Communication, Observability and Cooperation: a Field Experiment on Collective Water Management in India

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Abstract

This study is an empirical investigation of the potential for communication and observability interventions to increase cooperation around communal water treatment systems amongst villagers in rural India. Despite the dependence of many rural communities in India on communal water sources and treatment plants for safe drinking water, they often fail to collectively manage these resources, resulting in abandoned water points and treatment systems with consequent health and mortality impacts. Results of public goods games framed in terms of the management of communal water treatment systems suggest that these communities can cooperate to provide the public good. However, public disclosure of behaviour had the very significant effect of *decreasing* contributions to the public good. Analysis indicates that early rounds of play were critical in this regard: the observed behaviours of other individuals at the start of play strongly determined subsequent cooperation levels. Thus, frequently-observed free-riding behaviour in early rounds of the game led other players to follow suit. Only when participants were actively encouraged to negotiate agreements, did cooperation increase significantly - albeit intermittently. Overall, findings suggest that interventions that provide opportunities for communication and negotiation may be most effective at enhancing cooperation around communal water treatment plants, while the impacts of interventions that make behaviour observable are strongly dependent on behaviour in the early stages of cooperative interaction.

JEL codes: C71, C93, D70, D83, H41, I31, Q53

1. INTRODUCTION

In many rural areas of the developing world, the provision of safe drinking-water represents a 'social dilemma' (Dawes, 1980) - a situation in which there is a conflict between short-term individual interest and long-term collective interest. This is due to the communal nature of water facilities typically found in rural communities. The long-term operation and maintenance of shared facilities depends on successful cooperation among users, yet individual incentives to free-ride may undermine collective efforts (Ostrom, 1990). This is particularly problematic when it comes to complex technologies needed to treat groundwater with naturally-occurring contaminants such as fluoride or arsenic. In the absence of regular upkeep and ongoing operation, the technology cannot treat the water, resulting in health and mortality impacts. In these situations, it is clearly in the community's interest to cooperate in the management of their water treatment plants.

One approach to solving social dilemmas involves the centralized provision of the public good coupled with regulation. In the case of water provision, this might consist of introducing piped water to individual homes. However, this requires extensive infrastructure development, which is contingent on economic growth and institutional development, both of which take time (Guiteras et al. 2016). Another alternative is the development of privately-operated pay-per-container 'water kiosks'. These are only feasible however if there is institutional support from governments as well as buy-in from local communities (Kariuki and Schwartz, 2005). Moreover, a lack of regulation and regular water quality monitoring has been found to limit the ability of water-kiosks to provide safe water (Opryszko et al., 2009).

Privatization and centralized provision, however, are not the only alternatives. There is ample evidence from field observations (e.g. Meinzen-Dick, Raju and Gulati, 2002; Baland and

Platteau, 1996; Ostrom, 1990) and field experiments (e.g. Midler et al. 2015; Velez, Stranlund and Murphy, 2012; Travers et al. 2011; Cardenas, Rodriguez and Johnson, 2011) that individuals can cooperate to overcome social dilemmas without the need for external interventions. This suggests that people are not solely driven by self-interest, but may also be influenced by social cooperation norms, such as reciprocity, conformity and reputation. In such cases, collective action is achieved via internal self-regulating mechanisms or institutional 'design principles' that promote cooperation and self-governance by stimulating these pro-social cooperation norms.

One of the most researched of these self-regulating mechanisms is communication (e.g. Velez et al. 2012; Velez, Murphy and Stranlund, 2010; Cardenas, Stranlund and Willis, 2000; Andreoni and Rao, 2011; Isaac and Walker, 1988). By communicating with each other, individuals may potentially identify the intended behaviour of others, making coordination on the public good more effective. Communication can also reduce perceived 'social distance' and enhance group identity, which influences trust and reciprocity (Balliet, 2009; Cardenas et al, 2011; Bohnet and Frey, 1999). Most importantly, communication allows for the crafting of agreements (Ostrom, 1998), in the absence of which, communication may have a negligible effect on cooperation (Orbell, van der Kragt and Dawes, 1988). For example, in a study by Cardenas, Rodriguez and Johnson. (2011) which used public goods games in the field to explore cooperation around watersheds for irrigation, communication failed to increase cooperation amongst participants in Kenya, although the same treatment did increase cooperation in Colombia. The authors conclude that the difference depends on the crafting of agreements: while 75 per cent of Colombian participants believed that the group reached an agreement during the game, only 33 per cent of the Kenyan participants answered this question affirmatively in the exit survey. Communication thus appears to be most effective when agreements are negotiated, even if the agreements are non-binding (Ostrom, Gardner and Walker, 1994; Orbell et al. 1988).

However, despite the extensive evidence about the positive effects of cooperation, most of which are based on laboratory experiments, there are a handful of field experiments that find mixed or no effects of communication on cooperation (e.g. Midler et al., 2015; Meinzen-Dick et al., 2014; Velez, Murphy and Stranlund, 2010; Velez, Stranlund and Murphy, 2012; Cardenas, Rodriguez & Johnson, 2011; Ghate, Ostrom and Ghate, 2011), suggesting that communication effects in the lab may not translate directly to the field, especially in villages where players tend to know each other and have shared norms which communication may not change. Therefore, we feel that the question of communication effects on cooperation in the field merits further empirical work.

Another important mechanism that has been found to increase cooperation, and other pro-social behaviour, is 'observability' (e.g. Alcott, 2011; Andreoni and Bernheim, 2009; Shang and Croson, 2009; Frey and Meier, 2004; Rege and Telle, 2004). Observability refers to the visibility of individual behaviours to others. By increasing the observability of behaviour, individuals can monitor each other, and free-riders can be identified and sanctioned using fines as well non-pecuniary punishments, such as social disapproval and shame (Rege and Telle, 2004; Loewenstein, 2000). This motivates cooperation because most people prefer to be well-regarded amongst peers and to have a positive view of themselves (Benabou and Tirole, 2005; 2011). There are many group-based experiments in which a first-mover or 'leader' makes a publicly observable contribution before anyone else in their group. In these studies, first-movers consistently give significantly more than later-movers (e.g. Dannenberg, 2014; Figuieres et al, 2012)[1], and empirical evidence suggests the main reason for this behaviour is reputational (Arbak and Villeval, 2013; Andreoni and Petrie, 2004).

Furthermore, observing others cooperating can help establish cooperation as the norm, as people tend to conform to frequently-observed behaviour (Velez, Stranlund and Murphy, 2009; Andreoni and Bernheim, 2009). This is observed in first-mover experiments in which second-movers usually increase their contributions when first-movers set a good example (e.g. Rivas and Sutter, 2011). However, conformity may also lead to declines in cooperation if free-riding is observed frequently in the early stages of cooperative engagement (Carpenter, 2004). This is particularly relevant in novel contexts in which there are no strong norms about behaviour; whichever happens to be the most frequently observed behaviour early on may become established as the new norm due to conformity. For example, Carpenter (2004) found that declining cooperation levels observed in a public goods game could be explained by conformity to frequently observed instances of free-riding. Similar findings have been reported with regards to environmental behaviours, such as littering (Dur and Vollard, 2015; Keizer, Lindenberg and Steg, 2008; Cialdini et al. 1990).

In this paper, we aim to identify the relative influence of these broad mechanisms - communication/negotiation and observability - on cooperation around communal drinking water treatment plants in the state of Maharashtra in India, with a focus on individual behaviour. To do this we conducted an framed field experiment with residents of villages with water treatment

[1] In these studies, leaders are selected either randomly or via self-selection. Findings regarding cooperation of self-selected versus randomly-selected leaders are non-conclusive: some studies find voluntary leaders are more cooperative and motivate higher contributions (e.g. Rivas and Sutter, 2011), whilst others find no difference between leader types and respective follower contributions (e.g. Dannenberg, 2014).

plants. Framed field experiments are the same as conventional lab-based economic experiments except that they are conducted in the natural (field) environment of interest using subjects from the population of interest, with framing in terms of the good (Harrison and List, 2004). Increasing evidence suggests that the cultural and local context in which experiments are conducted have a significant impact on behaviour (Herrmann, Thoni and Gachter, 2008; Henrich et al., 2005; Anderies et al., 2011); by taking the lab to the field, these cultural and local factors are taken into account.

Using a public goods game (PGG) framed in terms of the communal management of village water treatment systems, we explore the potential for simple, cost-effective communication and observability mechanisms to increase cooperation in relation to the public good. The various mechanisms examined in this study were selected with policy-relevance in mind, as the Maharashtra state government is currently seeking approaches that may improve the likelihood of success of the water treatment plants that have been implemented in villages across the state over the past ten years[2].

To explore the influence of communication, we used two treatments: 1) a simple 'communication' treatment, whereby game participants could talk to each other openly before each round; 2) a 'negotiation' treatment, which was identical to the communication treatment except that participants were instructed to negotiate agreements with each other. We expect that, if groups engage in crafting agreements during open communication, then there will be similar cooperation levels between the two treatments. However, engagement in negotiation processes is socially determined (Kramer, 1995); it is therefore an empirical question whether individuals

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will spontaneously engage in negotiation. To the best of our knowledge, ours is the first experimental study to explicitly compare the influence of open communication versus instructed negotiation on cooperation; this will allow us to identify the extent to which the positive effects of communication on cooperation are determined by engagement in negotiation processes.

To examine the influence of observability, we used a ‘public observability’ treatment, which involved individual contributions by all group members being made public to the rest of the group in each round. Although many experimental studies have implemented observability treatments, none have disentangled the influence of reputation-seeking motives (of those whose actions are observable) versus conformity motives (of those who observe others) on behaviour. Hence, we also implemented a ‘leadership’ treatment, whereby the contribution of a ‘first-mover’ (the ‘leader’) was announced to the rest of the group in each round. This type of leadership does not confer the power to enact sanctions on leaders, but only permits them to lead by example. This set-up allows us to identify reputation-seeking behaviour of leaders in the absence of conformity to others’ contributions (although we acknowledge that conformity effects may occur with respect to previous leaders’ behaviour); and it allows us to identify conformity (of followers) to leader contributions in the absence of reputation effects. By examining these effects separately using our ‘leadership’ treatment, we hope to identify the relative influence of reputation concerns versus conformity on participants in the public observability treatment. Many lab-based experimental studies (e.g. Dannenberg, 2014; Figuieres et al, 2012; Potters et al., 2007) and a few field experiments (e.g. Jack and Recalde, 2014) have examined the effect of ‘leading-by-example’ on first-mover and later-mover behaviour. There are also a few field experiments that have examined the impact of leaders with sanctioning powers on cooperation (e.g. Gatiso and Vollan, 2017; Grossman and Baldassari, 2012). Our study adds to this literature by using the leadership treatment to shed light on the relative influence of reputation concerns versus conformity on publicly observable behaviour.

Our findings indicate that - contrary to findings in other studies that residents in India exhibit non-cooperative strategies in experimental games (Valerio and Cococcioni, 2015; Fehr, Hoff and Kshetramade, 2008) - baseline (i.e. anonymous) cooperation levels are quite high and similar to those found in other experimental studies, both in the lab (e.g. Zelmer, 2003) and in the field (e.g. Cardenas et al., 2011). Communication only had a weakly positive effect on cooperation; when participants were encouraged to negotiate, however, cooperation levels improved modestly compared to the baseline, suggesting that participants did not always initiate negotiation processes during open communication. Most strikingly, public observability had a significantly negative impact on cooperation. This was because co-operators responded to observable free-riding behaviour in early rounds of the game by rapidly decreasing their own contributions (similar to findings in Carpenter, 2004). Results from the leadership treatment confirm that observable behaviour in the early rounds of play are critical: we find that only the *first* appointed leader in each group has any influence on subsequent contributions by all players. Leadership however does not change leaders' choices relative to their baseline choices, suggesting that reputation benefits do not follow from contributing visibly to the public good.

Our study adds to the extensive literature exploring communication and observability effects (mainly in the lab), by taking these well-researched interventions into a context involving the subjects of interest, i.e. rural villagers faced with a real social dilemma. As found in d'Adda (2011), results from our field experiments differ from those of analogous experiments in the laboratory. This suggests that the local conditions in which experiments take place may have major influences on behaviour, which may limit the ability to generalize from the laboratory to real world settings (noted in Gneezy and Imas, 2016).

The remainder of this paper is organized as follows: Section 2 presents the contextual background to the study; Section 3 describes the field-experimental design and data collection; Section 4 presents the results, and Section 5 concludes the paper.

2. BACKGROUND

2.1 Water treatment plants in Maharashtra

In rural India, most households depend on communal sources of water, including boreholes (hand pumps), hand dug wells, protected springs, and surface water sources. When surface water levels are inadequate to meet the demand for water, drinking water is sourced from groundwater. This is the case in the state of Maharashtra, where surface water levels are below the national average for India (GOI, 2007). However, groundwater can often contain very high levels of naturally-occurring contaminants, and fluoride is a particular problem across India.

The continuous consumption of fluoride-contaminated water can lead to *fluorosis*, which can cause discoloration and mottling of teeth, deformation and decalcification of bones, hardening of joints and ligaments, and in extreme cases, disruption of the gastro-intestinal organs (Aoba and Fejerskov, 2002; Susheela et al., 1992). In addition, it has been found to interfere with hormonal function in children (Susheela, Bhatnagar and Vig, 2005). Unfortunately, fluorosis has no direct cure. In the absence of alternative drinking water sources, defluoridation treatment is the only mitigation approach.

According to a 2005 Maharashtra Development Report (Government of Maharashtra, 2005), as many as 1183 villages in the state of Maharashtra are impacted by fluoride. A more recent survey of chemical contamination in the Maharashtrian State[3] lists 179 villages as 100% impacted by fluoride contamination without any alternate drinking water sources within the community. Six districts alone (out of a total of 35) contain 133 of those villages, with the largest cluster (n=53) in Yavatmal, where this study is based.

In Maharashtra, when a village is found to be impacted by fluoride and water treatment selected as the course of action, the government contracts a private company to build and maintain a treatment facility for the first three to five years. After this initial period, the treatment facilities are then turned over to the communities. As of 2012, there were 36 villages with treatment plants in Yavatmal (although we could not obtain figures regarding which were functioning, and which were not).[4]

The practice of turning facilities over to communities is part of recent policy reforms[5]. These schemes involve the decentralization of planning and administration of water supply to the *Gram Panchayat* (village council) level (GOI, 2007). However, the success of these programs in

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[3] Details from 'Government of India and Government of Maharashtra Guidelines and Issues' presentation (unpublished) given in 2012 by Mr. S.A. Rode (Senior Engineer with the Maharashtra Jeevan Pradhikara (MJP) Water Supply and Sanitation Department Maharashtra).

[4] This information pertains to water treatment plants in Yavatmal that are operated and managed by Nagpur Aquatech Pvt Ltd under the 3-5 year subsidy. This private company operates almost all water treatment plants in the district. Data provided by the District of Yavatmal Engineer and the Government of Maharashtra officer in charge of treatment.

[5] Reforms include the Government of India Sector Reform Pilot Programme (1999), Swajaldhara (initiated in 2002), the Prime Minister's Gramodaya Yojana-Rural Drinking Water Programme (PMGY Programme) (initiated in 2000), and the World Bank-aided Jalswarajya Project (initiated in 2003).

Maharashtra is hindered by a lack of investment in capacity-building required to ensure the long-term operation and maintenance by local communities[6], an issue affecting water provision worldwide (RWSN, 2009; Schouten et al., 2011; Skinner, 2009). Most installed technologies in the region have either been abandoned or are experiencing problems due to insufficient support from the community[7]. This study aims to identify whether there is the potential for villagers to cooperate around the management of these public goods.

2.2 Study sites

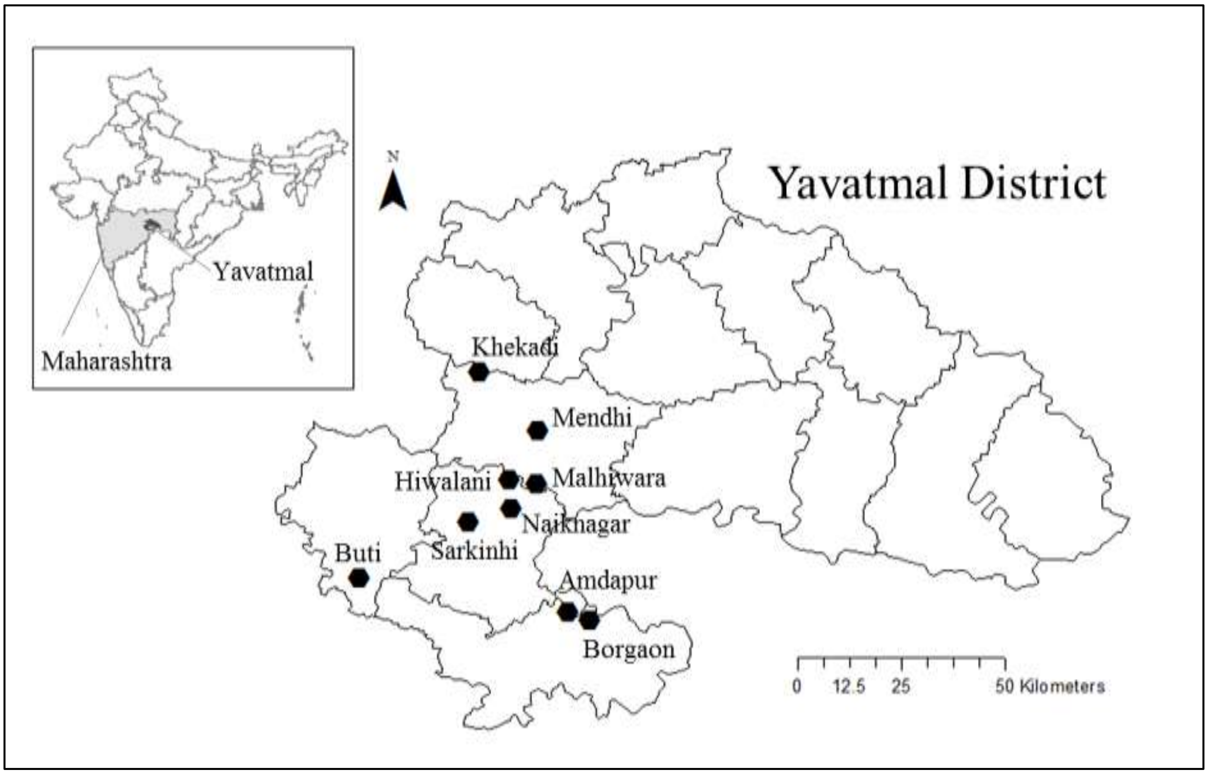
We conducted our study in nine villages (Figure 1) located in the district of Yavatmal, in the state of Maharashtra, which as noted, has one of the highest concentrations of villages with fluoride-contaminated water sources in the region. These villages had been previously surveyed six months earlier in November 2014 using a ‘contingent valuation’ (CV) survey[8] of willingness to pay for and contribute time towards the long-term operation and maintenance of the local water-treatment plant (for more details regarding the methods and results from this survey, see Alfredo and O’Garra (2019).

[6] Many of these concerns were expressed by researchers at NEERI during a scoping visit in Feb-March 2014

[7] Information from Mr. S.A. Rode (Senior Engineer with the Maharashtra Jeevan Pradhikara (MJP) Water Supply and Sanitation Department Maharashtra) from interviews conducted as part of the pilot research.

[8] ‘Contingent valuation’ (CV) is a survey-based methodology which aims to estimate economic values associated with non-marketed goods or services, such as clean air, water or ecosystem services. Respondents to a survey are presented with a hypothetical market in which they can pay for (or be compensated for) increases (or decreases) in the provision of a non-marketed good, such as environmental quality. See Bateman et al., (2002).

Figure 1. Location of field experiments in the district of Yavatmal, Maharashtra



The selected villages met the following criteria: (1) they contain identical technologies (electrocoagulation) for fluoride treatment, and (2) the water treatment facilities were in the process of being turned over from government-subsidized private companies to the communities between October-December 2014. These criteria ensured that technical differences in the operation and maintenance of the treatment plants, and differences associated with the timing of the transition from government-subsidized to community-owned water treatment systems were controlled for, and would not represent a source of variation in the study.

Results from the CV survey showed that respondents were willing to contribute significant amounts of time and/or money to support the long-term operation of these systems, suggesting that villagers have an incentive to cooperate in order to secure these benefits. Bearing this in mind, the present study was designed to identify key factors that might encourage cooperation, and thus increase the likelihood that rural communities have access to safe water.

3. METHODS AND MATERIALS

3.1 Experimental Design

To explore the potential for local villagers to collectively manage their water treatment plant, we designed our experiments around a standard linear public goods game (PGG), also known as a 'voluntary contribution mechanism' game. As noted, communal water systems require regular operation and maintenance to function. The treatment system can be accessed by anyone, so it is a 'non-excludable' resource. Due to potential congestion, water treatment systems are not fully non-rival and so may be considered 'impure public goods' (Kaul, Grunberg and Stern, 1999). However, results from the earlier CV survey (discussed briefly in Section 2(b)), as well as personal observations during fluoride sampling visits to the treatment plants in each village, suggests that congestion is not an issue in these villages. Thus, the PGG is suitable to address the fundamental social dilemma around the management of communal water treatment systems[9].

In the PGG, n subjects play the game in a group. Each individual i receives an identical endowment e at the beginning of the game and has to decide how much to contribute towards a group account, c_i , and how much to keep for herself. The sum of each individual's contributions

[9] Water treatment systems share the non-excludable, rival nature of common pool resources; however, a 'common pool resource' (CPR) game was not used for this study as we are not interested in the extraction of water (and CPRs typically focus on extraction decisions), but on the long-term operation and management of the water treatment plant. The communal treatment plant is the focus of this study.

to the group account is multiplied by a constant, α, and the resulting amount is distributed equally amongst all the group members. Each individual's earnings is determined by the following payoff function:

$$\pi_i = e - c_i + \alpha \sum_j c_j$$

In our study, we used a value of $\alpha = 0.4$, which is equivalent to a doubling of the total contribution to the public pot and equal distribution amongst all group members[10]. At all values of $\alpha < 1$, the individually optimal decision is to contribute nothing ($c_i = 0$). By doing so, a player not only keeps her entire endowment but also receives 1/nth of the total contributions made by the other players in the group. However, if all players choose to behave this way, they only earn $\pi_i = e$. The socially optimal contribution is for all players to contribute e, in which case players will earn $\pi_i = \alpha \sum_j e$.

The PGG reflects social dilemmas in which individual and social preferences work in opposition to each other. This experimental design is well-suited for the purpose of the present study, which is to identify the potential for community members to cooperate around a public good. The instructions of the game were framed in terms of the potential contribution towards the communal management and operation of a water treatment plant that would benefit everyone in the village[11]. The script read:

[10] Public goods games have been conducted using varying group sizes and MCPRs – a meta-analysis of linear PGGs conducted between 1998 and 2003 (Zelmer, 2003) indicates that the most commonly-used group sizes are 4 and 5, and the MCPR varies between 0.3 and 0.5. Hence our experimental set-up falls well within the bounds of standard linear PGGs.

[11] Although opinions about the acceptability of paying for water among the subject pool may differ (and hence represent a source of heterogeneity, as suggested by a reviewer), we do not consider this problematic, as our aim is to identify potential cooperation around communal water treatment plants in a context in which this is really

“Think of the group account as a water treatment plant that benefits everyone in your village - when you contribute money towards the operation and maintenance of the water treatment plant, the whole community can have access to clean and safe drinking water.”

We ran the experiments with n=5 subjects per group, and they played the game over twenty rounds. Group membership remained fixed over the entire experiment. At the beginning of each round, each player received an endowment of e_i =10 rupees. The maximum possible earnings per individual, framed in terms of the benefits from safe water from the managed treatment plant, came to Rs20 per game, or Rs400 per experiment[12].

During the game, players signalled their contribution decision by circling the number of rupees (between 0 and 10) that they wished to contribute towards the treatment plant (see Supplementary Material for Experimental Script and Decision Sheets used). Once they had made their choices, players placed their decision sheets inside a folder and handed them to the instructor. An assistant then entered the contribution decisions into a mobile survey interface on a smartphone[13], which recorded the data and calculated the group contributions and individual earnings. Total group contributions and individual earnings from the group account were written on a large whiteboard at the end of each round. Meanwhile, participants were guided by the

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happening. That said, despite the general conception that rural villagers are not willing to pay for water, our previous contingent valuation study finds that villagers are willing to pay significant amounts, and most zero contributors explain their zero contributions in terms of affordability rather than a refusal to pay. Our findings are supported by other studies in India on willingness to pay for water (e.g. Majumdar and Gupta, 2009; Roy et al, 2004).

[12] The average daily wage in these villages is about Rs200 per day, equivalent to about $3/day. We did not communicate the levels of potential earnings at any point during recruitment, or before playing the game.

[13] We used Formhub (https//www.formhub.org), a mobile data collection tool developed by Modi Research Group at Columbia University.

instructor in the completion of their 'calculation sheets', in which they would record their contributions and calculate their earnings in that round[14]. Participants with reading or writing difficulties were assisted by the instructor or the assistant. Communication between group members was not permitted during the baseline rounds (see below). Furthermore, to ensure that contributions were private, players were instructed to sit back-to-back whilst playing the game.

We implemented the experiment as a mixed within- and between- subjects design. All groups played ten initial **baseline rounds**, in which players made their contribution choices as described above - simultaneously and anonymously - followed by ten more rounds in one of four different treatments. The purpose of a common baseline is to establish patterns of individual behaviour in the absence of social interactions[15].

After the baseline rounds, each group received a different treatment intended to explore the potential for specific social mechanisms around communication and observability to increase cooperation. As in the baseline rounds however, all decisions were made individually and privately unless otherwise stipulated by the treatment instructions. The treatments are described below:

Face-to-face communication: the communication treatment implemented here is modelled on that implemented in Cardenas et al. (2011) in which participants were allowed to openly

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[14] Our motivation for encouraging participants to calculate their earnings themselves was based on feedback from a pre-pilot study carried out at Columbia University (n=5), in which it was found that participants lost interest whilst waiting for the assistant to calculate the earnings. The 'calculation sheet' (also used in Cardenas et al. 2011) was piloted in the field in Dongargaon (n=20) and found to be a very effective way of keeping participants engaged in the experiment through active involvement in calculating their own earnings.

[15] We note that the main focus in this paper is on comparing treatments to each other; however, we acknowledge that collection of baseline data in rounds 11-20 (i.e. without treatment) would have allowed us to make more robust comparisons between treatments and baseline behaviour.

communicate with each other for two minutes before each round, but were not permitted to make promises about side-payments with respect to the game. Our intention in implementing this treatment was to reflect the day-to-day reality in the villages in which people might talk to each other informally regarding the village public goods.

Negotiation: this treatment is identical to the face-to-face communication treatment except that participants were *actively* instructed to negotiate and try to reach an agreement regarding their contribution decisions in the two minutes given for discussion before each round. The script specifically read:

> "*For the first two minutes of each game you should negotiate with each other regarding how much everyone should give for the following game of play. To do this, everyone will turn towards the middle of the room to face one another. You should only discuss the game and contribution amounts.*"

Any agreements are 'cheap talk' and have no influence on player payoffs. However previous empirical evidence indicates that voiced agreements increase cooperation even if they are non-binding (Cardenas et al. 2011; Orbell et al., 1988). Hence, we anticipate that, by actively instructing participants to negotiate agreements, this will increase the likelihood of voiced agreements *vis a vis* the simple communication treatment, and hence, this will increase cooperation. Our intention is to explore whether unstructured communication is sufficient to motivate the crafting of agreements, compared to the explicit requirement to negotiate agreements.

Public observability: the threat of public disclosure of one's actions is often found to be a very effective motivator of pro-social behaviour, and the evidence suggests this may be mostly due to a desire for social approval (Arbak and Villeval, 2013; Andreoni and Petrie, 2004). At the beginning of this treatment, the instructor explained to the group that individual contributions were to be made public at the end of each round. Players made their decisions individually and in private, and when all decisions had been made, the decision sheets were collected by the instructor who proceeded to write down the individual contributions on a whiteboard, in the same order in which participants were seated in the room to ensure association between contribution and subject identity. This fact was iterated by the instructor when writing down the individual contributions. We note that observability without communication (as implemented here) may induce behaviour change through guilt and shame, which can be effective enforcers of cooperative behaviour without the need for communication or the expression of disapproval, such as found in Lopez et al., (2012), D'Adda (2011) and Rege and Telle (2004).

Leading-by-example: in our leadership treatment, the leader was selected by the instructor. The process of leader selection was not implemented as a random process; the instructor started by selecting the participant seated at the farthest left of the room to be the first-mover and then selected subsequent leaders by moving left-to-right across the room. This order was repeated once again so that each participant acted as first-mover twice. The leader was instructed to circle his or her contribution decision on his or her decision sheet. All other players were instructed to wait for the first-mover to make a decision. The leader then publicly announced his or her contribution amount to the other players, who proceeded to make their own contributions in private. Based on the literature, we expect that leader's contributions will be greater than second-mover contributions (e.g. Figuieres et al, 2012) due to reputational concerns mainly, and that good examples will motivate greater contributions amongst second-movers (e.g. Rivas and Sutter, 2011).

Finally, we note that although our motivation for this study is to explore cooperation around communal water treatment systems, results from this study may also pertain to a range of public goods in the villages, such as roads, schools and irrigation wells. However, the main social dilemma facing these villages at the time of the experiments concerned communal management of the water treatment plants, which had just been transferred to community-ownership. Thus, we consider that - in terms of subject pool, location, framing and timing - the link between the experiment and communal water management is quite robust. Findings from this study may guide decisions about the types of interventions that might encourage (or not) collective management around the water treatment plant, and the potential for villagers to manage this resource collectively. Given the high rate of failure of these plants in the state of Maharashtra (noted in Section 2.1), this is an important question that needs addressing.

3.2 Recruitment and Data Collection

We conducted the experiments in nine villages between March 19th and April 9th 2015. Villagers were recruited the day before by an advance team who, after gaining approval from the village council ('Gram Panchayat'), walked around the village inviting people to the experiment and handing out fliers about the study. Participants were informed that the study was about 'community resources' and they would be compensated Rs150 for participating (one day's wage is about Rs200 in these villages, based on results from baseline study; see [author details omitted for review]).

Table 1. Summary of Data Collection

Treatment	No. of groups	No. of players	No. observations (Rounds 1-20)
Communication	13	65	1300
Public	6	30	600
Negotiation	6	30	600
Lead-by-example	6	30	600
Total	31	155	3100

A total of 155 villagers participated in the experiments, resulting in 31 groups of five players (Table 1). Due to the large number of treatments in this study[16], and our randomization of enumerators amongst the treatments (which meant they had to be thoroughly familiar with every treatment), we opted to divide the treatments into two sets to maximize optimal delivery of the instructions, and hence, obtain better data. The first round of experiments thus started with the first subset of treatments (communication, public), and after additional training, the second subset of treatments was collected (communication, negotiation, leadership). Thus, treatments were not strictly randomized across villages, although the villages were indeed randomly assigned to the early (n=6 villages) versus later (n=3 villages) treatment subsets[17]. Each experimental session involved 2-3 groups. Five villages had only one experimental session; four

[16] We also ran two psychological 'anticipated emotion' treatments concurrently with the social information treatments to address a separate research question on the importance of emotions on cooperative behaviour. These are not reported in the present paper.

[17] Originally, five villages were assigned to the second subset, but two villages had to be dropped due to fact that most of the adults were engaged in agricultural labour in neighbouring farms at the time and hence were unavailable for participation in the experiments. We note that the villages included in this study (including the two that were dropped were the same as those in which we conducted the CV survey).

villages[18] had two sessions, which were conducted back-to-back to minimize interaction between participants from the two sessions.

The experiment was piloted in the field (n=20) which allowed us to adjust the experimental instructions to maximize learning and practice during the game. Instructions were provided by trained instructors in the local language, Marathi[19] (see Supplementary Material for Experimental Instructions), and participants were required to play ten practice rounds before the final experiment.

The experiments were conducted in village schools (after school was out). Participants were randomly assigned to treatments using coloured 'participant cards' that were handed out randomly. Colours indicated different treatments, which were assigned to different classrooms, thus ensuring there was no communication or interaction between different treatment groups. Members of the same household were not allowed to be in the same treatment group. All groups were isolated from each other in different rooms and could not communicate or interact with each other at any point during the experiment. Hence, there was no possibility of contamination between groups. Before starting, all participants were informed about their rights, and verbal consent was obtained as per Columbia University's ethics in human subjects' research requirements.

At the end of the experiment, players were instructed to remain quietly seated in the classroom whilst they were called out one-by-one to complete a short exit survey. The survey was conducted face-to-face by the assistant or main instructor, and data entered directly into a survey

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[18] Specifically Sarkinhi, Borgaon, Hiwalani and Naiknagar.

[19] The instructions were scripted in English and translated into Marathi by a verified translator. The translation was further verified by two additional fluent English-Marathi speakers to ensure correctness of the translation.

tool on individual smartphones. The survey identified basic respondent socio-economic and demographic characteristics, as well as involvement in community affairs and membership of the village council. After players had completed the exit survey, they were directed to a desk where they were paid. To receive their payments, participants had to hand in their decision-sheets and participant card, and were asked to provide their full name on a payment receipt.

The total amount earned by each participant depends on how the participant and other members of the group played the game. The social optimum is achieved if every player contributes their full endowment of Rs200 (i.e. Rs10 for each of 20 rounds), resulting in overall earnings of Rs400 per participant. The Nash equilibrium of zero contributions results in overall earnings of Rs200 - half that achieved under the social optimum. On average, total earnings ranged between Rs175 - Rs396, with a mean of Rs282.92 (representing an average earning of Rs14 per round). This amount lays almost directly half-way between the Nash equilibrium and the socially optimum outcome, indicating the presence of cooperation. Including the show-up fee of Rs150, villagers made on average Rs430. The total time taken for each experiment was about three hours.

3.3 Sample Description

Table 2 presents summary sample characteristics for the whole sample. We note that education levels are quite high, with almost half the sample reporting a minimum grade 11 education[20].

[20] Although we do not have education statistics for the Yavatmal population, we do have education statistics from our baseline CV survey sample, according to which only 11% of the survey sample had a minimum grade 11 education. This suggests possible self-selection in our experimental sample, whereby the more educated villagers might have felt more confident to participate in a 'study about 'community resources'' (the language used to recruit participants). We do not consider this potential self-selection a problem; we consider that our experimental

Only 7.74% (n=12) of participants are classified 'high caste', with the rest belonging to 'low caste' groups. Participant characteristics were compared across treatment groups to assess balance of treatments (see Table S.1. in Supplementary Material). Test results indicate that participants were very similar across treatments; given the small sample sizes in this study, we consider the treatment groups to be sufficiently balanced in terms of participant characteristics.

Table 2. Sample and household characteristics[a]

Variable	**Sample statistics (n=155)**
Respondent characteristics	
Female (%)	47.1 (0.04)
Age [b]	32.4 (0.93)
% Minimum higher secondary education (grades 11-12)	45.8 (0.04)
% respondents Brahmin ('high caste')	7.74 (0.02)
% respondents answered a survey about preferences for water 6 months earlier	38.1 (0.04)
Household characteristics	
Household size (mean no. of people)	6.87 (0.23)
Gross monthly household income (mean Indian Rupees)[b]	6,161 (646.33)

Figures in parentheses () are standard errors

[a] Tests (Supplementary Material Table S.1) confirm that subsample characteristics are balanced across treatments

[b] Income and age taken as mid-interval of categories

sample suitably reflects the characteristics of key decision-makers in rural villages, and hence, the individuals most likely to initiate collective action with regards to the public good.

4. RESULTS

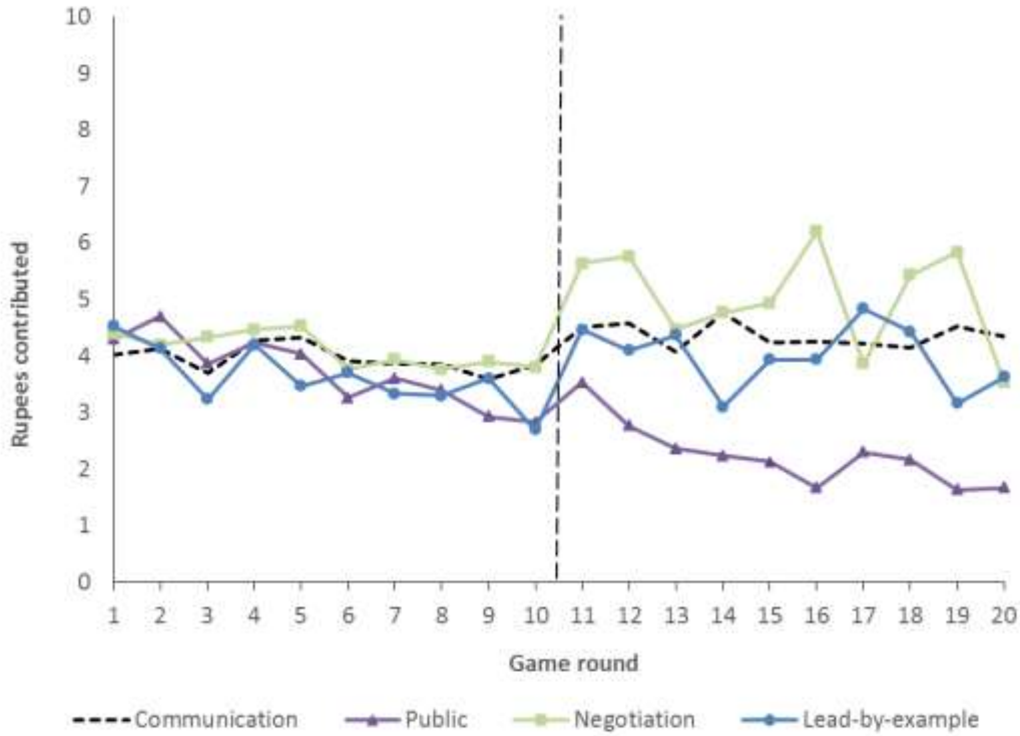
4.1 Overview of experimental data (group level)

Figure 2 presents time-series of mean group contributions towards the public good for the different treatments. Overall, mean contributions start at between 40-50% of the endowment which conforms to the common finding in repeated PGGs that contribution levels typically start at between 40% - 60% of the endowment (Cardenas and Ostrom, 2004; Isaac and Walker, 1988).

To verify whether baseline contributions are from the same distribution, we compare baseline contributions between treatment groups using a Kruskal-Wallis (KW) test, a rank-based nonparametric test used to compare the medians of two or more groups, and a Wilks Lambda test (WL), which is the parametric counterpart. Results of both tests, with treatment averages over all 10 periods as independent observations, confirm that baseline contributions between treatments are not statistically different (KW: Chi^2 =0.958, d.f=3, p=0.8031; WL: F-stat=0.9855; d.f.=3, p=0.9401). As an additional test, we conducted a random effects panel regression model on individual contributions (not reported in this paper); results confirm that there is no statistically significant difference between baseline contributions across individuals in different treatment groups[21]. This confirms that the baseline condition was identical across treatments, and that our treatment samples are not systematically biased in any way.

[21] The regression model included only baseline data (contributions in rounds 1 to 10) and dummies for each of the treatments (in rounds 11-20). A joint hypothesis test confirms that the coefficients on the treatment dummies are equal to zero (Chi2 test p=0.7976)

Figure 2. Mean contributions over repeated play



After implementation of the treatments in the separate groups, there is a 'jump' in contributions at round 11 to about 47% of the endowment (averaged over all treatments); a two-tailed t-test comparing overall contributions between round 10 and 11 at the group level indicates the increase is statistically significant ($p<0.001$). This is known as the 'restart effect' (Cookson, 2000), and refers to the observation that contribution levels in PGGs persistently return to about 50% of the initial endowment after a break between rounds of play, independent of treatment effects. Thus, the increase observed in round 11 is at least partly a result of the restart effect.

Over the following nine treatment rounds, public observability has the most pronounced effect on group contributions, which start at Rs3.53 in round 11 and *decrease* steadily to Rs1.67 in round 20. A Wilcoxon matched pairs signed-rank test comparing mean baseline contributions versus treatment round contributions indicate that this statistically significant (p=0.0277).The negotiation treatment on the other hand has a visibly positive - albeit somewhat jagged - effect on cooperation, with mean contributions over all treatment rounds increasing by about 24% (Rs0.96) compared to overall baseline contributions of Rs4.04; however, the Wilcoxon signed-rank test indicates that this change is not significant at the group level (p=0.1073).

To examine the relative impact of the different treatments on group contributions levels, we compared average group contributions in rounds 1-10 and rounds 11-20 using non-parametric Mann-Whitney tests. Results, shown Table 3, indicate that the public observability treatment resulted in significantly lower group contributions compared to the negotiation and leadership treatments (a weak difference is also found with regards to communication). There is no significant difference in mean group contributions between the other treatments.

Table 3. Comparing Mean Contributions in Rounds 11-20 by Treatment (group level)

Tests of difference between mean group contributions by treatment	Difference	Mann-Whitney test (p-values)
H_0: Communication – negotiation = 0	-0.68	0.483
H_0: Communication - public = 0	+2.12	0.087^{*}
H_0: Communication - leadership = 0	+0.37	1.000
H_0: Public - negotiation = 0	-2.80	0.010^{***}
H_0: Public - leadership = 0	-1.75	0.016^{**}
H_0: Negotiation - leadership = 0	+1.05	0.109

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.2 Regression Analysis

In this section we examine treatment effects on contributions at the individual level. Although we recognise that the treatments in this study were implemented at the group level, *decisions were made at the individual level*, and so we consider it appropriate to focus on the effects of the treatments on these individual decisions. This complements the group-level statistical analyses presented in the previous section.

Given that we have multiple observations per individual participant, we analyse the data as a panel. Table 4 presents results from the fixed effects panel regressions, which take into account within-subject variation as a result of the different treatments. This model controls for all time-invariant individual-specific characteristics, such as age, education and income (Wooldridge, 2009). Individual contributions during baseline rounds (1 to 10) are the control against which we compare all treatment contributions (similar to the modelling approach in Travers et al., 2011). The assumption is therefore that behaviour in treatment rounds would be similar to that in baseline rounds had the treatments not been implemented. This is broadly supported by findings in the literature regarding cooperation behaviour before and after restarting play (e.g. Cookson, 2000; Croson, Fatas and Neugebauer, 2005), and is also the assumption used in other lab-in-the-field experiments using treatments after baseline rounds (e.g. Narloch et al, 2012; Cardenas et al, 2011).

Model (1) in Table 4 identifies the main effects of the treatments on individual contributions. All the treatment variables are dummy variables, which take a value of one for contributions made during treatment (i.e. between rounds 11-20), and zero if otherwise. The constant in this regression can be interpreted as the mean contribution over all rounds under baseline conditions. Model (2) incorporates controls for learning effects from repeated play via the inclusion of time dummies. We also anticipate that contributions at time *t* will be influenced by contributions made by everyone else in the previous round, so we include a lagged variable representing the sum of contributions by the rest of the group in *t-1*. Table 5 presents results of pairwise tests of the equality of the treatment effects for both model specifications.

As a robustness exercise, we also conducted separate regressions on the subsets of villages to which different treatments were assigned (as described in Section 4.2): subset 1 included communication and public observability treatments only; subset 2 included communication, negotiation and leadership treatments. Results from these extra regressions (see Table S.2 in the Supplementary Material) broadly confirm findings reported in Table 4 and Table 5.

Table 4. Linear fixed effects panel regressions on individual contributions [a]

	(1)		(2)	
	Coef.	Std. Err.	Coef.	Std. Err.
Treatments				
Communication	0.41	(0.37)	0.62^{*}	(0.36)
Negotiation	0.93^{*}	(0.54)	1.05^{**}	(0.51)
Public observability	-1.47^{***}	(0.27)	-0.85^{***}	(0.30)
Leadership	0.37	(0.36)	0.70	(0.44)
Controls				
Rest of group's contribution at *t-1*			0.05^{***}	(0.01)
Time dummies (T_1=reference)			included	
Constant	3.87^{***}	(0.58)	3.44^{***}	(0.25)
N. obs.	3100		2945 [b]	
N. individuals (panels)	155		155	
F-test	9.94^{***}		22.05^{***}	
F-test joint hypothesis test of equality of treatment coefficients	10.53^{***}		7.15^{***}	

[a] Fixed effects regression with clustering of groups; robust standard errors in parentheses
[b] Round 1 is not included in model (2) as there are no previous contributions by others
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5. F- Tests of Equality of Treatment Effects (p-values)

Hypotheses being tested	Model (1)	Model (2)
H_0: Communication=negotiation	0.43	0.42
H_0: Communication=public	0.00^{***}	0.00^{***}
H_0: Communication=leadership	0.94	0.86
H_0: Public=negotiation	0.00^{***}	0.00^{***}
H_0: Public=leadership	0.00^{***}	0.00^{***}
H_0: Negotiation=leadership	0.39	0.52

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.3 Analysing Treatment Effects

Results in Model (1) in Table 4 indicate that only *negotiation* has a significantly positive effect on contributions to the public good compared to the baseline; *public observability* has a significantly negative effect on contributions, reflecting what is visually apparent in Figure 2. These findings are confirmed in model (2) where we take into account learning effects and contributions of others in previous rounds. Results in model (2) show that past contributions made by the rest of the group have a positive effect on contributions in the present round. This is a standard finding across public goods games (e.g. Cardenas et al, 2011; Croson et al., 2005), suggestive of either positive reciprocity or conformity, both of which are sources of conditional cooperation (Bardsley and Sausgruber, 2005). We also find that contributions in the communication treatment are significantly higher compared to those in the baseline rounds in model (2). Pairwise comparisons of treatment coefficients in Table 5 however suggest that there are no significant differences between communication, negotiation and leadership treatment effects. Only public observability has a significantly different effect compared to the other treatments.

4.3.1 Communication and Negotiation

We present and compare results of the communication and negotiation treatments together here, as they were virtually identical bar the explicit encouragement to negotiate agreements in the negotiation treatment. Results in Model (2) in Table 4 show that both negotiation and communication have a positive effect on contributions when compared to the baseline rounds, although contributions under negotiation were about 60% higher than in the open communication treatment. As noted in the Introduction, the success of communication in enhancing cooperation in small rural villages - in which players tend to know each other and have established notions of their social distance from the other players – may depend on whether individuals discuss the task and negotiate agreements (Cardenas et al. 2011; Orbell et al., 1988). If engagement in negotiation processes is a social practice/norm in these villages, then we would expect cooperation to be similar in both groups. Although the distribution of choices over all treatment rounds suggests that there is a difference between treatments (see Figure S.3 in the Supplementary Material), test results in Table 5 suggest that the apparent difference between the effect of communication and negotiation on cooperation is not statistically significant in any of the models. Hence, results show that encouraging individuals to negotiate with each other does not enhance cooperation beyond the basic impact of open communication. Unfortunately, we did not collect data on the actual discussions that players engaged in during either communication or negotiation treatment, so we cannot verify whether the lack of difference is due to the fact that the discussions were similar in content.

4.3.2 Leadership

Leadership has no effect on mean contributions compared to the baseline. Summary statistics indicate that first-movers failed to set an example: they contributed an average Rs3.98 per round, compared to second-movers who contributed an average Rs4 per round. Even if we consider only the first few rounds (11 and 12) after the leadership treatment was implemented, there is still no significant difference between leaders and followers. A fixed effects regression on the leadership treatment subsample not reported in this paper confirms that leader contributions had no effect on follower contributions overall.

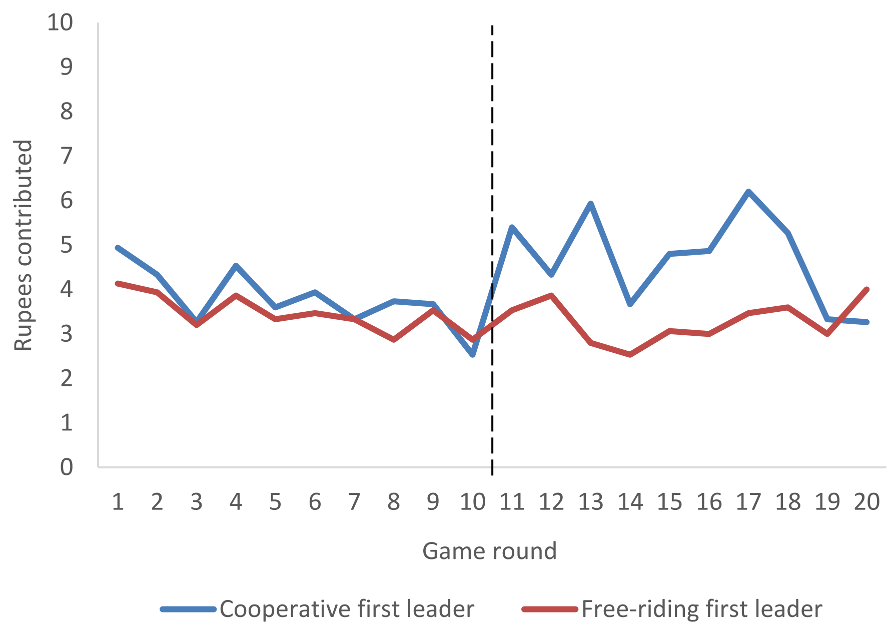
The lack of influence of our treatment on leader contributions contrasts with findings in the literature (reviewed in Section 2) which show that players consistently give higher contributions when appointed as leaders. The usual reason for higher leader contributions is reputational – by contributing more, they reinforce their reputation as a cooperator (Arbak and Villeval, 2013; Andreoni and Petrie, 2004). Assuming that ‘leaders’ in our games care about their reputation and securing social approval, then their muted contributions suggest that contributing generously to the public good is not reputation-enhancing. This may be simply because the public good is not valued, or because visibly high contributions do not confer reputation benefits – either because they are construed as status-seeking (Benabou and Tirole, 2005) or because non-cooperation may be the social norm (e.g. such as observed by Robert Putnam in his study about social capital in Southern Italy (Putnam, 1993). However, this is speculative, and we cannot confirm with our data why first-movers did not ‘rise to the challenge’ and set an unambiguously positive example.

Another way of analysing the impact of leadership on leaders is to compare contributions of players when in the role of leader with their contributions in baseline. Overall, we find that players contribute marginally more when in the role of leader than in baseline (Rs3.98 versus Rs3.62) but the difference is not statistically significant (paired t-test p=0.3001). We also compare contributions of the first appointed leader (in round 11) with the player's first round contribution in baseline. This will provide an indication of whether leadership led to enhanced willingness to cooperate independent of other players' contributions (as in Gatiso and Vollan, 2017). We find only two players increase their contributions after being appointed leaders, whilst three leaders reduce their contributions and one registers no change. Although we cannot make any conclusions based on six players who were first appointed the role of leader, leadership has no discernible effect on leaders.

In terms of effects on followers, despite finding leader contributions have no effect overall on followers, it is possible that only the first appointed leader (in round 11) had an influence on contributions by subsequent players - leaders and followers alike - who anchor their contributions to the action of this first-mover[22]. To identify whether this is the case, we separate the data depending on whether the leader is classed as a 'free-rider' or not. For this purpose, we classify first leaders who make contributions of Rs2 or less (25% of the endowment) in round 11 as 'free-riding' first leaders (n=3) and all others as 'cooperating' first leaders (n=3). Results are shown in Figure 3.

[22] We thank an anonymous reviewer for making this suggestion.

Figure 3. Impact of First Appointed Leader's Decision on Cooperation



As we can observe, cooperative behaviour by the first appointed leader in each group motivates higher contributions in subsequent rounds, suggesting path-dependency based on initial leader choices, although this effect disappears quite abruptly in the last two rounds of play. A Kolmogorov-Smirnov test of the equality of distributions confirms that these distributions are significantly different ($p=0.015$). Thus, this analysis suggests that – even though leadership has no influence on the leader him or herself, as discussed above - the behaviour of the first appointed leader in each group influences subsequent contributions by all players. Thus, cooperative first leaders motivate more cooperation that free-riding first leaders.

4.3.3 Public observability

Results in Table 4 confirm what is visually apparent in Figure 2: *public observability* has a very pronounced negative impact on contributions. The process of making individual contributions known to the rest of the group decreased contributions by an average of about Rs0.85 per player, representing a 21% decrease in mean contributions relative to the mean baseline contribution of Rs4.05 (using results from Model 2). This is a very large impact, and one that we did not anticipate based on the general findings in the literature.

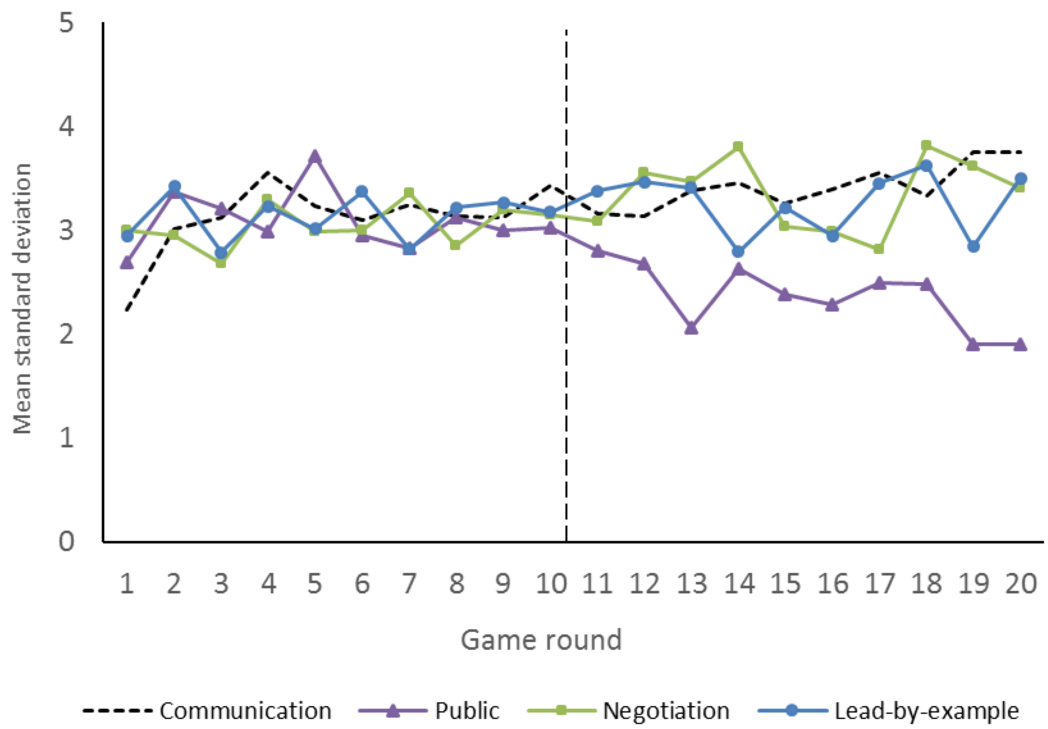
There are various possible explanations for this downwards decline. Firstly, perhaps cooperation in these villages does not enhance one's reputation, as suggested by the leadership treatment. This might occur either because cooperation is not a social norm, or because the social norm is in fact non-cooperation. If observability did not impel first-movers in the leadership treatment to exhibit higher cooperation levels for the usual reasons of reputation or status, as noted in the previous section, then a similar motivation (or lack of) would have affected participants in the public observability treatment. This is similar to the explanation proposed by Dufwenberg and Muren (2006) who found that observability decreased contributions in a dictator game involving economics students. Their explanation was that the participants were conforming to the economic stereotype of selfishness to gain social approval from fellow students, even though in private they acted more generously. Thus, observability could in fact depress cooperation via conformity to a non-cooperative social norm. To test whether this is the case, we compare first round contributions in baseline and treatment to identify whether the *threat* of observability affects contributions, independent of other players' actions. If we find that first round contributions under observability are significantly lower than first round contributions in baseline, this might suggest that non-cooperation is a social norm or expectation. However, we find no significant difference between first round contributions (paired t-test p=0.1165; Wilcoxon signed rank test p=0.0933), suggesting there are no strong social norms regarding cooperation behaviour. Thus, the downward decline in contributions in the observability treatment cannot be explained by non-cooperative social norms. This is confirmed by the fact that leaders (in the leadership treatment) gave a consistent amount of about 40% of the endowment in all rounds of treatment; if non-cooperation was the norm then we would have expected to see no cooperation at all, or at least, declining cooperation levels such as those in the public observability treatment.

Another possible explanation for the downward trend in contributions, is that co-operators conform to individual free-riding behaviour observed in early rounds of play. This was observed by Carpenter (2004) with regards to public observability effects in a PGG in the lab. As noted by Carpenter (2004, p400), observability allows players to identify whether there is a consensus around specific contribution amounts. This may motivate greater levels of conformity compared to information about aggregate group contributions (provided in all games, as noted in Section 4.2), which provides no information about specific contributions made by others. Hence, if individuals observe significantly high levels of free-riding in early rounds of play, then they may conform to this behaviour, which in turn would lead to decreasing contributions over rounds of play as each player conform to free-riding behaviour. We explore this possibility in greater depth in Section 4.3.4.

4.3.4 Further Analysis of Public Observability Results

To examine whether observation of individual instances of free-riding induces co-operators to lower their own contributions, we analyse the data in greater depth using procedures used in Carpenter (2004). Firstly, we examine whether the variance in behaviour declines faster under public observability compared to the baseline. Faster declines in variance may indicate increasing conformity as contributions move towards the most frequently-observed contributions. Figure 4 presents mean standard deviations per round of play for the treatments.

Figure 4. Mean standard deviation on contributions per round



The figure shows fairly high and stable standard deviations during baseline rounds for all treatments, which then decrease during treatment rounds only for the public observability treatment. Tests confirm that average standard deviations of the different treatments are not significantly different between baseline rounds (KW: Chi2 = 0.992, d.f=3, p=0.8031; WL: F-stat= 0.9946; d.f.=5, p=0.8453), but are significantly different during treatment rounds (KW: Chi2 = 20.199, d.f=3, p=0.0002; WL: F-stat= 0.8717; d.f.=5, p=0.0001). Furthermore, two-tailed t-tests on average standard deviations show that the observability treatment has significantly different standard deviations to all other treatments (all p-values<0.001), whereas there is no significant difference between the average standard deviation of the other treatments. Thus, decreasing variance in contributions in the observability treatment suggests that conformity to frequently-observed free-riding may at least partly explain our results.

Additionally, we conduct an econometric analysis to examine whether frequently-observed free-riding explains the decline in cooperation. To do this, firstly we identify levels of free-riding in all treatments using three different contribution cut-offs: contributions of zero rupees, contributions of Rs2 or less (≤ 1/5 of the endowment) and contributions of less than Rs5 (<1/2 of the endowment)[23]. Using these three different cut-offs, we ran fixed effects panel regressions (Table 6 below) to examine the impact of the number of free-riders in the previous round of play

[23] We use more stringent cut-offs than those used in Carpenter (2004), who uses three cut-offs of ≤1/5, ≤1/3 and ≤1/2 of the endowment. We instead use a zero-contribution cut-off as our most stringent definition, as we expect the strongest effects with regards to zero contributions. We also opt to use a cut-off of <1/2 of the endowment, as opposed to ≤1/2 of the endowment, as it is debatable whether contributions of half of the endowment would be viewed as free-riding by players. We prefer to use cut-offs that in no way can be interpreted as cooperation.

(*t-1)* on individual contributions in round *t*. Regressions also control for the positive effect of previous group contributions in time *t-1* (as used in the main regressions in Table 4).

Table 6. Linear fixed effects panel regression exploring influence of observed free-riding on cooperation

	Free-rider cut-offs					
	Contribute Rs0		Contribute ≤Rs2		Contribute <Rs5	
	Coef	Std. Err.	Coef	Std. Err.	Coef	Std. Err.
(Communication x no. free-riders)$_{t-1}$	-0.31	(0.27)	-0.13	(0.17)	-0.05	(0.10)
(Negotiation x no. free-riders)$_{t-1}$	0.49	(0.78)	-0.21	(0.57)	0.28	(0.97)
(Public observability x no. free-riders)$_{t-1}$	-0.77**	(0.34)	-0.64*	(0.35)	-0.41	(0.26)
(Leadership x no. free-riders)$_{t-1}$	-0.73*	(0.40)	-0.52	(0.32)	-0.43	(0.30)
Rest of group's contribution at *t-1*	0.03**	(0.01)	0.03**	(0.01)	0.03**	(0.01)
Constant	3.92***	(0.33)	4.00***	(0.32)	3.95***	(0.30)
N. obs	1395		1395		1395	
F-test	3.98***		2.59**		2.64**	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Results show that the number of free-riders in the previous round has no effect on individual contributions in either of the communication or negotiation treatments. In contrast, individual contributions in the public observability treatment are significantly negatively impacted by the number of free-riders in the previous round - although only for the more stringent definitions of 'free-rider' (those that contribute Rs0 or ≤Rs2 in the previous round). We also observe that the number of zero contribution free-riders negatively impacts contributions in the leadership treatment. For the looser definition of free-rider (those who contribute <Rs5), we do not find such any significant effect. This suggests that it is only when individuals observe very low contributions to the public pot, do they respond by lowering their own contributions significantly.

Overall, results indicate that observing very low individual contributions in the public observability treatment (and to a lesser extent, in the leadership treatment) induces other players to reduce their own contributions. The explanation proposed by Carpenter (2004) is that this occurs due to conformity. However, it is also possible that what is occurring here is in fact ‘negative reciprocity’, whereby cooperators retaliate against free-riders by decreasing their contributions to the public good[24]. This explanation is proposed by Noussair and Tucker (2007) who also find that public observability decreases contributions, although they do not confirm whether negative reciprocity indeed explains their findings. Only a few studies have examined the relative influence of conformity versus reciprocity on cooperation (e.g. Velez et al. 2009; Bardsley and Sausgruber, 2005) and results are mixed. Further research is warranted to explore how observation of free-riding behaviour affects cooperative behaviours, and the mechanisms (i.e. reciprocity versus conformity) through which these effects occur. Nonetheless, regardless of whether conformity or negative reciprocity explains our results, the key finding is that the threat of being observed and shamed has very little effect on cooperative behaviour (noted in Section 4.3.3) whereas observing others free-riding in early rounds of play induces others to decrease their cooperation levels.

Finally, in a similar vein to the leadership treatment, we explore the effect of the first round of observable contributions (in round 11) on all subsequent contributions in a group. This will help ascertain to what extent the dynamic of the first round influences all subsequent cooperation levels. To do this, we separate the data depending on the number of ‘free-riders’ observed in round 11. For this purpose, we classify players who make contributions of Rs2 or less (25% of

[24] We thank an anonymous reviewer for making this suggestion.

the endowment) in round 11 as ‘free-riding’ and all others as ‘cooperating’. Results are shown in Figure 5.

Figure 5. Impact of Free-Riding in First round of Treatment on Cooperation

Figure 5 shows that cooperation levels in rounds 11-20 are influenced by the presence of free-riders in a group in round 11, although the number of free-riders has no discernible influence. Kolmogorov Smirnov tests indicate that the distribution of contributions in groups with zero free-riders in round 11 is significantly different from the distributions with two and three free-riders in round 11 ($p<0.001$ in both cases), and there is no significant difference between the two free-rider distributions ($p=0.852$). Analysis also shows that, although baseline distributions are not significantly different between free-rider distributions, or between the distribution with zero and two free-riders, they are significantly different between groups with no free-riders and three free-riders (p=0.006). Hence, the influence of free-riders on overall contributions in the baseline rounds is clear. Making these individual choices observable in round 11 appears to accelerate the decline in cooperation that is apparent in the baseline rounds.

4.3.5 Type A and Type M Errors

Given that this study is based on a small sample size, we assess the probability of making a sign error ("Type S error") or exaggerating the effect size ("Type M (magnitude) error") as proposed by Gelman and Carlin (2014)[25]. We do this using treatment effects (coefficients) reported in Model 2 in Table 4. A Type S error measures the probability that our estimate has the incorrect sign, given that it is found to be statistically significant. A Type M error refers to the factor by which the effect could in expectation be overestimated, given that it is statistically significant. These *ex post* error measurements have been proposed by Gelman and Carlin (2014) as more appropriate in retrospective design analysis than power analyses, especially when strong statistical effects have been found with small samples.

To conduct these analyses, Gelman and Carlin (2014) recommend using the literature to obtain a hypothesised true effect size; however, in the present case, these 'hypothesised' effect sizes are generally larger than those found in the present study, so instead we simply identify Type S and M errors for our regression coefficients assuming the hypothesised 'true' effect size is half the size of our own estimates. Doing this, we find that - conditional on the estimate being statistically significant - the probability of making a sign error (Type A) is always less than 0.05

[25] We thank Professor Erik Ansink (Managing Editor for this paper) for making this suggestion.

for all treatments, indicating a very low chance of having the wrong sign with the hypothesised estimate.

Using these same hypothesised coefficients, the mean exaggeration ratio (the Type M error) ranges between 1.825 for public observability treatment to a maximum of 2.853 for the communication treatment. This suggests that, for public observability, the estimated effect size is about 1.825 times too large, given that it is statistically significant and assuming the true effect size is half of what was observed; for communication, the estimated effect is 2.853 times too high given the same assumptions. These Type M error values are quite low compared to examples that Gelman and Carlin (2014) consider problematic, which include Type M errors of 9 and above.

If we conduct the same analyses on group level summary statistics (also using hypothesised 'true' effect sizes that are half the size of our own estimates), we find that the probability of making a sign error (Type A) is still less than 0.05 for the public observability and negotiation treatments, confirming findings based on individual-level regression coefficients. However, for the leadership and communication treatments, the probability of a Type S error using group-level statistics is marginally greater, at 0.092 and 0.074 respectively. The probability of Type M errors is also greater using group-level statistics, reaching a maximum value of 4.941 for the leadership treatment. Despite these Type S and Type M errors being somewhat larger for group-level statistics, they are still within the bounds of acceptable error levels. Overall, we conclude that our results are not affected in any major way by sign or magnitude errors.

5. DISCUSSION and CONCLUSIONS

This paper explored the impact of behavioural interventions designed to enhance communication (and negotiation) and observability of behaviour on cooperation around the management of communal water sources in a developing country setting. Data from a series of public goods games conducted in nine villages in Maharasthra in India confirm previous empirical findings that people's behaviour does not conform to rational choice expectations. Cooperation levels in our experiments were consistently above the Nash equilibrium of zero cooperation, but never reached the social optimum of full cooperation. Introduction of mechanisms designed to bolster cooperation by encouraging coordination, social comparison and the attainment of social approval had both expected and unexpected effects.

For example, communication had the expected positive effect on cooperation. We found that providing the opportunity for experimental participants to communicate with each other improved cooperation and this was only modestly (but not significantly) enhanced by the explicit encouragement to negotiate agreements. This suggests that villagers in our study were mostly engaging in negotiation processes during open communication. If this had not been the case, we would have expected lower levels of cooperation in the communication treatment. This has important implications for successful collective action in these villages: from our results, it appears that simply providing villagers with an explicit opportunity to discuss the public good might be sufficient to stimulate cooperation through the crafting of agreements.

The most striking result however was that public disclosure of individual contributions led to rapidly decreasing contributions over time. The explanation given for the typically positive relationship between public observability and cooperative behaviour is that people desire social approval, and contributing to the public good is a socially-approved behaviour (Andreoni and

Bernheim, 2009; Rege and Telle, 2004). However, in our study, this relationship doesn't play out as expected. We are confident that this is not because people in our study do not desire social approval; results from the CV survey carried out in the same villages six months earlier indicate that 94% of respondents agreed with the statement: "*It is important to me that people approve of my behaviour"*. Thus, we assume these attitudes are indicative of preferences for social approval in the experimental sample.

Our results suggest that the decline in cooperation mainly occurs because co-operators immediately reduce their contributions when they observe others free-riding in early rounds of play. Carpenter (2004) suggests that this response is due to conformity, although other processes such as negative reciprocity may also explain this effect. Although we cannot identify the precise psychological mechanism that explains the influence of observable individual free-riding on cooperation, our findings show that the initial conditions of play have a major influence on subsequent behaviour. We found this with regards to both the leadership treatment and the public observability treatment: in both cases, cooperation rates over treatment rounds were strongly determined by early behaviours of observable others. The implication is that, early behaviours are critical: behavioural norms may be established based on initially observed behaviours, even if they are socially suboptimal. Thus, in the context of collective action around water treatment plants, the lack of experience of cooperating around the management of communal water facilities, coupled with a lack of reputation benefits associated with conspicuous contributions to the public good (as observed in the leadership treatment), may stall the development of cooperation and the long-term management of these facilities.

Our results allow us to draw some basic policy recommendations regarding potential interventions to motivate collective action around communal water treatment plants in

Maharashtra. On the one hand, public disclosure of others' contributions is unlikely to increase cooperation. In practical terms, a public observability intervention might have been as simple and cost-effective as posting the names of the families or households who had made their contributions towards the water treatment plant in a public place such as a community center, school or at the treatment plant itself. These types of intervention have been successful at encouraging reductions in residential energy-consumption in the U.S. (e.g. Cuddy et al., 2010). However, results from our study suggest this may not have the desired effect of increasing cooperation. Instead, based on our findings it is suggested that policy-makers and village councils might promote the active participation of villagers in the process of negotiating and crafting agreements around their water treatment plants. This might enhance the potential long-term viability of these water treatment plants and may also help to develop social capital in the villages, which may benefit resource management in general. Further empirical work could examine the potential for different mechanisms to enhance the positive effect of communication and negotiation mechanisms.

More broadly, our study shows that the success of institutional 'design principles' designed to increase cooperation around public goods may depend on characteristics of the community of interest, and the behaviour of those who might be considered to influence those norms. This is especially relevant at critical junctures, such as early stages of cooperative interaction; a failure to account for the initial conditions and behaviours at the start of a cooperative interaction may result in unintended consequences, such as declining levels of collective action. Future research should seek to identify how initial conditions determine the impact of institutional design principles on cooperation, with an eye to developing a generalized understanding of these influences.

We recognise that the external validity of our findings may be limited by the fact that they are based on results from a lab-in-the-field experiment, and that the real world may throw up additional factors that might not affect behaviour in the experiment. We also recognise that, despite framing the experiment in terms of water treatment, players may interpret the game in which ever way they choose. We cannot control for this and suggest that this can be an issue in all experimental studies, much as individual backgrounds, prior perceptions, social status etc will affect how individuals play experimental games (as noted in Cardenas and Ostrom, 2004).

Notwithstanding these limitations, we propose that the findings in this study may provide a point of reference for future studies examining cooperation and collective action around rural public goods in general, and the management of communal water sources specifically. The resolution of social dilemmas, such as the provision of safe drinking water in rural communities in developing countries, remains a major development challenge; hence it is critical to identify mechanisms that will encourage communities to cooperate in the management of their local environmental and technological resources.

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