talk". First, communication is highly structured and the information content is particularly salient. Second, communication is N-way and simultaneous as opposed to bilaterally simultaneous (like typical communication). As such, farmers have an indication of what the whole group intends to do.

Despite its potential to inform policy, to the best of our knowledge, there is limited evidence on the impact of communication on coordination in the field, particularly when the setting has historically been conducive to coordination failure. We thus designed these artefactual field experiments in order to assess whether structured, N-way communication has the potential to improve coordination in such contexts. Despite their artefactual nature, these experiments also shed light on what a naturally occurring communication institution could look like. In fact, as a follow-up to these experiments, we designed natural field experiments that implemented communication in a similar manner: A farmer group meeting is called in which intended actions (agricultural production intended for the group) are revealed to group members on a white board at the front of the meeting room. The experimental variation arises from the amount of information that is revealed to group members. In a subsequent paper, we will use the findings from the artefactual experiments to predict or further explain those from the natural field experiments, similarly to Barr et al. (2014) and de Arcangelis et al. (2015) among others.

Acknowledgments

We would like to thank participants at the 2015 Allied Social Science Association/National Economic Association Meetings, CERDI, Duke University, the 2013 Economic Science Association North American Meetings, the Experimental Methods in Policy Conference on Curaçao, Georg-August-University of Goettingen, the International Food Policy Research Institute, the 6th

Annual Morehouse College Research Colloquium, the 2013 Symposium on Economic Experiments in Developing Countries, Université Bordeaux, and the 2014 Workshop on Producers' Organizations in Agricultural Markets at Toulouse School of Economics for helpful comments. We also gratefully acknowledge financial support from the CGIAR Research Program on Policies, Institutions, and Markets and the IFPRI Mobile Experimental Economics Laboratory (IMEEL).

Appendix: Proofs

H1: Using the Implicit Function Theorem (IFT), we have that $\frac{\partial A_i}{\partial N} = -(\partial G/\partial N/\partial G/\partial A_i) \Rightarrow \frac{\partial A_i}{\partial N} = -\left(\left(\frac{\partial P}{\partial N} + A_i \frac{\partial^2 P}{\partial A_i \partial N}\right) \Big/ \left(2\frac{\partial P}{\partial A_i} + A_i \frac{\partial^2 P}{\partial A_i^2}\right)\right) = - \cdot +/+ < 0$, depending on the properties of P and the location of A_i.

H2: Similarly to H1, we have that $\frac{\partial A_i}{\partial T} = -\left(\left(\frac{\partial P}{\partial T} + A_i \frac{\partial^2 P}{\partial A_i \partial T}\right) \Big/ \left(2\frac{\partial P}{\partial A_i} + A_i \frac{\partial^2 P}{\partial A_i^2}\right)\right) = - \cdot -/+ > 0$, depending on the properties of P and the location of A_i.

H3: Similarly to above we have that $\frac{\partial A_i}{\partial H} =$
$-\left(\left(\frac{\partial P}{\partial H} + A_i \frac{\partial^2 P}{\partial A_i \partial H}\right)\big(U(H) - U(L)\big) + \left(P + A_i \frac{\partial P}{\partial A_i}\right) U' \Big/ \left(2\frac{\partial P}{\partial A_i} + A_i \frac{\partial^2 P}{\partial A_i^2}\right)\right) = - \cdot -/+ > 0$, depending on the properties of P, the location of A_i, and how it changes relative to A_{-i}.

H4: Since, in expectation, external uncertainty can be seen as a reduction in the premium H, the proof for H3 also shows that an increase in external uncertainty (decrease in H) leads to a decrease in A_i.

H5: Similarly to H1-H4, we have that $\frac{\partial A_i}{\partial S_{-i}} = -\left(\left(\frac{\partial P}{\partial S_{-i}} + A_i \frac{\partial^2 P}{\partial A_i \partial S_{-i}}\right) \Big/ \left(2\frac{\partial P}{\partial A_i} + A_i \frac{\partial^2 P}{\partial A_i^2}\right)\right) \neq 0$. The direction of this effect depends on the proximity between $\Sigma_{-i}^{N} S_{-i}$ and $T - S_i$.

H6: Using the expression for $\frac{\partial A_i}{\partial S_{-i}}$, we have $\frac{\partial^2 A_i}{\partial S_{-i} \partial N} \neq 0$, depending on the properties of $P(.)$ and the location of S_{-i} and A_i.

H7: Similarly to H6, we have $\frac{\partial^2 A_i}{\partial S_{-i} \partial T} \neq 0$, depending on the properties of $P(.)$ and the location of S_{-i} and A_i.

H8: Similarly to the above, we have $\frac{\partial^2 A_i}{\partial S_{-i} \partial H} \neq 0$, depending on the properties of $P(.)$ and the location of S_{-i} and A_i.

H9: The proof follows from H5 based on the properties of $P(.)$.

H10: The proof follows from H5 and H9 based on the properties of $P(.)$ and by looking at the comparative static with regard to the median signal (relative to i's action) as opposed to sum of all other signals.

References

1. de Arcangelis, G., M. Joxhe, E. Tiongson, and D. Yang. 2015. Directing remittances to education with soft and hard commitments: evidence from a lab-in-the-field experiment and new product take-up among Filipino migrants in Rome. *Journal of Economic Behavior and Organization* 111, 197-208.
2. Agranov, M., and L. Yariv. 2015. Collusion through Communication in Auctions. Working Paper.
3. Aguiar, M., M. Amador, E. Farhi, and G. Gopinath. 2015. Coordination and Crisis in Monetary Unions. Working Paper.
4. Aldana, M., J. Ashby, G. Burpee, S. Ferris, G. Heinrich, C. Quiros, T. Remington, and K. Wilson. 2007. The Organization and Development of Farmer Groups for Agroenterprise: Conclusions from a CRS & RII-CIAT Study Tour in Asia, Africa and Latin America. CRS Internal Working Paper.

5. Alzua, M. L., J. C. Cardenas, and H. Djebbari. 2014. Community mobilization around social dilemmas: Evidence from lab experiments in rural Mali. Working Paper.
6. Ashraf, N., D. S. Karlan, and X. Giné. 2009. Finding Missing Markets (and a disturbing epilogue): Evidence from an Export Crop Adoption and Marketing Intervention in Kenya. *American Journal of Agricultural Economics* 91 (4), 973-990.
7. Barr, A., T. Packard, and D. Serra. 2014. Participatory accountability and collective action: Experimental evidence from Albania. *European Economic Review* 68, 250-269
8. Battaglini, M., and R. Bénabou. 2003. Trust, Coordination, and the Industrial Organization of Political Activism. *Journal of the European Economic Association* 1 (4), 851-889.
9. Battalio, R., L. Samuelson, and J. Van Huyck. 2001. Optimization Incentives and Coordination Failure in Laboratory Stag Hunt Games. *Econometrica* 69 (3), 749-764.
10. Battigalli, P., and M. Dufwenberg. 2009. Dynamic Psychological Games. *Journal of Economic Theory* 144, 1-35.
11. Ben-Porath, E. 2003. Cheap Talk in Games with Incomplete Information. *Journal of Economic Theory* 108 (1), 45-71
12. Bernard, T., A. De Janvry, and E. Sadoulet. 2010. When Does Community Conservatism Constrain Village Organizations? *Economic Development and Cultural Change* 58 (4), 609-641.
13. Bernard, T., and A. Seyoum Taffesse. 2012. Returns to Scope? Smallholders' Commercialisation through Multipurpose Cooperatives in Ethiopia. *Journal of African Economies* 21 (3), 440-464.

14. Bernard, T., L. Sène, A. C. G. Viceisza, and F. Wouterse. 2014. Coordination requires more than one leader! Experimental Evidence from Rural Producer Organizations in Senegal. Working paper.
15. Bernheim, B. D. 1994. A theory of conformity. *Journal of Political Economy* 102 (5), 841-877.
16. Binswanger, H. P. 1980. Attitudes Toward Risk, Experimental Measurement in Rural India. *American Journal of Agricultural Economics* 62 (August), 395-407.
17. Bryant, J. 1983. A Simple Rational Expectations Keynes-Type Model. *The Quarterly Journal of Economics* 98 (3), 525-528.
18. Charness, G., and B. Grosskopf. 2004. What makes cheap talk effective? Experimental evidence. *Economics Letters* 83, 383-389.
19. Charness, G., and M. Dufwenberg. 2006. Promises and Partnerships. *Econometrica* 74 (6), 1579-1601.
20. Charness, G., and M. Dufwenberg. 2010. Bare promises. *Economics Letters* 107, 281-283.
21. Charness, G., and M. Dufwenberg. 2011. Participation. *American Economic Review* 101, 1211-1237.
22. Chen, Y., N. Kartik, and J. Sobel. 2008. Selecting Cheap-talk Equilibria. *Econometrica* 76 (1), 117-136.
23. Cooper, R., D. V. DeJong, R. Forsythe, and T. W. Ross. 1992. Communication in Coordination Games. *Quarterly Journal of Economics* 107 (2), 731-771.
24. Crawford, V. P. 1991. An "Evolutionary" Interpretation of Van Huyck, Battalio, and Beil's Experimental Results on Coordination. *Games and Economic Behavior* 3, 25-59.
25. Crawford, V. P. 1998. A Survey of Experiments on Communication via Cheap Talk. *Journal of Economic Theory* 78, 286-298.

26. Crawford, V. P., and J. Sobel. 1982. Strategic Information Transmission. Econometrica 50 (6), 1431-1451.
27. Diamond, P. A. 1982. Aggregate Demand Management in Search Equilibrium. *Journal of Political Economy* 90 (5), 881-894.
28. Fafchamps, M., and R. V. Hill. 2005. Selling at the Farmgate or Traveling to Market. *American Journal of Agricultural Economics* 87 (3), 717-734.
29. Farrell, J. 1987. Cheap talk, coordination, and entry. *Rand Journal of Economics* 18, 34-39.
30. Farrell, J. 1988. Communication, coordination and Nash equilibrium. *Economics Letters* 27, 209-214; see also “Erratum,” 33, 299.
31. Farrell, J., and M. Rabin. 1996. Cheap Talk. *Journal of Economic Perspectives* 10 (3), 103-118.
32. Feltovich, N., and P. J. Grossman. 2015. How does the effect of communication vary with group size? Experimental evidence from a threshold public-good game. Working Paper.
33. Gaechter, S., D. Nosenzo, E. Renner, and M. Sefton. 2010. Sequential versus Simultaneous Contributions to Public Goods: Experimental Evidence. *Journal of Public Economics* 94 (7-8), 515-522.
34. Ganguly, C., and I. Ray. 2010. Cheap Talk: Basic Models and New Developments. In Dhar, A. (ed.), *Some Contemporary Issues on Development and Growth Economics*, 383-416, India: Allied Publishers.
35. Geanakoplos, J., D. Pearce, and E. Stacchetti. 1989. Psychological Games and Sequential Rationality. *Games and Economic Behavior* 1, 60-79.
36. Gneezy, U., B. Rockenbach, and M. Serra-Garcia. 2013. Measuring Lying Aversion. *Journal of Economic Behavior and Organization* 93, 293-300.
37. Heinemann, H., R. Nagel, and P. Ockenfels. 2009. Measuring Strategic Uncertainty in Coordination Games. *The Review of Economic Studies* 76 (1), 181-221.

38. Hellin, J., M. Lundy, and M. Meijer. 2007. Farmer Organization, Collective Action and Market Access in Meso-America. CAPRi Working Paper No. 67.
39. Hill, R. V., E. Maruyama, and A. Viceisza. 2012. Breaking the norm: An empirical investigation into the unraveling of good behavior. *Journal of Development Economics* 99 (1), 150-162.
40. Kartik, N. 2009. Strategic Communication with Lying Costs. *Review of Economic Studies* 76 (4), 1359-1395.
41. Key, N., E. Sadoulet, and A. De Janvry. 2000. Transactions costs and agricultural household supply response. *American Journal of Agricultural Economics* 82 (2), 245-259.
42. Levy, G., and R. Razin. 2007. On the Limits of Communication in Multidimensional Cheap Talk: A Comment. *Econometrica* 75 (3): 885-893.
43. Manski, C. 2000. Economic Analysis of Social Interactions. *Journal of Economic Perspectives* 14 (3), 115-136.
44. Ostrom, E. 2010. Beyond markets and states: polycentric governance of complex economic systems. *American Economic Review* 100 (3), 641-672.
45. Ragasa, C., and J. Golan. 2014. The role of rural producer organizations for agricultural service provision in fragile states. *Agricultural Economics* 45 (5), 537-553.
46. Rosenstein-Rodan, P. N. 1943. Problems of Industrialisation of Eastern and South-Eastern Europe. *The Economic Journal* 53 (210/211), 202-211.
47. Rousseau, J.-J. 1973. A Discourse on the Origin of Inequality. In *The Social Contract* (translated by G. D. H. Cole), 27-113. London, UK: Dent.
48. Sobel, J. 2005. Interdependent Preferences and Reciprocity. *Journal of Economic Literature* 43 (2), 392-436.
49. Van Huyck, J. B., R. C. Battalio, and R. O. Beil. 1990. Tacit coordination games, strategic uncertainty, and coordination failure. *The American Economic Review* 80(1), 234–48.

50. Vesterlund, L. 2003. The Informational Value of Sequential Fundraising. *Journal of Public Economics* 87 (3), 627-657.
51. Wydick, B. 2007. *Games in Economic Development*. Cambridge, UK: Cambridge University Press.