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Communication and coordination: Experimental evidence from farmer groups in Senegal

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Abstract

Coordination failure has been argued to be at the heart of development (poverty) traps. Communication has been proposed as a mechanism for reducing coordination failure. This paper reports artefactual field experiments that test the impact of communication on coordination using members of farmer groups in rural Senegal, a context where failure to coordinate on collective selling of agricultural production is common. In our baseline treatment, farmers played neutrally framed, high-stakes coordination games in randomly formed experimental groups of size N equal to 10 or 20, wherein all players in a given session belong to the same farmer group. In our communication treatment, a subset of these groups was exposed to N-way preplay communication in which farmers were able to signal their intended action. We find that communication significantly reduces coordination failure. Using treatment variation and additional survey data, we explore the mechanisms underlying this effect. (1) Communication only increases coordination in larger groups. (2) Communication increases (reduces) coordination due to reduced (increased) strategic uncertainty surrounding other players' actions. (3) By revealing information about other players' actions, communication establishes a norm of "equitable coordination". Ours is one of few studies that experimentally assess the impact of communication on coordination in a context where coordination failure has been prevalent. Our experiments were designed as a precursor to a naturally occurring communication institution, implemented as a natural field experiment. We thus use the findings to predict the potential effects of such an institution.

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

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

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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 (economic agents) 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. Similarly, Diamond (1982), Bryant (1983), van Huyck et al. (1990), and other references discuss contexts in which the returns to a player from undertaking a production opportunity are an increasing function of the number of others who have chosen to produce.

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. Consider the two-player coordination game in Figure 1 owing to Cooper et al. (1992, page 741).[5] 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 coordination failure'*. This has led to a related literature on communication (cheap talk) as a potential mechanism for increasing coordination; however, most of this literature is theoretical and/or based on conventional laboratory experiments[6] (see Crawford and Sobel 1982; Farrell 1987, 1988; Cooper et al. 1992; Farrell and Rabin 1996; Crawford 1998; Charness and Grosskopf 2004; Charness and Dufwenberg 2006, 2010, 2011; and Ganguly and Ray 2010).

In this paper, we focus on a microeconomic developing country field context, where strategic uncertainty has historically led to coordination failure. We study farmer groups (also known as rural producer organizations, RPOs) in Senegal that seek to sell their members' agricultural production (groundnuts/peanuts) collectively. Coordination and strategic uncertainty are relevant in the following way. Each individual farmer produces a relatively small quantity and accordingly, it tends to be economically inefficient (or even infeasible) for her/him to access larger, more profitable markets. By aggregating production across farmers, a group can behave as if it were a large seller, enter into contracts with a large buyer, and overcome different types of scale-dependent fixed costs such as transportation and storage. However, like the Big Push, such contracts, require coordination. In particular, an individual farmer's return from selling through the group (that is, committing to a large contract, also known as strategy 2 in Figure 1) is strategically uncertain, since it depends on the number of other members who also choose to do so.

[5] As Crawford (1991) highlights, the basic structure of this game is similar to that of the "stag hunt" parable discussed by Rousseau (1973).
[6] We stick to the terminology of Harrison and List (2004).

Previous survey and experiment data (in particular, 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.[7] 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 lower, but more certain payoff.[8] So, here too the key question is 'how to mitigate coordination failure'. Given the vast theoretical and lab evidence on communication as a way to reduce coordination failure and the limited field experimental evidence, this study seeks to formally test for the relevance of N-way pre-play communication to foster cooperation within existing farmer groups.

Specifically, this paper reports artefactual field experiments conducted with randomly selected members of the above referenced farmer groups to explore the role of communication on coordination. 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. In other words, each farmer gets to signal her/his intended action/strategy to the group.

From a policy standpoint, N-way communication is a desirable mechanism for reducing coordination failure since it is relatively non-costly to implement—after all, people "communicate" in unstructured ways (free form) every day and may only like a means to centralize information in order to fully facilitate coordination. Further, from a policy standpoint, communication is a desirable mechanism for reducing coordination failure since it is relatively non-costly to implement. Further, from a policy standpoint more closely related to our context, our results illustrate the following paradoxical situation. Collective action may generate the type of economies of scale needed for smallholders to access remunerative output markets. Yet, from the sheer limited size of their individual production, seizing these market opportunities require that a large number of small family farms are able to coordinate. Coordination in larger group is here shown to be more difficult and can lead to a poverty-driven coordination trap. N-way communication may contribute to overcome these constraints.

Yet, to the best of our knowledge, we are one of the first to study coordination and communication in a field-lab context, particularly when it has the potential to immediately inform policy.[9] So, our study makes at least two contributions to the literature. First, these artefactual field experiments were designed with subsequent natural field experiments (randomized controlled trials) in mind. We thus use the findings to predict the potential effects and mechanisms of a real-life communication institution.[10]. Second, by revisiting certain claims made by conventional lab experiments, this study sheds light on whether the effect of communication also holds when conducted with a non-student subject pool, particularly one that has faced coordination failure as part of its day-to-day environment.

Like previous lab studies, we find that communication significantly reduces coordination failure. However, this finding seems to be context-specific. Using treatment variation and additional survey data, we find that communication only increases coordination when in larger groups. We also find that communication works through two mechanisms. First, it increases (decreases) coordination due to reduced (increased) uncertainty

[7] Similar evidence is found in Uganda (Fafchamps and Hill 2005), in Burkina Faso (Bernard et al. 2010), in Ethiopia (Bernard and Taffesse, 2012), or in the Democratic Republic of Congo (Ragasa and Golan, 2013).

[8] This is likely reinforced by the fact that RPOs seldom enforce any sanctions against members who engage in side-selling. In effect, no formal contract is established that could lead to appeal to the court of law. Further, as they are located within villages with dense family and kin ties, these organizations are rarely ever able to exclude anyone from continued membership even when members have not fulfilled their obligations.

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

[10] Similarly, De Arcangelis et al. (2014) use artefactual field experiments to predict take-up of remittance products.

surrounding other players' actions. Second, by revealing information about others' actions, it establishes a norm of "equitable coordination".

The remainder of the paper is organized as follows. Section 2 constructs a simple framework and reviews existing theoretical and lab experimental evidence to develop testable hypotheses. It also discusses the experimental design, protocol, and empirical strategy in greater detail. 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 organization. We also conducted supplementary treatments that varied some of the parameters in the BCG and the CCG (more on these treatments further below).

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, aka strategy 2 in Figure 1) and keep the remainder $E - A_i$ for herself (the equivalent of selling individually, aka 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 $L < M < H$.

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. In order to think of this as a two-player game, we represent all other players by one other player $-i$ that represents the average move made by all other players. p then represents the probability that player $-i$ contributes $A_{-i} \geq (T - A_i)/(N-1)$.

The Nash equilibrium of this game requires that:

- Player i maximize $E\Pi_i$, leading to:
$$\frac{\partial E\Pi_i}{\partial A_i} = 0 \Rightarrow pU(H) + (1-p)U(L) - U(M) = 0 \Rightarrow p = \frac{U(M) - U(L)}{U(H) - U(L)}$$
- Player $-i$ maximize $E\Pi_{-i}$, leading to:
$$\frac{\partial E\Pi_{-i}}{\partial A_{-i}} = 0 \Rightarrow qH + (1-q)L - M = 0 \Rightarrow q = \frac{U(M) - U(L)}{U(H) - U(L)}$$
where q is the probability that player i sends at least A_i to the N-player pool.

Thus, whether or not the group achieves coordination will depend on player i's belief about p and player $-i$'s belief about q. In other words, if a given player believes that the other players in her group will send enough to reach or surpass the threshold (and also believes that all these other players believe the same), she will also send to the group. Otherwise, she will not.

The beliefs p and q depend on three main aspects: (1) the payoff trade-off between sending to the group, which pays H or L depending on other players' actions (strategic uncertainty), and keeping individually, which pays M regardless of other players' actions; (2) the properties of the utility function (in the analysis, we will control for risk, time, and social preferences as well as other characteristics); and (3) as discussed by Bernard et al. (2014), the cumulative binomial distribution, given each player can be assumed to send $A_{-i} \geq (T - A_i)/(N-1)$ to the N-player pool with a given probability.

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 $S_i \in \{0,1,\ldots,E\}$ of how much she plans to send to the N-player pool.[11] 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, where the latter comprises (a) one's own signal S_i sent to the other players and (b) all other players' signals S_{-i} sent to her.

In the BCG, players' actions (A_i) are driven by their beliefs about other players' actions (q) and hence, the likelihood that the N-player pool will surpass the threshold (T). As derived by Bernard et al. (2014), (perceived) increases (decreases) in other players' contributions to the pool increase (decrease) a given player's likelihood of coordinating p. In the CCG, this same logic applies. Communication in the form of signals will impact players' beliefs about others' contributions and thus their actions.

H1: Players' actions are impacted by the presence of N-way communication.

We also hypothesize that the effect of communication depends on the other experimental variations discussed previously. First, using the properties of a binomial distribution, one can show that as N increases, the likelihood of surpassing T decreases ceteris paribus. In other words, there is greater strategic uncertainty in larger player pools. So we expect that any effect of communication will vary with the size of the player pool, since communication is more informative in larger groups.

[11] Previously, we stated that in the day-to-day environment people "communicate" all the time. If this is the case, we may not expect a communication institution like the CCG to have an effect, if implemented in the naturally occurring environment. However, two main features distinguish this institution from everyday "talk". First, communication is structured and as such, the information content is particularly salient. Second, communication is N-way as opposed to bilateral (like typical communication) and as such, all players have an aggregate signal of what others in the group intend to do. We did not implement a bilateral (or 2-way) preplay communication treatment, since we thought that N-way communication would be more useful to these groups if implemented as a naturally occurring institution; in particular, the CCG was consistent with our natural field experiments. In fact, anecdotal evidence suggests that certain cooperatives in Mexico have implemented such an institution by maintaining a board where member farmers can reveal their intended sale through the group.

H2: Players' actions are impacted differentially by the presence of N-way communication in groups of different sizes. In particular, communication will have a stronger effect in larger groups.

Second, we also anticipate that the former effects interact with the threshold (T). Again, ceteris paribus, there is less strategic uncertainty as to whether larger groups can achieve a threshold. In other words, while communication is more informative in larger groups, it is less informative when facing lower thresholds, particularly relative to the size of the pool.

H3: Players' actions are impacted differentially by the presence of N-way communication in groups of different sizes when facing different thresholds. In particular, communication will have a stronger effect in larger groups, but only when facing higher thresholds.

Third, we expect that an increase in the premium H for achieving the threshold is likely to reduce strategic uncertainty with regard to achieving the threshold. So, communication will matter less when H is high and matter more when there is external uncertainty (an exogenous likelihood that the premium is low) with regard to achieving this premium.

H4: Players' actions are impacted differentially by the presence of N-way communication when the premium is low or uncertain. In particular, communication will have a stronger effect when the premium is low or uncertain.

The above comparative statics, which are associated with the main experimental treatments, 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. Some reviews on communication and cheap talk were cited previously (see by 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).

First, as reviewed by Rabin and Farrell (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—in our context—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, $\{S_1, \dots, S_i, \dots, S_N\}$. 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—the player should keep her whole endowment individually. This intuition leads to our fifth hypothesis, which focuses on *strategic uncertainty and reassurance*.

H5: Players' actions are 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.*

As the previous discussion suggests, 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 the role of beliefs. When discussing the results, we also explore robustness of our treatment effects with regard to survey proxies for pre-existing beliefs 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 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 S 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. Under the assumption that S_{-i} was a good approximation for A_{-i}, we have:

H6: Players' actions are impacted by N-way communication in the following way: A_i may approximate the median of others' signals S_{-i}.[12]

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 pool. 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 pool. In fact, if she truly believes that a high signal will cause other players to increase their contributions to the N-player pool and the pool to surpass T, she should actually send all her endowment to the pool. 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 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 (revisions from signals), thus controlling for any such confounds.

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

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 decision-making with no feedback, followed by debriefing.[13]
5) A post questionnaire collecting additional basic 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, players' actions and payoffs were explained using several visual aids at the front of the room (see Figure 2 for a sample session). The visual aids were introduced systematically as the instructions moved along and varied depending on the treatment protocol under consideration. Here, we start with an explanation of the BCG protocol. Then, we elaborate on how the CCG and other treatment protocols differed.

Figure 2: Sample session

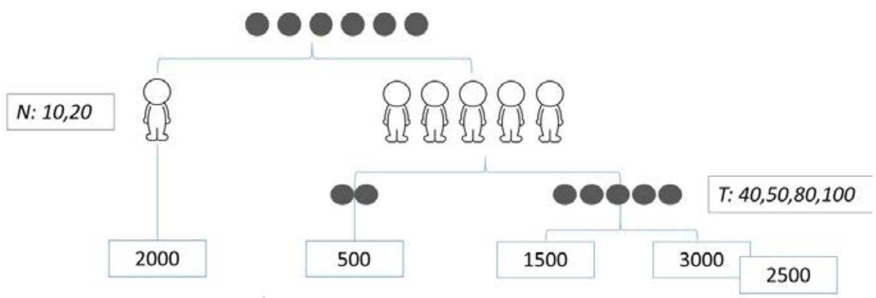
In the BCG, each player had an endowment E of six chips.[14] Each chip was worth 2000 FCFA (M) if held individually. So, players were explained that at the beginning of the game they held an endowment of 12000

[13] There was one session in which only two of the four rounds could be conducted due to subject time constraints.
[14] We did not vary E across individuals in order to keep the protocol simple. 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

FCFA. To mitigate windfall/house money effects, this endowment was presented as payment for the pre-survey. The payoff for each chip sent to the player pool was dependent on whether or not the threshold (T) was reached/surpassed. If $A \geq T$, each chip was worth 3000 FCFA (H); if not, each chip was worth 500 FCFA (L). So, each player had to decide how many of the six chips to send to the player pool (A_i) and how many to keep individually ($6 - A_i$), as shown in Figure 3.

Two primary aids were used when explaining the game. First, monetary payoffs were explained by displaying actual FCFA bills on a board at the front of the room (see bottom-left picture in Figure 2). 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.

Figure 3: Visual of BCG protocol

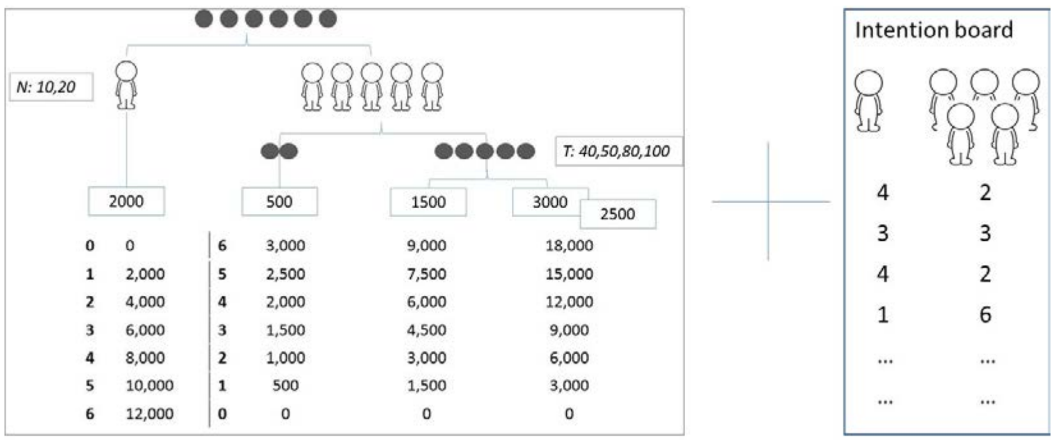


0	0	6	3,000	9,000	18,000
1	2,000	5	2,500	7,500	15,000
2	4,000	4	2,000	6,000	12,000
3	6,000	3	1,500	4,500	9,000
4	8,000	2	1,000	3,000	6,000
5	10,000	1	500	1,500	3,000
6	12,000	0	0	0	0

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 would be random such that other players would not know who sent which signal. It was made clear that this was an intended, but non-binding action. In other words, subjects would be able to change their actual decision once signals were revealed. Figure 5 shows the logic behind the CCG. It was identical to the BCG, except for an additional board, which contained randomly ordered signals. The bottom-right panel of Figure 2 shows an example of filled intention board in one of the sessions.

"endowments" (production quantities) due to issues such as land size, ability, and access to inputs, what primarily matters here is that we varied the group-level thresholds required for coordination.

Figure 4: 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 (within-subjects) would have complicated the experiment protocol. Also, as previously explained and indicated in Figures 3 and 5, we varied the following parameters:[15]

1. The (experimental) group size, N, which was fixed during a session at either 10 or 20. So, N was varied across sessions/between subjects. This is consistent with the naturally occurring environment where farmer groups vary in size.
2. The threshold, T, which was 40 or 50 in 10-person groups and 40, 50, 80, or 100 in 20-person groups. T was varied across rounds. This is consistent with the day-to-day environment in which a minimum quantity is 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 they consider per-capita contributions to the group.
3. The premium, H, which was either 2500 FCFA per chip or 3000 FCFA per chip. H was varied across rounds. This is also consistent with everyday environment where the premium to collective marketing (benefit from a contract) varies with market conditions.
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 (for example, bad weather leading to lower than expected harvests and thus, group-level prices) the premium would be lower than expected at 1500 FCFA per chip (instead of 2500 or 3000). This was varied across rounds. This too is consistent with the day-to-day environment where 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 variation and assess the extent to which the treatments are orthogonal to each other. There is some significant correlation between the N and T; however, as explained previously, this was by design of the experiment (only large groups were exposed to threshold of 80 or 100).

[15] Although the experiment was neutrally framed, in that no real-life example were used to explain variations in these parameters, these variations were chosen with the explicit goal to mimic existing constraints in collective commercialization environments.

Table 1: Distribution of the CCG treatment

	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 other treatments (N, T, H, Uncertainty)

	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 seek to produce groundnuts and sell those collectively (Bernard et al. 2014 discuss additional details and background history surrounding these same farmer groups). From each farmer group, two experimental groups—one of size N=10 and the other of size N=20—were randomly drawn.[16]

These experimental groups were selected from an up-to-date 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 "collective" behavior. 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

[16] Four of the 28 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.

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.

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)
Risk preferences (Binswanger)	3.10	3.14	3.07	0.07
	(1.45)	(0.07)	(0.07)	(0.10)
Time preferences	1.53	1.58	1.47	0.11
	(1.75)	(0.09)	(0.09)	(0.12)
Social preferences (dictator)	1.40	1.42	1.37	0.05
	(0.61)	(0.03)	(0.03)	(0.04)
Trust (survey proxy)	2.69	2.66	2.72	-0.07
	(1.44)	(0.07)	(0.07)	(0.10)
Number of observations	839	429	410	839

** Significantly different from 0 at the 5% level based on two-sided t-test.

2.5. Empirical strategy

Our first set of estimations seeks to test H1 through H4 and takes the following form:

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

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, $A_{\tau ri}$. The independent variables are: C_{τ} which is a dummy indicating whether the session was CCG; $D_{\tau r}$ which is a set of dummies for the other treatment variations (N, T, H, Uncertainty); X which is the set of covariates/characteristics; and R and τ, which stand for round dummies. μ_{τ} is a session-level error term which we account for by clustering standard errors at session level, while $\varepsilon_{\tau ri}$ is an independent, individually specific error term.. In some specifications, we also interact C with the other treatment dummies D.

Our second set of estimations seeks to test H5 and H6 as well as to perform any additional robustness checks. Given this purpose, which is to test the mechanisms by which communication is working, we focus our attention on data from the CCG sessions only. This set of specifications primarily takes the following form:

$$A_{\tau ri} - S_{\tau ri} = \alpha + M'_{\tau ri}\beta + S'_{\tau ri}\lambda + D'_{\tau r}\delta + X'_{\tau ri}\gamma + \rho R_r + \mu_{\tau} + \varepsilon_{\tau ri} \quad (2)$$

The main outcome variable in this specification is the difference between the actual amount a player sent to the group and her signal. The main independent variables are as in the previous specification, except for $M_{\tau ri}$, which represents a proxy for the different mechanisms under consideration. When testing H5, this proxy represents the per-capita distance between the threshold T and the aggregate signals $S = \Sigma_i S_i$. When testing H6, this proxy represents the distance between an individual's signal and the median signal sent by the group.

3. Results

3.1. BCG: coordination without communication

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 successfully the coordination threshold in 34% of the cases. This figure in itself is not informative: one would have likely found different results with different initial endowments and different threshold levels. More interesting is to investigate the individual- and round/session-level determinants of investments through the group, which we propose in Table 5 below. The proposed estimation is that of Equation 1, without the C_τ term to which we'll turn later on.

In columns (1) to (3), we introduce each group of elements sequentially to assess the robustness of the parameter estimates. As is clear from all treatment variables (*T, H, Uncertainty,* and *N*), the introduction of individual-level characteristics or round-level fixed effect does not affect their point estimates – except of that of uncertainty which is not statistically significant in all cases. Accordingly, we find that changing the level of premium from 2500 to 3000 CFA per chip invested in the group is associated with 0.15 additional chip played through the group – that is roughly 60 cents of a dollar. Although imprecisely estimated, we also uncover a large point estimate associated with the size parameter. Individuals in groups of size 20 tend to invest 0.4 chip less through the group – 800 CFA, or 1.6 USD - than their counterparts in groups of 10. The economic and statistical significance of threshold and uncertainty treatments, in comparison, are meaningless.

In column (2) we control for individuals' characteristics measured before the game started. Results indicate that age and land-size contributes to explain players' behavior, although 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 to lower investment in the uncertain strategy (investing through the group). Interestingly, we also find that generosity correlates positively with investment through the group; pointing towards individuals' perception of group investments as a group endeavor above and beyond individual gains from it. In column (3) we control for the round number (from 1 to 4). In effect, 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), one cannot fully ascertain that learning behavior does not occur across rounds. We do not find that controlling for rounds significantly affect the results obtained in columns (1) and (2).

In column (4) we further investigate the effect of group-size treatment onto individuals' decision. 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. Therefore, two opposite effect may blur the results: (i) coordination is often considered more difficult in larger groups, and (ii) the level of individual effort needed to reach low threshold is lower in larger groups. To distinguish between these, we further restrict the sample to only include, amongst the larger groups, those exposed to high threshold ranges. This way, we are able to assess the effect of group size for a similar level of individual effort – the contribution necessary from each individual is now 4 to 5 chips per individual. Results are rather clear, showing that for the same level of effort required to reach the threshold, individuals in larger group tend to contribute 0.8 chips less (that is 1600 CFA or 3.2 USD) through the group than their counterparts in smaller groups.

Overall, results from the Baseline Coordination Game point towards a mixed story. Premium level is a strong driver of coordination, as well as group size for a given level of individual effort and risk aversion. 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 suggestive evidence of a generosity driven investment through the group, suggesting a type of norm of group cooperation that we will further investigate below.

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

	(1)	(2)	(3)	(4)
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)
Size (*N*)	-0.408	-0.412	-0.410	-0.848
	(0.342)	(0.262)	(0.263)	(0.363)**
Age		0.010	0.010	0.010
		(0.004)**	(0.004)**	(0.006)
Sex (1= 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)
Patience		0.054	0.054	0.019
		(0.042)	(0.042)	(0.054)
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)*
Trust		0.123	0.123	0.120
		(0.194)	(0.195)	(0.227)
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 is measured by trust in a random group member, altruism is based on a hypothetical dictator game, risk preferences is based on a hypothetical Binswanger 1980 style lottery choice framework, and time preferences are based on a hypothetical multiple price list between 100,000 FCFA tomorrow and an increasing amount three months from today.

3.2. H1

Table 6 shows the main estimation results for H1 thru H4. In Column (1) we report the estimates from the full specification described in Equation 1. Results indicate a clear effect of pre-play communication, leading to a 0.4 additional chip invested through the group (800 CFA or 1.6 USD) as compared to sessions without pre-play communication. Thus, communication reduces coordination failure (H1), confirming what several conventional lab experiments have found (see previously mentioned references; in particular, those reviewed by Crawford 1998 and Ganguly and Ray 2010).

Further, results presented in column (1) broadly confirm those obtained in Column (3) of Table 5. We find little effect of threshold or uncertainty on one's willingness to invest through the group. We find however an estimated parameter of the same magnitude as before for the effect of premium. Together, these results confirm the relative independence of all experimental treatments on individuals' behavior. However, we do find a drastic reduction in the point estimate previously obtained from the experimental variation related to group-size, suggesting complementarity between the two treatments. We turn to this point below.

Table 6. Main regression estimates testing H1 through H4
Dependent variable: Number of chips sent to the group

	(1)	(2)	(3)
CCG	0.401	-0.373	-0.778
	(0.191)**	(0.678)	(0.632)
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)
N	0.015	-0.382	-0.844
	(0.231)	(0.259)	(0.338)**
CCG*T		-0.007	-0.026
		(0.009)	(0.009)***
CCG*H		-0.016	-0.009
		(0.098)	(0.135)
CCG*N		0.789	1.687
		(0.438)*	(0.592)***
CCG*Unc.		-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, patience, generosity, risk-aversion and trust included, along with round-level fixed effects. Robust standard errors clustered at session-level in parentheses

3.3. H2 thru H4

To test H2 through H4, column (2) adds interaction effects between the communication treatment and the remaining treatments (T, H, Uncertainty, and N) to the specification in column (1). Results show that there are interactions between communication and group size (H3). We do not however uncover clear interaction effects between communication and the threshold level as specified in H2. Further, neither uncertainty nor the premium seems to interact with communication (H4).

As before we further investigate the relationship between communication and size, keeping the individual level of effort constant that is, removing from the sample data concerning groups of large size exposed to lower threshold ranges (40 and 50). Results are presented in Column (3). Accordingly, for a given effort of 4 to 5 chips required per individual, communication essentially matters in larger groups leading to an increase 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 of size 20. There does not appear to be any reaction in small groups of size 10, as suggested by the no-longer significant direct effect of communication. They also suggest that communication more than overcome the constraint faced by larger group, with the direct effect of size being about half of the interacted effect of size and communication.

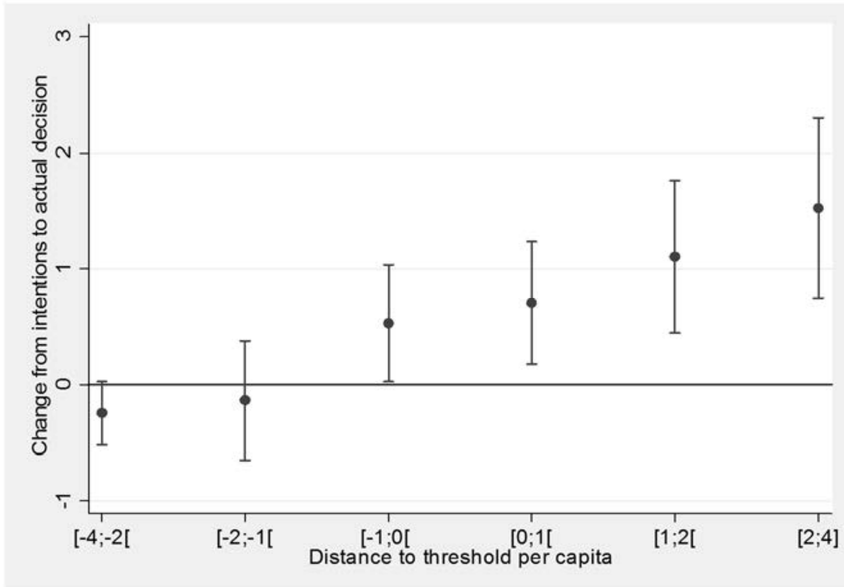
Lastly, in column (3) we also find small but significant evidence that communication lowers group contribution for higher levels of threshold. In effect, having access to information on others' intentions helps one revise her anticipation vis-a-vis the likelihood of success in reaching the threshold. With higher threshold, this information may reveal a greater distance to threshold than with lower threshold, and lead to revise downward his/her actual play.

Overall, results suggest that communication helps overcome the constraint of group-size. This result carry significant implication for producers' organizations in real-life setting. In effect, large number of such small-scale producers need to aggregate their produce to effectively reap the benefits of economies of scale. Absent N-way communication, coordination of such large pools may be infeasible, contributing to the overall coordination failure described in introduction.

3.4. Mechanisms of communication

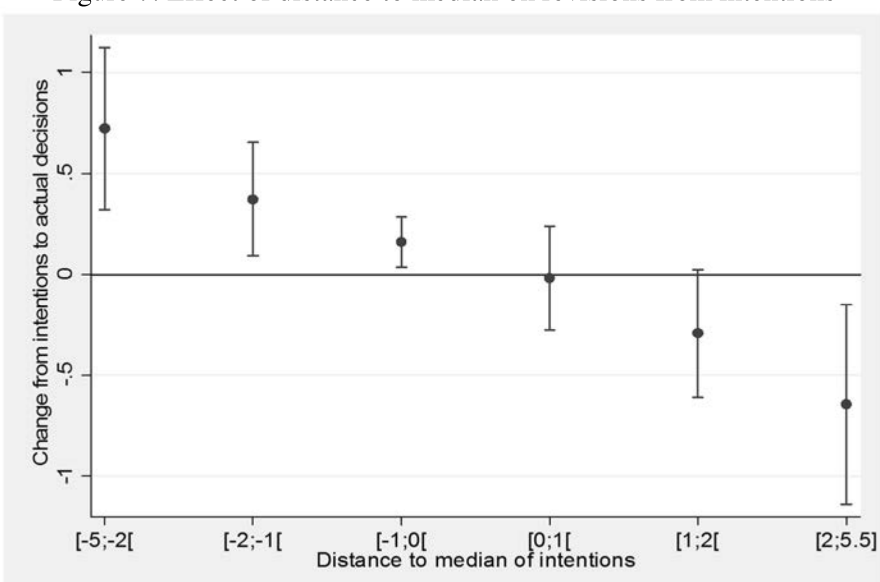
Lastly, Figures 6 and 7 explore the mechanisms by which communication effects are occurring. These figures present point estimates from conditional regressions specified as in equation (2). Consistent with H5, Figure 6 shows that the closer the aggregate signal is to the threshold, the more likely players are to revise their actions upward from their initial signals. So, indeed, players are using the aggregate signal as a way to assess their likely payoffs. This is consistent with the finding in the previous section. So, Figure 6 confirms the mechanism set forth by H5.

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



This said, Figure 7 shows, consistent with H6, that players revise their actions toward the median signal sent by the player pool. So, there is also evidence of conformity to a norm of coordination, as established by other players' signals. This effect holds even after controlling for altruistic/other-regarding motives. While we do not have players' first- and second-order beliefs to explicitly test for mechanisms such as guilt aversion (see for example Charness and Dufwenberg 2006, 2010), we also note that this effect persists even after controlling for players' trust attitudes towards a random group member. Indeed, this effect can be consistent with belief-based models of communication and promises.

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



4. Conclusions

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 by studying farmer groups in Senegal that seek to sell their members' agricultural production collectively. We report artefactual field experiments conducted with randomly selected members of the above referenced farmer groups to explore the role of communication on coordination.

Like previous lab studies, we find that communication significantly reduces coordination failure. However, this finding seems to be context-specific. Using treatment variation and additional survey data, we find that communication only increases coordination in larger groups. In our context, we also find that communication works through two mechanisms. First, it increases (decreases) coordination due to reduced (increased) uncertainty surrounding other players' actions. Second, by revealing information about others' actions, it establishes a norm of "equitable coordination".

From a policy standpoint, communication is a desirable mechanism for reducing coordination failure since it is relatively non-costly to implement. Further, from a policy standpoint more closely related to our context, our results illustrate the following paradoxical situation. Collective action may generate the type of economies of scale needed for smallholders to access remunerative output markets. Yet, from the sheer limited size of their individual production, seizing these market opportunities require that a large number of small family farms are able to coordinate. Coordination in larger group is here shown to be more difficult and can lead to a poverty-driven coordination trap. N-way communication may contribute to overcome these constraints. Yet, to the best of our knowledge, we are one of the first to study coordination and communication in a field-lab context, particularly when it has the potential to immediately inform policy. So, our study makes at least two contributions to the literature.

First, these artefactual field experiments were designed with subsequent natural field experiments (randomized controlled trials) in mind. We thus use the findings to predict the potential effects and mechanisms of a real-life communication institution. In fact, natural field experiments that replicate variants of our artefactual institution have already been conducted and in a separate paper, we will combine these two data sources.

Second, by revisiting certain claims made by conventional lab experiments, this study sheds light on whether the effect of communication also holds when conducted with a non-student subject pool, particularly one that has historically faced coordination failure as part of its day-to-day environment.

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