Farmers' Adoption of Irrigation Technologies: Experimental Evidence from a Coordination Game with Positive Network Externalities in India

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Abstract. *Electric irrigation contributes to food security in rural India, but deteriorating electrical infrastructures threaten the functioning of farmers' pump sets. This problem could be solved through investments in energy-efficient technologies. However, network externalities create a coordination problem for farmers. We develop a framed field experiment to study the effects of group size, leading by example, and payoff structures on the ability to coordinate technology adoption investments. The experiment is based on a game that combines features of a step-level public goods game and a critical mass game. Our findings show that smaller groups more frequently coordinate on payoff-superior equilibria and that higher payoffs lead to more investments. Contrary to previous studies, leading by example reduces investments but has no effect on efficiency. Building on this analysis, we discuss possible bottom-up solutions to the energy crisis in rural India.*

JEL classification: *Q15, O33, C93.*

Keywords: Step-level public goods game; leading by example; group size; framed field experiment.

1. INTRODUCTION

A staggering 70% of global freshwater withdrawals worldwide are used by farmers to irrigate their fields (Molden, 2007). Irrigation from groundwater resources is an essential input for agricultural production and has the potential to foster rural development and to reduce poverty in rural areas (Giordano and Villholth, 2007). Furthermore, given the erratic rainfalls in most parts of South Asia, groundwater-based irrigation provides a consistent water supply for water-intensive crops. In India, the expansion of electric agricultural pump sets has allowed many farmers to gain direct access to and control over irrigation water from groundwater resources. The free disposal of water has contributed to a rise in production, rural incomes, and food security (Badiani *et al.*, 2012; van Koppen *et al.*, 2002; Shah, 2010), turning India's Green Revolution into a 'tube well revolution' (Repetto, 1994, p. 35).

However, unregulated access to groundwater and a lack of incentives for efficient energy use have led to a water and electricity crisis in India. In many parts of the country, groundwater levels are alarmingly low and continue to decrease (Alauddin and Quiggin, 2008). Simultaneously, electric infrastructures are also deteriorating (World Bank, 2001). Electricity for irrigation is provided to farmers at subsidized flat-rate tariffs, but electric utilities have started to ration the power supply, and in large parts of India, electricity for irrigation is available for only a few hours per day. Farmers face low power supply, high voltage fluctuations, and decreasing groundwater levels, resulting in frequent pump motor burnouts, high repair costs, distorted cropping patterns, and harvest losses (Kimmich, 2013a; Sagebiel *et al.*, 2015).

A policy debate has ensued, and numerous solutions have been proposed to address this dilemma at various levels of intervention (Birner *et al.*, 2011). At the community level, one promising approach has been to test the adoption by farmers of energy-efficient demand-side measures (Sagebiel *et al.*, 2015). The problem is thus transferred to the village level, at which legitimate and effective solutions are more likely to occur (Kimmich, 2016). In the common sub-grids of rural villages, installing capacitors or standardized, quality-approved pump sets (two exemplary demand-side measures) can improve the power factor, thereby reducing the likelihood of damage for all other farmers. However, the size of this positive network externality is a nonlinear function of the number of capacitors or standardized pump sets already installed in the grid, which necessitates the coordinated action of farmers (Kimmich, 2013b).

In this paper, we study this challenge by means of a framed field experiment (Harrison and List, 2004). We develop a binary-choice step-level public goods game that shares features with a critical mass game. Drawing on the literature and the empirical setting, we vary the following factors: group size, payoff levels, and the sequence of decision-making. Our case is an interesting example of how technical properties affect the social realm within agriculture.

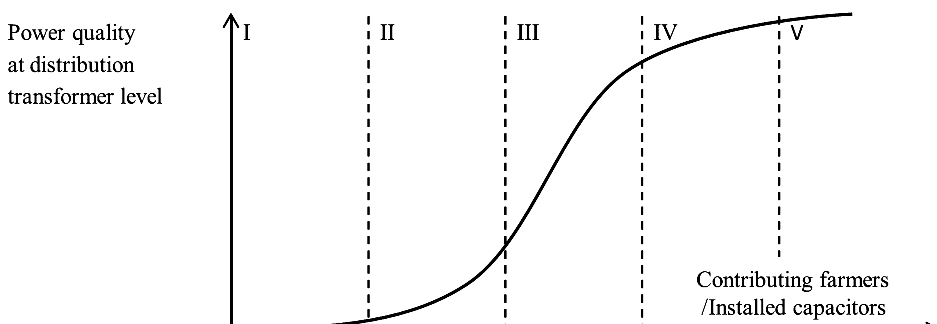
The remainder of the paper is structured as follows. The next section highlights the study context of electrically supported agricultural irrigation systems, and it introduces the coordination problem. Section 3 discusses the experimental design and develops hypotheses. Section 4 reports the experimental results, including a socioeconomic analysis, and section 5 discusses the results and draws conclusions.

2. FARMERS' COORDINATION PROBLEM

In addition to its unquestionable benefits, the current policy regime of subsidized electricity has negative consequences for those actors involved in the energy sector. Agricultural power subsidies in Andhra Pradesh are by far the largest input subsidy in agriculture, even exceeding state expenditures for education and health services (Birner *et al.*, 2011; Kimmich, 2013a). Several authors have described the development as being a vicious circle (Badiani *et al.*, 2012; Shah *et al.*, 2008; World Bank, 2001). As a consequence of the successive withdrawal of maintenance and repair services by distribution companies (Joseph, 2010), farmers have started to assume several maintenance duties, such as collectively repairing distribution transformers.

In the study area, on average 15–30 pump sets are connected to one distribution transformer. At the transformer level, voltage fluctuations and power phase distortions frequently occur, reducing the power quality and resulting in damaged motors and distribution transformers. Using high-quality appliances not only positively affects one's own pump but also creates a network externality that benefits all other farmers connected to the same distribution transformer (Kimmich, 2013b). Power quality at the distribution transformer level is thus a function of the number and quality of installed demand-side measures and can therefore be managed by farmers. Power quality shares properties with public goods because it is non-excludable and non-rival.

Given the low power quality in most villages, it is individually not beneficial to invest in efficient pump sets because they are more vulnerable to voltage fluctuations and motor burnouts (Kimmich, 2013a). However, when the number of high-quality pumps installed surpasses a certain threshold, it becomes individually beneficial to invest. One tangible example of adapting to higher quality appliances is the installation of shunt capacitors close to the pump set (Sagebiel *et al.*, 2015). Capacitors are a feasible solution at the grass-roots level because they are cheap (approximately 200 INR or $4 USD) and relatively easy to install. Capacitors are used in this study to exemplify the effects of adopting energy-efficient appliances. Figure 1 schematically displays power quality as a function of the number of installed capacitors in the sub-grid.



Power quality	Very low	Low	Medium	High	Very high
Marginal increase from additional capacitor	Very low and increasing	Low and increasing	High	Low and decreasing	Very low and decreasing
Net *social* benefit from additional capacitor	Negative	Positive	Positive	Positive	Negative
Net *individual* benefit from additional capacitor installation	Negative	Negative	Positive	Negative	Negative

Figure 1 The production function of power quality

The main characteristic of the production function is its s-shaped form, which implies an inverted u-shaped distribution for individuals' benefits from the adoption of energy-efficient pump sets. On the left part of the function, where power quality is very low (area I), the marginal increase in power quality from a capacitor is close to zero. This situation depicts the current situation in the field, where only a few connected capacitors can be found. Considering the investment cost of a capacitor, it is individually and socially rational to oppose installation because there is no benefit from a single capacitor. In area II, by contrast, the positive network externality means that it would be socially rational to invest in capacitors but still not individually beneficial. When a certain threshold is passed, the increase in power quality is large enough to make investments an individually rational strategy (area III). In area IV, similar to area II, it is still socially rational to invest in more capacitors, but individual benefits are no longer large enough. In area V, it does not pay off socially to invest.

3. THE EXPERIMENT

A framed field experiment (Harrison and List, 2004) was conducted in eight villages in Andhra Pradesh, India. A total of 225 farmers were repeatedly asked to either decide for or against investing in a capacitor. Participants played in groups of five or ten players. For each possible combination of the other farmers' decisions, Table 1 displays the payoff that a farmer receives if he decides to invest or not to invest. The depicted payoffs were chosen to reflect the coordination problem described above, reduced to the areas I, III and IV.

Our game relates to binary-choice step-level public goods games (Dawes *et al.*, 1986; Erev and Rapoport, 1990; Rapoport and Eshed-Levy, 1989; Van de Kragt *et al.*, 1983) in which a public good is provided only if a certain threshold of contributions is reached. However, our game differs from those games because it also entails features of a critical mass game (Keser *et al.*, 2012). Given a critical mass of other contributors, contribution becomes and remains a best response even

Table 1 Payoffs and modified payoffs for small groups (large groups in parentheses)

		Number of other farmers buying a capacitor		
		0–1 (0–3)	2–3 (4–7)	4 (8–9)
Low payoffs				
My choice	Buy capacitor	−10 ₹	4 ₹	5 ₹
	No capacitor	0 ₹	2 ₹	6 ₹
High payoffs				
My choice	Buy capacitor	−10 ₹	10 ₹	11 ₹
	No capacitor	0 ₹	5 ₹	12 ₹

beyond the contribution threshold. Furthermore, our game differs from a critical mass game because it also has an upper threshold beyond which contributing does not pay off individually, although contributions still pay off socially. Thus, our game has a symmetric Nash equilibrium where no one invests and a number of payoff-superior Nash equilibria in which the upper threshold is provided. In contrast to step-level public goods games with continuous endowments (e.g. Normann and Rau, 2015; Suleiman and Rapoport, 1992), all payoff-superior equilibria in our binary game are asymmetric. There is at least one player (the fifth or 9th and 10th) who has an incentive to free ride if all others invest, whereas the fourth investing player (or 7th and 8th) has no incentive to deviate from investing. The number of asymmetric equilibria is five in the five-player treatment, and $\sum_{i=1}^{n}(n-i)=45$, where n is ten for the large group treatments. For both group sizes in our game, the asymmetric equilibria correspond to an 80% investment ratio, and the welfare optima are at a 100% investment ratio for small groups and a 90% investment ratio for large groups.[1] The game also has two Pareto-ranked symmetric equilibria in mixed strategies, which we present in the appendix.

3.1. Treatments and experimental procedure

To study the role of factors that are considered to be relevant in the field, we varied three aspects of the game: group size, the sequence of decision making, and the payoff function. For each of these factors, two versions are employed. First, we tested the effect of different group sizes by doubling the group size from five players (henceforth, 'small group') to ten players[2] ('large group') while holding the payoffs constant. Second, we tested the difference between simultaneous decision making and leading by example. Every group played a series of six rounds with and six rounds without a leader in random order across groups. In the leading by example version, a leader was drawn randomly and anonymously in each round for each group, and the leader decided before the rest of the group. Only after the leader's decision was made and announced to the group did the other group members make their decisions simultaneously.[3] Third, the

1. Chidambaram *et al.* (2014) conduct a framed field experiment on transportation in India. Six players can choose between using the bus or the car. Similar to our game, there are multiple asymmetric Nash equilibria in pure strategies where three players choose the bus and three players choose the car. However, it is socially optimal to have five players choose the bus and one player choose the car. Our game has a similar area in which it would be socially optimal to invest, although investing offers no Nash equilibrium.
2. In the context of the experiment, a group of ten persons can still be considered small. For simplicity, we will henceforth refer to the five-player groups as small and to the ten-player groups as large.
3. Following the procedure developed by Rommel and Janssen (2015), all participants first decided on their investments if they were selected as the leader. One decision was then randomly drawn and publicly announced without revealing the leader's identity. Without using software, this procedure is difficult to implement. To ensure anonymity and to avoid conflict in the community, we decided against aiming for endogenous non-anonymous leadership as in, for instance, Jack and Recalde (2015). With a small note, randomly selected leaders were informed that no additional decision was necessary. In accordance with laboratory experiments on leading by example (e.g. Güth *et al.*, 2007; Potters *et al.*, 2007), this procedure ensures that the leaders know that they can impact the subsequent choices and that the followers are fully aware of the leader's decision.

Table 2 Combinations of treatments and number of subjects

		High payoffs		Low payoffs	
		Group-size treatment (between subjects)		Group-size treatment (between subjects)	
		Group size: five	Group size: ten	Group size: five	Group size: ten
Leadership treatment (within subjects, in random order, balanced within villages)	Simultaneous decisions (six rounds) Sequential decisions with one 'lead farmer' (six rounds)	8 groups, 40 subjects	7 groups, 70 subjects	7 groups, 35 subjects	8 groups, 80 subjects

experiment was conducted with different payoff levels to test the responses to changes in the relative attractiveness of the equilibria. Similar to a subsidy, payoffs above the threshold in area two of the lower panel of Table 1 were increased and compared to the low-payoff version.[4] The combination of all three factor variations resulted in eight single treatments, where each treatment was played the same number of times in a full factorial design.

Table 2 summarizes the combination of experimental treatments. For practical reasons, group size and payoff levels were implemented between subjects, whereas leading by example was implemented within subjects (see Charness *et al.*, 2012, for a discussion of the merits and demerits of within- and between-subject designs). Group composition was constant across the course of the experiment (partner-matching design).

The experiment was conducted as a 12-round paper and pencil game in eight sessions around the town of Vemulawada in Karimnagar District of Andhra Pradesh, India (cf. Figure 2). In collaboration with local partners, farmers were recruited directly in the villages. Each session involved 30 farmers from one village (two groups of five players and two groups of ten players).[5] After a welcome address, general instructions were read aloud in the local language, Telugu. Instructions consisted of 1) a short explanation of the electrical interdependence of pump sets and the effect of capacitors, including the threshold, supported by illustrations on a whiteboard, and 2) an explanation of the game, including payoffs, the structure of the player sheets to write down decisions, and how leading by example would be implemented. Subjects were allowed to ask questions at

4. Note that payoffs were not changed by a simple linear transformation. We changed payoffs by a factor of 2.5 in the middle area and by 2 (2.2, respectively) in the right area to emphasize the marginal decreases in the upper part of the curve. One could think of this change as a flexible subsidy for energy-efficient measures. Early adopters receive a higher subsidy than latecomers. A similar policy is currently being discussed by the government (Sagebiel *et al.*, 2015).
5. Function halls or public spaces were used to conduct the experiment. In two cases, we did not find enough farmers to play the game. Thus, one large and one small group are missing from a total of eight villages.

Figure 2 Location of the study area in Andhra Pradesh, India

any time. Field staff was trained to respond to questions individually. It was explained that participation was voluntary and that anyone could leave the experimental sessions at any time (no one did).

After the instructions, subjects were randomly assigned to groups by drawing numbers from a box. Participants started with an endowment of 120 INR (Indian Rupees) to ensure positive payoffs after 12 rounds of play. Another 100 INR was added as a participation fee after completion. In each group, two or three assistants organized the game. After each round, participants handed over their decision sheets; the total number of capacitor investments was announced in Telugu and in English; individual payoffs were calculated and recorded on paper; and decisions and payoffs were read aloud and illustrated on a prepared whiteboard.

After completion of 12 rounds, participants (assisted by field staff) were asked to complete a questionnaire, which was designed to control for socioeconomic heterogeneity in the behavioral data. Table 3 summarizes the experimental procedure.

Table 3 Sequence of events in an experimental session

1. Welcome and instructions	
2. Random assignment to groups by drawing ID numbers and handout of player sheets	
3a. Small groups (five players); every player starts with 120 INR	3b. Large groups (ten players); every player starts with 120 INR
4a. Groups play either six rounds simultaneously or with leading by example	4b. Groups play either six rounds simultaneously or with leading by example
5a. Groups play six rounds: the opposite of 4a.	5b. Groups play six rounds: the opposite of 4b.
6. Post-experimental questionnaire on socioeconomic and field context variables	
7. Receive payoffs in sealed envelope and sign receipt	

3.2. Hypotheses

The experimental literature on pure coordination games and (step-level) public goods games shows that some key variables can facilitate coordination (Camerer, 2003; Croson and Marks, 2000; Devetag and Ortmann, 2007; Ledyard, 1995; Ochs, 1997; Van Huyck *et al.*, 1990). Research in the lab has shown that an increase in group size often leads to lower payoffs because small groups are more likely to coordinate on efficient outcomes (Franzen, 1995; Harrison and Hirshleifer, 1989; Isaac and Walker, 1988; Van Huyck *et al.*, 1990). In step-level public goods games, Offerman *et al.* (1996) and Schram *et al.* (2008) find that an increase in group size from five to seven players reduces contributions when keeping the threshold constant. Generally, strategic uncertainty is reduced in a smaller group, and strategies can be aligned more easily. Conversely, more actors increase the strategy space, and the technical threshold at the transformer escalates. In our game, in small groups, a player needs investment from only one out of the four other participants (= 25%) to achieve a positive payoff from investing, while in a large group, three out of the nine other participants (= 33%) are needed. It is thus more difficult to obtain investments in the large group. Therefore, we conjecture as follows:

H1 A larger group size reduces the ability to coordinate investments on Nash equilibria in pure strategies; and

H1a The relative frequency of investments is higher in small groups.

The experimental literature also shows that leading by example has a positive impact on coordination outcomes (Erev and Rapoport, 1990; Harrison and Hirshleifer, 1989; Normann and Rau, 2015; Weber *et al.*, 2001; Wilson and Rhodes, 1997). Leader decisions in sequential coordination experiments might create a focal point for the choices of the subsequent players (Foss, 2001; Wilson and Rhodes, 1997), thus reducing strategic uncertainty. In the five-player leading-by-example version of our game, six subgame-perfect Nash equilibria exist. Two equilibria remain as a pure coordination problem if the leader does not invest. Followers must then coordinate between the inefficient equilibrium, where no one invests, and the efficient equilibrium, where all four remaining players invest. However, if the leader invests, four equilibria remain, where three additional players invest. Similarly, in the ten-player treatments, investment by the leader eliminates the Nash equilibrium where no one contributes. Non-investment by the leader reduces the number of asymmetric subgame perfect equilibria from 45 to 36. Hence, we test the following hypotheses:

H2 Leading by example increases the coordination on Nash equilibria in pure strategies; and

H2a Leading by example increases the relative frequency of investments.

Social dilemma games have shown that increasing the payoff from cooperative strategies reduces defection. Brandts and Cooper (2006) varied the bonus in a four-player weakest-link coordination game and found that subjects were more likely to choose higher effort levels when the bonus was high. Increasing the value of the public good in step-level public goods games also increases contributions (Ledyard, 1995; Schram *et al.*, 2008). Considering the practical implication of a change in the payoff structure, we expect as follows:

H3 Increasing the returns from the public good of power quality by increasing payoff levels increases the relative frequency of investments.

4. RESULTS

Table 4 displays summary statistics from the coordination experiment and the post-experimental questionnaire. Data are reported for all 225 participants and aggregated over 12 rounds for variables with within-differences. Aggregated over all treatments, participants decided to invest in 65.07% of the cases and in 67.55% of the cases in the first round (a distributional graph can be found in Figure S3 in the appendix).

On average, participants switched four times between options, with a range from zero to ten changes (*CHOICE_CHANGE*). At the group level, *SHARE_INVESTMENTS* reports the relative frequency of positive investments per round for all 30 groups over 12 rounds. The average earnings were five INR per round. Adding the participation fee to the individual earnings, participants received 282 INR on average, with a minimum of 177 INR and a maximum of 356 INR. The average payoff was equal to approximately 4.50 USD, which is more than the daily income for farmers in the study area.

Table 4 also displays data from participants that were collected through the post-experimental questionnaire[6]: All participants were male (although female participation was explicitly welcomed), and almost all participants were farmers.

6. While socioeconomic characteristics were fairly balanced across the group-size treatments, variations in age, farm income, household size, and paddy cropping were found to be different for the payoff treatment. We control for these variables in the regression models and find that none of them influences decisions at a statistically significant level. However, we cannot rule out that unobserved socioeconomic characteristics are confounded with the payoff treatment. The corresponding comparisons can be found in the appendix (Tables 1 and 2).

Table 4 Summary statistics for experimental decisions and socioeconomic variables

Variable name	Explanation	*N*	Mean	SD	Min	Max
INVESTMENT	Choice in the game (= 1 if invested)	2,700	0.65	0.48	0	1
INVESTMENT_FIRST	First-round choices (= 1 if invested)	225	0.68	0.47	0	1
AGGR_INVESTMENTS	Sum of investments of a player across all 12 rounds	225	7.81	3.07	0	12
CHOICE_CHANGE	Individual frequency of switches	225	3.98	2.68	0	10
SHARE_INVESTMENTS	Relative frequency of investments at the group level	360	65.53	19.36	0.1	1.0
LEADERSHIP	= 1 if choice is in leading by example treatment	2,700	0.5	0.50	0	1
HIGH_PAYOFFS	= 1 if player is in high-payoff treatment	225	0.49	0.50	0	1
SMALL_GROUP	= 1 if player is in a small group (five players)	225	0.33	0.47	0	1
LEAD X SMALL	= 1 if choice is in leading by example and small group	2,700	0.17	0.37	0	1
STARTS_SIM	= 1 if player/group starts with simultaneous play	225	0.53	0.50	0	1
EARNROUND	Earnings per round	2,700	5.19	5.18	−10	12
EARNROUND_FIRST	Earnings in first round	225	5.21	5.10	−10	12
MONEY_PAID	Total money paid at the end of the experiment	225	282.32	43.68	177	356
AGE	Age in years	225	37.79	12.00	18	80
EDU	Years of education	217	8.05	4.76	0	17
FARM_INCOME	Average monthly farm income (in 1,000 INR)	225	5.93	4.80	0	33.3
INCOME_SOURCE	= 1 if farming is the main source of income	221	0.79	0.41	0	1
HH-SIZE	Household size	224	5.27	2.00	2	15
ONLY_BICYCLE	= 1 if farmer owns only a bicycle	225	0.31	0.46	0	1
LARGE_FARMER	= 1 if farmer cultivates five or more acres	225	0.23	0.42	0	1
HIGH_CASTE	= 1 if farmer belongs to a higher caste[a]	225	0.10	0.30	0	1
COOP_MEMBER	= 1 if member of a farmers' cooperative	225	0.24	0.43	0	1
PADDY_FARMER	= 1 if farmer mainly cultivates paddy rice	225	0.53	0.5	0	1
PUMPSET	= 1 if farmer owns a pump set	225	0.83	0.37	0	1
KNEW_CAPACITOR	= 1 if participant knew about capacitors before the game	222	0.79	0.41	0	1
CAPACITOR_INSTALLED	= 1 if participant had installed a capacitor at his pump set	184	0.09	0.29	0	1
BURNOUTS_LY	Count for the number of motor burnouts last year	187	0.99	0.98	0	5
MEAN_INCOME	Average income at the group level	30	5.84	2.69	2	12.53
INEQUAL	Standard deviation of farm income at the group level	30	3.70	1.77	1.28	9.67

Note: [a]According to the Indian Constitution and standard statistical classification, Indian citizens are either classified as Scheduled Tribes (ST), Scheduled Caste (SC), Other Backward Caste (OBC), or Other (higher) Castes.
Source: Own data.

Approximately, 80% of the participants stated that farming is their main source of income, and the monthly average earnings are approximately 6,000 INR. The variable *ONLY_BICYCLE* was used as an additional indicator of poverty because most farmers possess a motor bike, and some even own a car or tractor. However, nearly one-third of participating farmers owned only a bicycle. The vast majority of farmers possess a pump set. Although 80% had heard about capacitors before the game, only 10% of the pump-owning farmers had installed a capacitor. On average, the farmers had suffered from one motor burnout within the last 12 months, with a maximum of five burnouts. Finally, *INEQUAL* provides a simple measure for inequality at the group level by calculating the standard deviation of monthly farm income at the group level.

We begin the statistical analysis of the treatment effects by reporting the occurrence of asymmetric Nash equilibria over the full period of the game to test H1 and H2, followed by discussing the unconditional treatment effects on first-round investment decisions to test H1a, H2a, H3, ordering effects, and socioeconomic heterogeneity. Finally, we provide an analysis based on data aggregated across rounds.

4.1. Treatment effects on equilibrium play

The participants never coordinated their investments on the lower, inefficient equilibrium where no one invests (cf. Table 5). In the following, whenever we speak of coordination on the equilibrium, we are thus referring to the asymmetric superior equilibria. No power quality was provided in only 21 out of 360 cases. In all other cases, at least two players invest in small groups, and at least four players invest in large groups. Small groups achieved the social optimum in 16 out of 180 cases and large groups in 14 out of 180 cases. Aggregated across treatments and rounds, in 98 out of 360 cases, one of the asymmetric Nash equilibria was reached. Table 5 displays the relative frequency of investment decisions by the eight treatments for first-round decisions and for decisions over the course of the game. The table distinguishes groups that start with simultaneous decisions from groups that start with leading by example. The table also shows the relative frequency of Nash equilibria in pure strategies and the social optima over the course of the game at the group level. Figures S1 and S2 in the appendix show the number of players investing by groups and by treatments.

There are fewer combinations of strategies in the small group, and as expected, participants in small groups coordinate more frequently on the asymmetric Nash equilibria (36.67% vs. 17.78% in large groups, $p < 0.001$ for a one-sample test of a difference in proportions). We therefore accept H1. Leading by example does not improve coordination over all rounds at the group level (26.67% in simultaneous play vs. 27.78% in leading by example). Leading by example marginally increases coordination in small groups from 35.56% to 37.78%, whereas there is no effect from leading by example in large groups. Consequently, we reject H2.

4.2. Treatment effects on first-round investment decisions

To investigate how the treatments impact individual decisions, we first look at first-round investment decisions. In the game, participants were more likely to

Table 5 Relative frequency of investment decisions and equilibrium play by treatments

	Low payoffs		High payoffs	
	Five players	Ten players	Five players	Ten players
Simultaneous play				
First-round decisions	70.00% (N = 20)	72.50% (N = 40)	65.00% (N = 20)	75.00% (N = 40)
All-round decisions starting with simultaneous play	56.67% (N = 120)	67.50% (N = 240)	71.67% (N = 120)	75.83% (N = 240)
All-round decisions starting with leading by example	66.67% (N = 90)	53.75% (N = 240)	69.17% (N = 120)	67.22% (N = 180)
Symmetric Nash equilibrium[a]	0% (N = 42)	0% (N = 48)	0% (N = 48)	0% (N = 42)
Asymmetric Nash equilibrium[b] for groups starting with simultaneous play	16.67% (N = 42)	8.33% (N = 42)	29.17% (N = 42)	25.00% (N = 42)
Asymmetric Nash equilibrium[b] for groups starting with leading by example	44.44% (N = 18)	12.50% (N = 42)	54.17% (N = 42)	27.78% (N = 18)
Welfare optimum[c]	2.38% (N = 42)	8.33% (N = 48)	12.5% (N = 48)	11.90% (N = 42)
Leading by example				
First-round decisions	66.67% (N = 15)	55.00% (N = 40)	75.00% (N = 20)	63.33% (N = 30)
All-round decisions starting with simultaneous play	56.67% (N = 120)	61.67% (N = 240)	79.17% (N = 120)	74.58% (N = 240)
All-round decisions starting with leading by example	66.67% (N = 90)	50.83% (N = 240)	68.33% (N = 120)	62.22% (N = 180)
Symmetric Nash equilibrium[a]	0% (N = 42)	0% (N = 48)	0% (N = 48)	0% (N = 42)
Asymmetric Nash equilibrium[b] for groups starting with simultaneous play	20.83% (N = 24)	37.50% (N = 24)	50.00% (N = 24)	16.67% (N = 24)
Asymmetric Nash equilibrium[b] for groups starting with leading by example	38.89% (N = 18)	8.33% (N = 24)	41.67% (N = 24)	5.56% (N = 18)
Welfare optimum[c]	4.76% (N = 42)	2.08% (N = 48)	16.67% (N = 48)	16.67% (N = 42)

Note: [a]In pure strategies; Relative frequency at the group level over all rounds where no one invests.
[b]In pure strategies; Relative frequency at the group level over all rounds where four [eight] players invest in small [large] groups.
[c]In pure strategies; Relative frequency at the group level over all rounds where five [nine] players invest in small [large] groups.

invest when they were part of a small group only if they started with the leading by example version of the game (cf. Table 5). The gross effect of the large group treatment is zero, and we therefore reject H1a.

Leading by example in the first round resulted in fewer investment decisions (62.86% vs. 71.67% in simultaneous play, one-sample test of proportions, $p = 0.0795$), and we reject therefore H2a. The lower frequency can be partially attributed to the decisions by the leaders, who invested only in 50% of the cases in the first round. Followers invested in 70.83% of the cases if the leader did not invest and in 58.14% of the cases if the leader invested in the first round. Overall, followers in the leading by example treatment still invested less in the first round than players in the simultaneous version of the game (64.84% vs. 71.67%).

The gross effect of a change in the payoff structure was small for first-round decisions. On average, 70.00% of all choices in the high-payoff treatment were in favor of investment, compared to 65.22% when payoffs were lower, but a one-sided two-sample test of proportions shows that this difference is zero ($p = 0.2207$), and we therefore reject H3 for first-round decisions.

4.3. Ordering effects

Changing the sequence of simultaneous decision making and leading by example between groups led to an ordering effect. Participants who started with simultaneous decision making in round one invested more frequently than participants who started with leading by example (68.61% vs. 61.03% in simultaneous decisions, two-sample test of proportions, $p = 0.0000$). If participants started with simultaneous play, they invested in 69.17% of the cases in the first six rounds. After leading by example was introduced in the seventh round, this frequency changed only slightly to 68.06%. Conversely, if participants started with the leading by example version, they invested at a frequency of only 59.68% in the first six rounds. Introducing simultaneous decision making in round seven increased this frequency slightly to 62.38% ($p = 0.3261$).

A different picture emerges when equilibrium play is considered. Groups that started with simultaneous play coordinated less often on one of the Nash equilibria (in 25.52% of the cases) than groups that started with the leading by example treatment (29.17%, $p = 0.4381$). Here, a conflicting role of leading by example can be observed. Groups that started with the simultaneous version coordinated in 19.79% of the cases on one of the Nash equilibria in the first six rounds. At 31.25%, this figure is significantly larger after the introduction of leading by example in round seven ($p = 0.0686$). Groups that started with leading by example coordinated in 23.81% of the cases on one of the Nash equilibria in the first six rounds, whereas this figure increased to 34.52% ($p = 0.1266$) for rounds 7 to 12. However, because we randomized the order, the data can be pooled for analysis.

We also observe a small learning or experience effect for equilibrium play. Groups coordinated significantly less often on one of the Nash equilibria in the first six rounds than in rounds 7 to 12 (21.67% vs. 32.78%, $p = 0.018$). However, we do not find a clear linear trend. Furthermore, there is no learning or experience effect if we only consider the average investments (64.74% from round one to six vs. 65.40% from round 7 to 12, Figure S4 in the appendix).

4.4. Socioeconomic heterogeneity

To investigate how socioeconomic heterogeneity drives decision making and to test the robustness of the treatment effects when controlling for contextual factors, we present the results from several regression models. We first analyze the first-round decisions.[7] Next, we present a number of dynamic regression models, which include lagged variables on outcomes of the previous round. Model (1) in Table 6 is a logistic regression on first-round decisions with treatments as explanatory variables. Model (2) additionally controls for socioeconomic characteristics.

The lack of an effect from group size on investments is confirmed. The negative effect of leading by example is statistically significant at the 10% level for first-round decisions, as indicated by the coefficient of *START_SIM,* which in the first round is always the opposite of the *LEADERSHIP* variable. The coefficient for the interaction term of leading by example in small groups is positive but not significant at the 10% level.

When controlling for socioeconomic characteristics, the treatment variables do not substantially change, but some do affect behavior. Higher income inequality at the group level (*INEQUAL*) significantly increases the likelihood that farmers invest and also leads to higher payoffs. This finding is robust across specifications and at the group level (Table S4 in the appendix). While the average income at the group level does not affect investments, inequality does.

4.5. Pooled data and leaders and followers' decisions

To test for learning and dynamic effects, we extend the analysis to the full dataset across individuals and rounds.[8] Table 6 displays three dynamic panel data models that include lagged variables. All three models are random-effects logistic panel regressions with the binary choice in the game as the dependent variable. In addition to the between-treatment variables and socioeconomic variables, these models include leading by example as a dummy and lagged variables. *INVESTMENT_PREV* is the lagged variable representing the decision from the previous round, and *SHARE_INVESTMENTS_PREV* reports the relative frequency of investments per group in the previous round.

Models (3) and (4) confirm that there is no effect from group size on investment. Contrary to first-round investment decisions, players invested significantly more over the course of the game when they played in the high-payoff treatment. We do not observe a linear trend from *ROUND* over time. For further information on decisions over time, the appendix provides additional material, disaggregated by treatments and groups (Figures S1–S5).

Model (4) also shows a positive effect of group-level income inequality and additionally a negative effect of high caste and being a large farmer on individual investments. The negative caste effect is in line with another coordination

7. To include the consideration of individual investments over the course of the game, we further estimate regression models on aggregate investments per participant (*AGGR_INVESTMENTS*). These models can be accessed in the appendix.
8. As an additional robustness check, we ran two linear OLS on the pooled decision data at the group level, which can be found in Table S6 of the appendix.

Table 6 Regressions on first-round decisions and dynamic models

	(1) *INVESTMENT* First-round logit	(2) *INVESTMENT* First-round logit	(3) *INVESTMENT* All-rounds random-effects logit	(4) *INVESTMENT* All-rounds random-effects logit	(5) *INVESTMENT* Random-effects logit: only followers
HIGH_PAYOFFS	0.179	−0.034	0.782***	0.739***	0.653***
	(0.289)	(0.329)	(0.212)	(0.219)	(0.228)
SMALL_GROUP	−0.303	−0.106	−0.033	0.093	0.296
	(0.423)	(0.471)	(0.244)	(0.247)	(0.235)
STARTS_SIM	0.675*	0.673*	0.467**	0.300	0.383*
	(0.352)	(0.393)	(0.209)	(0.210)	(0.222)
LEAD X SMALL	0.848	0.965	0.317	0.288	
	(0.616)	(0.709)	(0.209)	(0.217)	
ROUND			−0.006	−0.003	
			(0.015)	(0.016)	
LEADERSHIP			−0.216*	−0.209*	
			(0.119)	(0.122)	
INVESTMENT_PREV			−0.168	−0.101	0.261
			(0.127)	(0.131)	(0.232)
SHARE_INVESTMENTS_PREV			−0.020	−0.305	
			(0.330)	(0.344)	
LEAD_INVEST					0.121
					(0.170)
SHARE_FOLLOW_PREV					0.432
					(0.410)
PADDY_FARMER		−0.379		−0.170	
		(0.350)		(0.223)	
HIGH_CASTE		−0.942		−2.751**	
		(1.502)		(1.133)	

Table 6. Continued

	(1) *INVESTMENT* First-round logit	(2) *INVESTMENT* First-round logit	(3) *INVESTMENT* All-rounds random-effects logit	(4) *INVESTMENT* All-rounds random-effects logit	(5) *INVESTMENT* Random-effects logit: only followers
LARGE_FARMER		–0.334		–0.464*	
		(0.426)		(0.272)	
INEQUAL		0.367***		0.204***	
		(0.133)		(0.071)	
Constant	0.270	0.356	0.440	–0.605	–0.355
	(0.272)	(1.100)	(0.288)	(0.722)	(0.293)
N	225	209	2,475	2,299	975
Pseudo R^2	0.016	0.072			
Log lik.	−139.510	−124.260	−1,436.878	−1,331.910	−593.857
χ^2	4.559	19.352	23.447	43.201	21.407
χ^2 for comparison test			192.516	143.348	17.824
Log lik. comparison model			−1,533.136	−1,403.584	−602.769

Notes: Models (2) and (4) additionally control for *AGE, EDUCATION, FARM_INCOME, INCOME_SOURCE, ONLY_BICYCLE, HH_SIZE, KNEW_CAP, BURNOUTS_LY*, and *COOP_MEMBER*; A table with all coefficients can be found in the appendix in Table 5.
Standard errors in parentheses, $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

experiment conducted in the field in India that explicitly analyzes caste behavior (Brooks *et al.*, 2014). The coefficients of the lagged variables are rather small and statistically not significant, suggesting that dynamics do not play a large role in decision making. The overall effect of leading by example is negative and statistically significant at the 10% level, even when controlling for socioeconomic characteristics. This finding contradicts previous findings from coordination and step-level public good games.

For additional analysis, Model (5) in Table 6 uses only the decisions of followers in the leading by example treatment. We include *LEAD_INVEST* as a variable that takes the value of one if the leader has invested and zero otherwise. This approach allows us to investigate the effect of leading by example conditional on the leader's decision. Here again we do not find large or significant effects, suggesting that the leaders' decisions do not strongly impact their followers' choices. Compared to an overall relative investment frequency of 66.00% in the simultaneous game, leaders also invested at 66.00% across both group sizes. By contrast, followers chose to invest moderately less compared to the simultaneous version. When split by group size, follower behavior shows considerable differences. The decisions reveal that followers in large groups invested less often by 5.7 percentage points (62.09%) than those in small groups (67.78%). While the leaders' decisions do not explain the overall negative effect of leading by example on decisions in the game, followers' decisions do. Leaders based their lead decision on previous play in their groups and were more likely to invest if the frequency of investments in the previous round was relatively higher (Figure S6 in the appendix displays this relationship).

In line with findings from other coordination experiments (Selten *et al.*, 2007), frequent strategy changes resulted in lower overall payments. In all treatments, the Spearman rank correlations between cumulative earnings (*SUM_EARNROUND*) and the individual sum of choice changes (*CHOICE_CHANGE*) are strictly negative. The correlation coefficient for all 225 participants across treatments is $\rho = -0.3341$ and statistically significantly different from zero at a level of 1% (Figure S7 in the appendix provides a graphical illustration).

5. DISCUSSION AND CONCLUSION

Only a few farmers in our study area use capacitors and other energy-efficient irrigation equipment in the field. However, farmers managed to coordinate on the efficient asymmetric equilibrium quite frequently in our experiment and chose to invest in approximately 65% of the cases. This difference might be explained by differences in the decision context, which is more complex in the field (Kimmich, 2013b). Similar to other coordination or step-level public goods games, we find a negative effect of group size on equilibrium coordination. However, this effect is not explained by individual investments, probably because the number of potential outcomes is lower in small groups. In contrast to previous experiments, we found that leading by example has no effect on equilibrium coordination and tends to *decrease* investments. This finding cannot be primarily attributed to the leaders' decisions. In our experiments, leaders did not succeed in establishing focal points or in affecting followers' decisions. However, we also do not find evidence of 'bad leadership' (Keuschnigg and Schikora, 2014), and similar to previous studies (e.g. Dong *et al.*, 2013; Weber *et al.*, 2001), the overall effect of leading by example was small.

In previous studies, leading by example improves coordination, especially in cases where coordination failure had prevailed in the absence of leadership. In our study, this is not the case; even without leading by example, participants achieve large investments. Leaders in the experiment were anonymous and randomly selected; it would be instructive to see how decisions would change if leadership was tangible through the presence of actual publicly known leaders making decisions in an experiment, although this would be forbidden by experimental ethics. Furthermore, the credibility of the leaders' choices might be considered (Wilson and Rhodes, 1997). A negative difference between the external assignment of a leadership role (as in the experiment) and the endogenous evolution of leadership was also observed in other studies (Cartwright *et al.*, 2013).

We showed that over the course of the game, investments increase when payoffs increase. This pattern provides a strong argument for subsidizing capacitors or other energy-efficient technologies to reduce energy use and stabilize electricity grids in rural India. The clash between individual and social interest might be bridged by a subsidy that internalizes the positive network externality. We found that economic inequality at the group level has a positive effect on investments: poor farmers might be encouraged to invest by expecting that the rich will invest. Although some studies find that heterogeneity in wealth makes coordination and cooperation more likely, the issue is still much contested in the debate on collective action in rural India (Bharamappanavara *et al.*, 2016; Poteete and Ostrom, 2004). Future research could investigate this issue in greater detail by manipulating endowments in an experimental design in which investment decisions are not binary but continuous.

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SUPPORTING INFORMATION

Additional Supporting Information may be found in the online version of this article:

Figure S1. Number of investments in the low-payoff treatments (group level).
Figure S2. Number of investments in the high-payoff treatments (group level).
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Figure S4. Relative frequency of investments by rounds across all treatments.
Figure S5. Relative frequency of investments by treatment combinations.
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Table S1. Test of socioeconomic differences by group size treatment.
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