

Participatory Development and Social Preferences: Experimental Evidence from Bangladesh Pre-Analysis Plan

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Abstract

This document describes the design and analysis plan for evaluating the impact of a Community-Driven Development (CDD) program on fairness norms and value of participatory decision-making. The CDD intervention is an arsenic mitigation intervention conducted in rural Bangladeshi communities. We designed a novel lab-in-the-field experiment in order to measure fairness norms and value of participatory decision-making.

The goal of this document is to outline the hypotheses to be tested and the specifications to be used in the empirical analysis. This document was written before the analysis of any of the experimental data, and before the data collection in connection to the lab-in-the-field experiment was completed. We do not exclude the possibility to conduct additional exploratory analyses. When reporting results we will mark all analyses not planned ex-ante and therefore not included in this document.

1 Motivation

Since the 1990s, Community Driven Development (CDD) has become an increasingly common approach for development assistance. These projects, by encouraging participation and dialogue between social groups, are often promoted as a potential channel to build social cohesion and strengthen democratic values and practices. This motivates a growing literature on the impact of CDD programs on social norms, behaviors and institutions in beneficiaries communities, as well as a renovated attention from policy makers on potential spillover effects from development interventions to broad social changes (World Bank, 2015).

One fundamental challenge in this evaluation consists in the difficulties of observing and measuring social preferences, as both self-reported measures and choices or behaviors realized in real world scenarios might be biased.¹ Perhaps as a consequence of these measurement issues, the existing evidence

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¹On the one hand, exposure to participatory messaging may make members of program communities more likely to self-report higher willingness to cooperate, levels of trust or support for democracy regardless of any substantive change in attitudes or practices (Mansuri and Rao, 2013). These concerns are supported by Avdeenko and Gilligan (2015), as they contrast survey measures with results from lab-in-the-field experiments and show that the use of the former ones lead to overestimate the effect of CDD programs on trust and social cohesion. On the other hand, changes in preferences

on the effect of participatory development on social values and local institutions is mixed, calling for further research on this topic and new measurement strategies.

We contribute to this debate by providing novel evidence on the impact of a CDD program on two dimensions which, to the best of our knowledge, have not been previously addressed by the existing studies: (i) fairness norms on equity and equality of redistribution; (ii) value of participatory decision-making. Following a rising practice that relies on lab-in-the-field experiments to study how social values and norms respond to the implementation of development interventions, we designed a set of novel behavioral games in order to measure our main variables of interest.²

The CDD program we evaluate is conducted within the research project by Cocciolo et al. (2017), “Community contributions, participatory decision-making and local public goods: A field experiment in Bangladesh” (CCLPG), and currently implemented in rural Bangladesh in collaboration with a Bangladeshi non-governmental organization, NGO Forum for Public Health.

2 Program description

We evaluate the impact on social preferences of an arsenic mitigation program conducted in rural Bangladesh by Cocciolo et al. (2017). The program consists of a package of technical advice and subsidies for the installation of new sources of safe drinking water, and it has strong participatory components. It is conducted in the Bogra regions, in eight Union Parishads.³

We exploit the random assignment of communities to the treatment or control group. Treated communities are further randomly assigned to three contribution requirements: (i) under the cash contribution approach communities are required to co-fund the installation costs; (ii) under the labor contribution approach communities are required to provide labor to help with the installation work; (iii) under the waiver approach the new water source is installed for free.

The key decisions to be taken in relation to the program are: (i) how many water sources to install in the community; (ii) where to construct them; (iii) how to divide the required contributions between households; (iv) who are the households responsible for the management and maintenance of each new water source. Communities take all decisions at meetings organized by our project staff. Our field supervisors play a strong facilitatory role, both before and during the community meetings. We organize information meetings in order to increase awareness of water safety issues and stressing the importance that everyone takes an active part in the community meeting. We impose minimum participation requirements in order to start the community meeting, and require that all decisions are taken by unanimous consensus during the meeting in the presence of project staff. We do not implement the project in communities where an agreement is not found after a maximum of three community meetings. The rules and procedures imposed on the decision-making process are designed to reduce the likelihood that influential groups or individuals could co-opt the decision-making process, and ensure that everyone is guaranteed the right to express his/her voice.

In the context of rural Bangladesh, informal decision-making processes are often restricted to elites and influential individuals, and women rarely play an active role in the public sphere. This consideration motivates our study, which aims at evaluating whether the temporary introduction of a more participatory process can have long lasting effects on local institutions and norms.

may not necessarily correspond to immediate changes in realized choices and behaviors, especially if institutions are persistent, controlled by influential minorities or otherwise constrained. This might partly explain why Casey et al. (2012) and Humphreys et al. (2012) find no evidence that CDD programs durably reshape local institutions and the quality of local governance.

²Few recent examples are Almås et al. (2015) and Attanasio et al. (2015), that develop innovative lab-in-the-field experiments to assess the impact of cash transfers program on, respectively, women empowerment and cooperation attitudes. In the context of CDD programs, Fearon et al. (2009) and Fearon et al. (2015) rely on public good games within a lab-in-the-field setting in order to measure collective action capacity.

³Union Parishads are the smallest rural administrative and local government units in Bangladesh.

3 Lab-in-the-field experiment

The purpose of the lab-in-the-field experiment is to measure two novel dimensions of social preferences: (i) fairness norms on equity and equality of redistribution; (ii) value of participatory decision-making.

The lab-in-the-field experiment is conducted with 14 groups per community (1,344 groups in total), 12 groups assigned to group bargaining tasks (1,152 groups in total) and 2 groups assigned to spectator tasks (192 groups in total). We organize one experimental session per community.

Throughout the whole experimental session, instructions are provided verbally by our field supervisors and enumerators. This is motivated by the low literacy rate in the communities where we conduct our study. The field supervisor introduces the lab-in-the-field experiment to all participants and gives the main instructions for each task. Enumerators provide additional clarifications, if needed, to their assigned groups, and explain in details the procedure to elicit the value of participatory decision-making. Enumerators play a crucial role in ensuring that all participants fully understand the rules of each task.

We finalized the experimental design based on the piloting activities described in Appendix E.

It is worth noticing that we always discussed the purposes of this project with our local staff in terms of understanding the dynamics of group bargaining and preferences for participation in the context of rural Bangladesh. We never revealed that the project aims at evaluating how the CCLPG intervention shapes social preferences, and therefore our findings will not be confounded by researchers' demand effects or differential behaviors of our staff in treated and control communities.

3.1 Group bargaining tasks

For the 12 groups engaged in the group bargaining tasks, the experimental session is divided in 5 main parts, outline in Table 1.

During Task 1, Task 2 and Task 3, participants are divided in groups of three and complete a group task. Groups are pre-formed, and are different for each task. We pre-select one person per group (groups as in Task 3) for the two "Participation tasks". We reveal group formation and the identity of the person selected for the "Participation task" only at the beginning of each task.

Players complete each part of the experimental session using tokens. Before Task 1, Task 2 and Task 3 we assign to each participant an initial individual endowment. Within each community, we randomly pre-assign players to the equality/inequality treatment:

- Equality: before each task, participants receive an initial endowment of 10 tokens.
- Inequality: before each task, participants in each group randomly receive initial endowments of 15, 10 or 5 tokens.

During Task 1 and Task 2, groups complete two group face-to-face decision-making exercises, the "Contribution task" and the "Redistribution task". The face-to-face design allows to mimic a real-life collective decision-making process: the negotiation process is unregulated, group decisions entail both common and individual economic interests, people from the same village know each other before and will meet each other after the bargaining process.

During the "Redistribution task", after receiving their individual endowment, participants negotiate on how to redistribute among themselves a group endowment of 30 tokens. In the "Contribution task", participants decide how much of their initial endowment to contribute for the creation of a common pool of resources, equivalent to twice the sum of the contributions, and simultaneously negotiate on how to split it.

In both games we require group decisions to be taken by unanimous consensus: everyone in the group must agree on how to redistribute the group resources (and on the contribution array, in the "Contribution task"). Groups have 20 minutes to reach an agreement. In case the group fails to agree,

Table 1: Timeline of the group bargaining tasks

•	Task 1 (“Contribution task” or “Redistribution task”):
	<ul style="list-style-type: none"> • Trial round • Real round
•	“Participation task” in relation to Task 1:
	<ul style="list-style-type: none"> • Elicitation of WTP for participatory decision-making • Beliefs elicitation
•	Task 2 (“Contribution task” or “Redistribution task”):
	<ul style="list-style-type: none"> • Trial round • Real round
•	“Participation task” in relation to Task 2:
	<ul style="list-style-type: none"> • Elicitation of WTP for participatory decision-making • Beliefs elicitation
•	Task 3 according to the decision taken during the Participation tasks:
	<ul style="list-style-type: none"> • Price and task extraction • Definition of outcomes for Task 3

players are only entitled to keep their initial endowment. The maximum possible winning is the same in the two games.

Participants play one training round of the “Contribution task” and the “Redistribution task”, and one round with real money at stake. Before starting the trial round we verify that all participants understand the rules of the tasks and how their final rewards are calculated. The tasks are designed in order to enable all players, even those with poor numerical skills, to effectively take part in the group discussion.

The order in which the “Contribution task” and the “Redistribution task” are played is randomized across communities.⁴

After each group bargaining task, we conduct an elicitation procedure in order to measure willingness to pay (WTP) for participatory decision-making relative to that task. In this way we obtain two measures of willingness to pay for participatory decision-making, one relative to the “Contribution task” and one relative to the “Redistribution task”. We conduct this part of the experimental session with 1/3 of participants, one player per group, assisted by one enumerator. We select the same players to complete the elicitation procedure after both Task 1 and Task 2. As the elicitation procedure is identical for Task 1 and Task 2, for simplicity in the next paragraphs we describe it referring to Task 1 only.

First we assign to these players their initial individual endowment for Task 3. We explain to these players that during Task 3 they might play again Task 1, with new group peers. During the “Participation task” players decide how they want their group to take decisions. The first option is to take part again in the bargaining stage as in the previous round. The other option is to not participate

⁴Because instructions are given by the field supervisor at the same time to all participants attending the experimental session, it is not possible to randomize the order of the tasks across groups within the same community.

in the decision and receive a given distribution of tokens. In the latter case we assign to the group the outcomes of another randomly extracted group with the same equality/inequality treatment during Task 1. Each person in the group will receive the final number of tokens obtained during Task 1 by the person in the assigned group with the same initial individual endowment.

We measure individual willingness to pay for participatory decision-making adopting a binding auction design. We start by presenting participants with a hypothetical choice between “participatory decision-making” and “assigned distribution” with zero price. We present to all participant ten other choices by varying the price attached to the participatory option, ranging from -5 tokens to +5 tokens (Table 2). We define individual WTP as the highest price attached to the participatory option at which the participant does not choose the “assigned distribution” option. We constraint answers to be coherent throughout the full elicitation procedure.

Table 2: Price list for WTP elicitation

Choice	Alternative A	Alternative B
1	Play Task 1(2)	Group extraction
2	Play Task 1(2) - 1	Group extraction
3	Play Task 1(2) - 2	Group extraction
4	Play Task 1(2) - 3	Group extraction
5	Play Task 1(2) - 4	Group extraction
6	Play Task 1(2) - 5	Group extraction
7	Play Task 1(2) + 1	Group extraction
8	Play Task 1(2) + 2	Group extraction
9	Play Task 1(2) + 3	Group extraction
10	Play Task 1(2) + 4	Group extraction
11	Play Task 1(2) + 5	Group extraction

Before the elicitation procedure, enumerators verify individual understanding for each participant on the two decision-making processes and on how their final outcome is determined under the two alternatives. In order to facilitate participants in their choices, we remind them about their results in the previous round with money at stake, we ask them whether they liked or disliked the bargaining stage and how much they expect to be influential in the last round given their initial tokens.

After eliciting the WTP for participatory decision-making for Task 1, we elicit players’ beliefs on their expected outcomes under the two decision-rules relative to Task 1. We incentivize their choices by awarding a small prize if their guess under the “assigned distribution” option is correct. The beliefs elicitation is not incentivized for the “participatory decision-making” option, as it would not be incentive compatible given the ability of players to collude during Task 3.

The procedure for WTP and beliefs elicitation is identical for Task 1 and Task 2. The decision rule for Task 3 is determined by randomly selecting the game (“Contribution task” or “Redistribution task”) and one price between -5 tokens and +5 tokens. Players in the “Participation task” with a WTP equal or higher than the extracted price complete Task 3 together with their new group peers, and pays/receives the extracted price. Players in the “Participation task” with a WTP lower than the extracted price, as well as their assigned group peers, receive an “assigned distribution” by randomly extracting one group (excluding their own) within the same equality/inequality treatment.

3.2 Spectator tasks

For groups engaged in the spectator tasks the experimental session is divided in 4 main parts (Table 3):

Table 3: Timeline of the spectator tasks

•	Task 1 (“Contribution task” or “Redistribution task”)
•	“Spectator task” in relation to Task 1
•	Task 2 (“Contribution task” or “Redistribution task”)
•	“Spectator task” in relation to Task 2

Participants acting as spectators are asked to take decisions in relation to the “Contribution task” and the “Redistribution task” for other groups, and they know that with some probability one of their decisions is going to be implemented, but do not know for which group they are taking decisions. In all communities, spectators’ choices define the outcomes for the other spectator group.

3.3 Compensation of participants

At the end of the experimental session, we reward participants with a fixed show-up fee of 40 Bangladeshi Takas (1 BDT = 0.013 USD in November 2016).⁵

We compensate participants assigned to the group bargaining tasks with a bonus equal to the sum of their outcomes from the real round with money at stake during Task 1, Task 2 and Task 3. For players completing the “Participation task” we add/subtract the price from the extracted scenario, in case they chose the “participatory decision-making” option under that scenario. We award a prize of 30 BDT for correct beliefs on outcomes under the “assigned distribution” option.

We determine the compensation of participants assigned to the spectator tasks by randomly assigning to them the decision of a spectator in the other group in the same community.

We convert 1 token to 5 BDT. Participants can expect a total reward between 250 BDT and 500 BDT.

3.4 Experimental treatments

In each community, we assign to participants a player ID, that they are instructed to keep during the whole duration of the experimental session. The random assignment of player IDs to participants is performed in office, before the lab-in-the-field experiment, and it is conditional on gender and leadership status of the household. We use player IDs in order to control the group formation and the assignment of enumerators to groups, as well as to randomize players in the group bargaining tasks or in the spectator tasks.

3.4.1 Group bargaining tasks

Within each community, the random assignment of the player ID will determine:

- whether they will take part in the bargaining or perform the spectator tasks;
- the equality/inequality status for the entire experimental session;
- if they will be in a group with/without a leader at each round;
- whether they will perform the “Participation task”;

⁵Since the Bangladeshi law requires a flat 10 BDT tax from those with a daily income larger than 400 BDT, we compensate for it by awarding a 50 BDT show up fee to participants whose outcome is larger than 400 BDT.

- the scenario (task and price) to be implemented for Task 3;
- whether their group bargaining audio will be recorded.

The pre-assignment of IDs is needed to stratify the equality/inequality treatment by two demographic characteristics we plan to consider in the empirical analysis: gender and leadership status (Table 4).

Table 4: Experimental treatments for group bargaining tasks

	Women		Men		Total
	Equality	Inequality	Equality	Inequality	
Groups	3	3	3	3	12
Groups with a leader	1	1	1	1	4

We select players in the “Participation task” using a stratified randomization which ensures that, within each equality/inequality treatment, gender and leadership status are balanced among players in the “Participation task” (Table 5).

Table 5: Experimental treatments for “Participation tasks”

	Women		Men		Total
	Equality	Inequality	Equality	Inequality	
Players in the “Participation task”	3	3	3	3	12
Players in the “Participation task” from a leader household	1	1	1	1	4

The treatment stratification by gender and leadership status described in Table 4 and 5 might not be perfectly implemented in the field in case fewer leader households take part in the experiment (we present the full procedure that determines the final sample of participants in Session 7.2).

Players are assigned their initial individual endowments before starting the group bargaining tasks and the “Participation tasks”. The assignment is based on lotteries performed by enumerators together with participants. In order to ensure understanding and transparency, we ask participants to extract differently colored tokens from a box, and the initial endowment is defined by the extracted color. Players extract their color in a pre-defined order within each group in front of the enumerator. We implement this procedure in order to randomize:

- the initial individual endowments for the “Contribution task” and the “Redistribution task” - performed in front of all players in the same group, each group separately;
- the initial individual endowment for players selected in the “Participation task” - performed only with the player in the “Participation task”, each player separately;
- the initial individual endowments for players completing Task 3 - performed in front of all players in the same group, each group separately; only for the two players not in the “Participation task”; only for groups completing Task 3.

We randomize the initial individual (experimental) endowment within each group. By design, since groups are homogeneous in terms of gender, this guarantees that this treatment is stratified by gender (but not by leadership status).

At the end of the experimental session enumerators perform lotteries in order to determine the final outcomes for Task 3 for groups that do not enter the negotiation stage. For these groups the final outcomes for Task 3 is associated to the decision taken by another group during the previous relevant round. The group is extracted from a lottery box by each player separately, extracting only among groups with the same equality/inequality treatment. The lottery is performed only with players in the “Participation task”, only for groups not completing Task 3.

3.4.2 Spectator tasks

Within each community, we use the player ID to randomly assign to each player:

- a set of spectator questions;
- the spectator whose answer will determine her final payoff;
- whether their audio is recorded.

During the experimental sessions enumerators perform lotteries in order to randomize the initial individual endowments for the trial rounds of the “Contribution task” and the “Redistribution task” for the trial bargaining round. We randomize the initial individual (experimental) endowment within each group. By design, since groups are homogeneous in terms of gender, this guarantees that this treatment is stratified by gender (but not by leadership status).

4 Data collection

This study relies on data collected at several stages, both within the CCLPG intervention and during the lab-in-the-field experiment. Unless specified otherwise, data is collected on tablets and uploaded to SurveyCTO servers. The set up allows us to link individual data from the lab-in-the-field experiment to the household and water source survey data and to the CCLPG project data.

The baseline data collection was carried out between August 2015 and February 2016, before the randomization of the CCLPG treatment status and before the implementation of the intervention. The CCLPG intervention started in October 2015 and it is currently ongoing. For logistics reasons and in order to simplify the field work, the baseline survey was conducted Union Parishad by Union Parishad, and we follow the same strategy for the implementation of the intervention.

The lab-in-the-field experiment started in December 2016 and it is currently ongoing. We conduct the lab-in-the-field experiment only after the CCLPG program is completed: i.e. either where the installation work is completed or where the community failed to raise contributions (if not a waiver community). As a result, the lab-in-the-field experiment is carried out approximately Union Parishad by Union Parishad. We work sequentially in nearby communities, as an attempt to collect the experimental data before information about the lab can spread across communities through existing social networks. We conduct the lab-in-the-field experiment between 1 week and 1 year from the implementation of the CCLPG intervention, but in most cases between 2 weeks and 5 months.

4.1 CCLPG baseline survey

We collected data on household characteristics within the baseline survey for the CCLPG project. It includes the water source census and the household survey in a randomly selected sample of households. During the water source census we collected basic information for each source of drinking water in the community. We surveyed 40 households per community, randomly sampling from up-to-date household lists, which we obtained by digitizing, verifying and correcting existing administrative household lists. The household survey consisted in a detailed interview on household’s composition, health, wealth,

network, leadership, participation in the life of the village, and habits related to water collection and use. We rely on this information in order to stratify the sample of participants to the lab-in-the-field experiment by household leadership status (see Section 7), as well as for estimating heterogeneous treatment effects.

4.2 CCLPG program implementation

We collected data during the implementation of the CCLPG project, including households participation in the community meetings, the duration of the community meetings, the competing proposals discussed during the community meetings, the final location of the new water sources and the array of financial/labor contributions within the community. Specifically, for each household we recorded the number of male and female attendees, disaggregated by age groups (16-34 and 35+), as well as whether household members were influential during the meeting.⁶

One interesting feature of our setting is that we have the complete mapping of existing water sources at baseline and their bacteria and arsenic contamination level. Therefore, we can rely on objective considerations in order to identify the areas most in need of a new public source of safe drinking water, assess how effective the program is in targeting those areas and improving access to safe water. Moreover, we can define households benefit from the program in terms of access to safe water, taking into account the distance between the household and the project tubewell and the arsenic contamination level of the main water source used by the household to collect drinking water before the intervention.

4.3 Individual survey before the experimental session

We conduct an individual survey on values and attitudes with all subjects that agreed to participate to the experimental session (Appendix C). The questionnaire is carried out within few days before the experimental session, during the house-to-house visits to personally invite participants to the lab. It includes measures of age, education, occupation, risk aversion, social values and attitudes, as well as a dictator game incentivized with 50 BDT.

4.4 Experimental data

We employ a team of 6 enumerators and one supervisor for the data collection during the experimental session. We record the outcomes from each task and the time required to each group/player to complete it. Enumerators register their observations on group dynamics, their perceived level of conflict within the group and individual bargaining skills. In each community we record the audio throughout the whole experimental session for half of the groups.⁷ We use the text audit tool of SurveyCTO, which provide us with the time spent on each question of the survey and an indicator for the occurrence of any mistake (for example cases when participants provide contradictory answers during the WTP elicitation procedure).

4.5 Individual survey after the experimental session

We conduct a short individual questionnaire after the experimental session, before payments are disbursed (Appendix D). It records participants' feedbacks on the lab, their satisfaction with their final outcomes from each game, and their understanding of the games.

⁶We record on papers attendance and participation data to the community meeting, the summary and observations by our project staff, and the financial/labor contributions as agreed during the meeting. We record the audio of the community meeting. We plan to digitize this data. We use tablets to collect data on location proposals discussed during the meeting and the final location and contributions.

⁷We plan to obtain transcripts and translation of the voice records for a sample of the audio records.

4.6 CCLPG follow-up survey

As part of the CCLPG program, we plan to conduct a follow-up household survey in order to collect information on household participation during different stages of the project implementation and general feedback on the decision-making process, water quality both in the household and at the water source, maintenance activity, engagement in collective actions or other activities carried out at the community level, and other measures of social values and preferences. The follow-up household survey is currently in the design stage.

5 Randomization of the CCLPG program

We enroll in the CCLPG program 155 communities in the Bogra region, 117 selected randomly to receive the intervention. Treated communities are randomly assigned to three contribution requirements described in Section 2: (i) cash approach; (ii) labour approach; (iii) waiver approach. The randomization of the program is performed during public lottery meetings, where we invite representatives from each eligible community. In order to guarantee complete transparency of the randomization process and full understanding by beneficiaries, the randomization is stratified only by Union Parishad.

The program is randomized at Treatment Unit (TU) level. We define Treatment Units as communities of 50-250 households. We used administrative household lists in order to define candidate Treatment Units. We excluded from the study villages with less than 50 households and we divided larger villages into several smaller Treatment Units along pre-existing geographic boundaries. We refer to “communities” or “Treatment Units” interchangeably.

The CCLPG program targets communities with high levels of arsenic contamination. The final list of Treatment Units enrolled in the CCLPG program and eligible for receiving the intervention is defined using testing results from the census of all sources of drinking water (Section 4.1).⁸

6 Variable definition

In the following sections we define the main variables of interest for the empirical analysis, collected during the experimental session (Section 4.4) and during the CCLPG baseline survey (Section 4.1). We also comment on variable definition from the questionnaire conducted before the lab-in-the-field experiment (Section 4.3).

We do not pre-specify the definition of variables collected during the CCLPG program implementation stage (Section 4.2) and during the CCLPG follow-up survey (Section 4.6). We will use these variables for exploratory analyses.

We do not exclude at this stage that we will include in the empirical analysis other variables not pre-defined in this document. We will comment these cases.

6.1 CCLPG baseline survey

- Household leadership status: indicator equal to 1 in case any member of the household is indicated by at least one other household (in the household survey) as holding a leadership position in the community.
- Number of households in the community that indicated the household as part of their network.

⁸We select communities for inclusion based on their baseline level of arsenic contamination: (i) communities where more than 25% of sources of drinking water are arsenic-contaminated; (ii) or communities where arsenic-contaminated water sources are geographically concentrated in few clusters.

- Household poverty score (2\$ poverty line).⁹
- Household income self-assessment, based on the following categories: very poor; poor; low income; middle income; upper income.

6.2 Experimental data

Group bargaining tasks:

- Number of initial and received tokens in “Redistribution task”/“Contribution task”.
- Number of contributed tokens in the “Contribution task”.
- Inequality in the distribution of initial and received tokens in “Redistribution task”/“Contribution task” (Gini index).
- Total number of tokens kept/contributed by the group in the “Contribution task”.
- Group with leader(s): indicator equal to 1 for groups with at least one player from a leader household.
- Individual value for participatory decision-making: Willingness to pay for participatory decision-making.
- Expected instrumental value of participatory decision-making: difference between the expected outcome for Round 3 from the “participatory” option and the expected outcome for Round 3 from the “group extraction” option.
- Any mistake in the WTP elicitation procedure: indicator equal to 1 for participants providing contradictory answers during the WTP elicitation procedure.
- Intrinsic value for participatory decision-making: difference between the WTP for participatory decision-making and the expected instrumental value of participatory decision-making.
- Total bargaining time in seconds - “Redistribution task”/“Contribution task”.
- Indicators for Tense / Relaxed bargaining as from enumerator observations: one indicator equal to 1 if the bargaining is tense/very tense; one indicator equal to 1 if the bargaining is fine/relaxed.

From the “Spectator tasks”, we define the inequality in the distribution of tokens implemented by the spectator (Gini index).

6.3 Individual survey before the experimental session

We include in the survey questions on social values and attitudes, in the form of categorical variables (Appendix C). For the empirical analysis we plan to use dummy variables rather than categorical variables. Because we expect several of these variables to have low variation, we do not pre-commit at this stage on how to group answers from these variables before being able to look at the data.

At this stage we define only:

- Female: indicator equal to 1 for women.

⁹The poverty score is the Progress out of Poverty Index (PPI), which uses answers to simple questions about a household’s characteristics and asset ownership in order to compute the likelihood that the household is living below 2\$ poverty line. We refer here to the construction of the PPI for Bangladesh. Further references can be found here: <http://www.progressoutofpoverty.org/>.

- Age in years.
- Player participated in the CCLPG community meeting.
- Share of kept BDT from dictator game incentivized with 50 BDT.

As discussed in Section 7, some players accepted to participate to the experimental session but ultimately did not. These players completed the individual survey before the experimental session, at the time of household invitation. We do not use their answers when defining variables to be used in the empirical analysis.

7 Sample

7.1 Selection of communities

We carry out the lab-in-the-field experiment in 96 rural Bangladeshi communities, 35 control and 61 treated communities.

We perform the sample selection in two stages, first in December 2, 2016, right before the start of the project, and then we reviewed it in February 2, 2017.

In our initial design, as in date December 2, 2016, we select 92 communities for the project, 35 control and 57 treated communities. We select the communities where to conduct the lab-in-the-field experiment in order to maximize the balance between the treatment and control group on a set of pre-intervention observables. The random sampling procedure is stratified at Union level and is described in details in Appendix A. Within each Union we enroll for the lab-in-the-field experiment a balanced number of control and treated communities.

In date December 2, 2016, when we performed our random sampling procedure, we excluded from the sample five TUs where installations failed due to hydro-geological constraints (“exogenous failures”). Moreover, we excluded four communities where the project failed “endogenously”: (i) one TU where the community was not interested in holding the meeting; (ii) two TUs where the community did not reach an agreement during the community meetings; (iii) one TU where installation failed for one tubewell and cash contributions failed for the other tubewell.

We excluded communities where the project failed due to hydro-geological constraints because in date December 2, 2016, we were exploring the possibility to adopt an improved technology to successfully install the tubewell(s) in the whole project area, and therefore we preferred not to contaminate the main CCLPG project with the lab-in-the-field experiment before the intervention was fully completed. In date December 2, 2017 this had happened in five TUs in Deuli Union.¹⁰ Installation failures are unlikely to be correlated with social values and local institutions, the main outcome variables for this project, nor with the CCLPG treatment assignment, which is random by design. However, they are a function of geography and this selection rule might bias our estimates. In Section 9.5.2 we clarify how we intend to deal with this issue.

We excluded TUs where the project failed “endogenously” because we considered unfeasible at that stage to conduct the lab-in-the-field experiment in communities where the project failed due to tensions and disagreements within the community or lack of interest in the CCLPG program. Although we are aware that this selection might bias our estimates, our choice was imposed by feasibility constraints.

In date February 2, 2017, we reviewed the sample selection. As our local staff gained more experience in introducing the project to the communities and in involving participants in the experimental session, we re-evaluated this decision. We added these four TUs to the 92 communities selected with

¹⁰In all Unions except Deuli we enroll all control communities in our project. Because of the exclusion of 5 TUs in Deuli Union due to installation failures, in date December 2, 2016 we randomly select 5 out of 8 control TUs.

the random sampling procedure performed in date December 2, 2016. Our final sample consists of 96 communities.

We expect the inclusion/exclusion of communities where the project failed “endogenously” to bias our estimates in opposite directions. In Section 9 we discuss further this issue and we describe our favorite specification and the robustness checks.

7.2 Selection of participants

In each community we invite 42 people to participate in the project, 21 men and 21 women. In order to facilitate participation among women as well as the invitation process, we invite one man and one woman per household. However, we accommodate cases when only one member from the household accepts to participate. Enumerators are instructed to invite household members that can actively participate to the experimental session and understand the rules of the different tasks, giving priority to the household head and his spouse.

We randomly pre-select 21 households among those interviewed during the CCLPG baseline household survey. In each community we constrain this random selection in order to invite 2 households reported as leader by other households in their community. In case one or two members from the household do not accept to participate, enumerators select a replacement household/player within the same community, following a pre-determined (randomized) order. In order to maintain the desired balance between leader and non-leader households taking part in the experiment we instruct our enumerators to replace households within the same leadership status. However, in some communities it might happen that the enumerators are not able to successfully invite to the lab all leader households required by our protocol. In these cases we allow to replace leader households with non-leader households, resulting in a non-perfect implementation of the treatment stratification by gender and leadership status described in Table 4 and 5.

The final sample of participants is determined endogenously by: (i) the household decision to accept our invitation, (ii) the household decision on the household member(s) participating in the lab; (iii) the player decision to actually take part to the experimental session. In the empirical analysis we use the full sample of participants to the lab-in-the-field experiment, unless specified otherwise (Section 6.3). We present the analysis to test for self-selection into the lab-in-the-field experiment in Section 9.6.

7.3 Power calculations

The resulting overall sample consists of 4,032 participants (1,344 groups) in 96 communities (Table 6). Specifically, we enroll: (i) 1,152 groups (3,456 participants) in the “Redistribution task” and “Contribution task”; (ii) 1,152 players in the “Participation task”; (iii) 192 groups (576 participants) in the “Spectator task”.

We follow Hemming et al. (2011) in order to calculate the minimum detectable Cohen’s d - the minimum detectable effect expressed in terms of standard deviation - from a randomized evaluation with a fixed number of individuals per cluster and a different number of clusters per treatment arm (35 control TUs and 61 treated TUs). I refer to the standard significance level $\alpha = 5\%$ and power $1 - \beta = 80\%$.

If we assume preferences within the same community to be totally unrelated, the detectable effect is 0.17, and it raises to 0.21 and 0.25 with intra-cluster correlations of, respectively, 0.05 and 0.10. Following standard descriptors for the magnitude of Cohen’s d , our project has enough power to detect a medium-low effect size.

Table 6: Sample

	Control	Treated			Total
		Cash approach	Labour approach	Waiver approach	
Communities in CCLPG	38	39	39	39	155
Communities in the lab	35	21	19	21	96
Group bargaining tasks					
Groups	420	252	228	252	1,152
Players	1260	756	684	756	3,456
Players in the “Participation task”	420	252	228	252	1,152
Spectator tasks					
Groups	70	42	38	42	192
Players	210	126	114	126	576

8 Hypotheses

We plan to study the effect of the CCLPG intervention on fairness norms and value of participatory decision-making in two separate projects. The hypotheses to be tested within each project are detailed separately in the following two sections. We will decide at a later stage whether to merge the two projects together.

We do not exclude the possibility to test additional exploratory hypotheses. When reporting results we will mark all analyses not pre-specified in this document.

8.1 Effect of the CCLPG program on fairness norms

8.1.1 Main hypothesis

Hypothesis 1a (H1.1a) *Receiving a CDD intervention decreases inequality in the distribution of outcomes from the group bargaining tasks.*

8.1.2 Secondary hypotheses: heterogeneous effects by community characteristics

This second set of secondary hypotheses tests for heterogeneous effects by community characteristics.

Hypothesis 2a (H1.2a) *The effect of the CDD intervention on inequality in the distribution of outcomes is larger in communities with larger inequality at baseline - Gini index from household poverty scores (2\$ poverty line).*

Hypothesis 2b (H1.2b) *The effect of the CDD intervention on inequality in the distribution of outcomes is larger in communities with larger inequality at baseline - Gini index from household income self-assessments.*

8.1.3 Secondary hypotheses: mechanisms

This set of secondary hypotheses will shed light on the mechanisms through which the CDD program shapes fairness norms.

Hypothesis 3a (H1.3a) *The effect of the CDD intervention on inequality in the distribution of outcomes is larger under the “Contribution task” than under the “Redistribution task”.*

Hypothesis 3b (H1.3b) *Receiving a CDD intervention increases the total group contributions in the “Contribution task”.*

Hypothesis 3c (H1.3c) *The effect of the CDD intervention on inequality in the distribution of outcomes is larger in groups assigned to the inequality experimental treatment.*

Hypothesis 3d (H1.3d) *The effect of the CDD intervention on inequality in the distribution of outcomes is larger in groups where there is one (or more) leader(s).*

Hypothesis 3e (H1.3e) *Receiving a CDD intervention decreases inequality in the distribution of outcomes from the spectator tasks.*

8.1.4 Exploratory hypotheses: heterogeneous effects by CCLPG program characteristics

We explore whether the characteristics of the implementation of the CCLPG program affect the size of the effect of the CDD treatment on: (i) inequality in the distribution of outcomes from the group bargaining tasks; (ii) total group contributions in the “Contribution task”. We consider different dimensions representing the efficacy of the CCLPG program, for instance whether an agreement was reached in the first community meeting, the overall improvement in access to safe water and the inequality in distribution of benefits from the intervention.

The characteristics of the implementation of the program are endogenously determined in each community, and are realized only in treated TUs. The evidence from this analysis will only be suggestive.

8.1.5 Exploratory hypotheses: survey measures

We test the effect of the CCLPG program on social values and attitudes self-reported during the individual survey before the experimental session (Section 4.3), for instance on altruism, positive and negative reciprocity, trust, attitudes towards merit and decision from the incentivized dictator game.

As discussed in Section 6.3, we do not pre-specify at this stage how to define relevant outcome variables from the answers given during the individual questionnaire.

We plan to include in this exploratory analysis also few regressions to test the impact of the CCLPG program on stated social preferences and realized local institutions, as collected during the CCLPG follow-up survey. As indicated in Section 4.6, we are currently design the questionnaire.

8.2 Effect of the CCLPG program on value of participatory decision-making

8.2.1 Main hypothesis

Hypothesis 1 (H2.1) *Receiving a CDD intervention increases the value of participatory decision-making.*

8.2.2 Secondary hypotheses: heterogeneous effects by household and individual characteristics

This set of secondary hypotheses tests for heterogeneous effects by individual and household characteristics. We expect a larger effect on subgroups that at baseline are more marginalized and typically less involved in decisions regarding their community.

Hypothesis 2a (H2.2a) *The effect of the CDD intervention on the value for participatory decision-making is larger among women.*

Hypothesis 2b (H2.2b) *The effect of the CDD intervention on the value for participatory decision-making is larger among non-leader households.*

Hypothesis 2c (H2.2c) *The effect of the CDD intervention on the value for participatory decision-making is larger among less-connected households.*

Hypothesis 2d (H2.2d) *The effect of the CDD intervention on the value for participatory decision-making is larger among poorer households (using the poverty score (2\$ poverty line)).*

8.2.3 Secondary hypotheses: mechanisms

This second set of secondary hypotheses shed light on the mechanisms through which the CDD program shapes the value of participation. This part of the analysis explicitly disentangles the effect of the CCLPG intervention on the intrinsic and instrumental value of participatory decision-making. Moreover, it explores the impact of the CCLPG program on the outcomes from the group tasks and the bargaining dynamics. Finally, we test whether the effect of the CCLPG intervention on value of participation varies between the “Contribution task” and the “Redistribution task”, inequality treatment status and initial individual endowment.

Hypothesis 3a (H2.3a) *Receiving a CDD intervention increases the expected instrumental value of participatory decision-making.*

Hypothesis 3b (H2.3b) *Receiving a CDD intervention increases the intrinsic value of participatory decision-making.*

Hypothesis 3c (H2.3c) *Receiving a CDD intervention decreases inequality in the distribution of outcomes from the group bargaining task.*

Hypothesis 3d (H2.3d) *Receiving a CDD intervention decreases the bargaining time to reach an agreement during the group bargaining task.*

Hypothesis 3e (H2.3e) *Receiving a CDD intervention decreases the likelihood that the bargaining is tense during the group bargaining task.*

Hypothesis 3f (H2.3f) *The effect of the CDD intervention on the value of participatory decision-making is larger under the “Contribution task” than under the “Redistribution task”.*

Hypothesis 3g (H2.3g) *Receiving a CDD intervention increases the total group contributions in the “Contribution task”.*

Hypothesis 3h (H2.3h) *The effect of the CDD intervention on the value of participatory decision-making is larger in groups assigned to the inequality experimental treatment.*

Hypothesis 3i (H2.3i) *The effect of the CDD intervention on the value of participatory decision-making is larger for players assigned to a smaller initial individual endowment for the next round.*

Hypothesis 3j (H2.3j) *The effect of the CDD intervention on the value of participatory decision-making is larger for players that in the previous round were assigned a larger initial individual endowment.*

8.2.4 Exploratory hypotheses: heterogeneous effects by CCLPG program characteristics

We explore whether the characteristics of the implementation of the CCLPG program affect the size of the effect of the CDD treatment on preferences for participatory decision-making. We expect communities to place greater value on participation when: the risks of capture of the decision-making process are lower and the process is greatly participated; the costs for reaching an agreement are lower; the improvements in access to safe water are greater and benefits from the interventions are more equally distributed. Similarly, we expect to observe greater value of participatory decision-making for households in treated communities that participated in the CCLPG community meeting(s) and that benefited the most from the program.

These dimensions are determined endogenously by community dynamics and existing social structures. The evidence from this analysis is only suggestive, but it can elucidate interesting mechanisms and provide novel and unexplored insights on when and why CDD programs might reshape local institutions and reinforce democratic practices in beneficiary communities.

8.2.5 Exploratory hypotheses: survey measures

We test whether the CCLPG program increases procedural preferences, participation attitudes and decision-making power, as measured by the following survey questions collected during the individual survey before the experimental session 4.3:

- “I think people should have a say about decisions regarding their community.”
- “If there was a village meeting in order to decide about an issue in my community (e.g. building a new road, school, temple/mosque, tubewell, etc), I would participate in the village meeting.”
- “Think about situations when your household have to take a decision about an important purchase (e.g. furniture). Are you usually involved in these kind of major decisions for the household?”

As discussed in Section 6.3, we do not pre-specify at this stage how to code the answers given to these questions.

We plan to include in this exploratory analysis also few regressions to test the impact of the CCLPG program on stated preferences for participation and realized local institutions, as collected during the CCLPG follow-up survey. As indicated in Section 4.6, we are currently design the questionnaire.

9 Empirical analysis

9.1 Estimation of the main treatment effects

We test the H1.1a from the following specification:

$$y_{gc} = \alpha + \beta T_c + \eta d_c + \epsilon_{gc} \quad (1)$$

where y_{gc} is the inequality of outcomes in group g in community c .

We test H2.1 from the following specification:

$$y_{igc} = \alpha + \beta T_c + \eta d_c + \epsilon_{igc} \quad (2)$$

where y_{igc} is the willingness to pay for participatory decision-making of player i in group g and community c .

T_c is the CCLPG treatment status of community c , d_c are Union fixed effects and standard errors are clustered at community level. We estimate models 1 and 2 on the dataset obtained by pooling together the data from the “Redistribution task” and the “Contribution task”.

We test our main hypotheses H1.1a and H2.1 using two-sided tests on the main coefficient of interest β .

The actual exposure to the treatment depends on: (i) the community decision on whether to receive the CCLPG intervention; (ii) the individual and household decisions about participation to the community meeting(s) and contribution to the installation costs/work; (iii) the decisions taken by the community regarding the project. Since communities can drop out from the program and community members receive the treatment with different intensities, β provides the intention-to-treat effect.

9.2 Secondary and exploratory analyses

In order to estimate heterogeneous treatment effects (Sections 8.1.2 and 8.2.2) we augment models 1 and 2 with the relevant interactions:

$$y_{igc} = \alpha + \beta T_c + \gamma T_c \times Z_{igc} + \delta Z_{igc} + \eta d_c + \epsilon_{igc} \quad (3)$$

where Z_{igc} is the dimension of interest for the analysis of heterogeneous effects and γ the coefficient of interest. We present results from including one interaction at the time, as well as from the model augmented with all interactions.

In order to explore mechanisms (Section 8.1.3 and 8.2.3) and for the exploratory analysis (Section 8.1.4, 8.1.5, 8.2.4 and 8.2.5), we estimate models 1 and 2, appropriately varying the dependent variable, or model 3 with the appropriate dimension for the heterogeneous effect, according to how each hypothesis is specified.

All hypotheses are tested using two-sided tests.

9.3 Dealing with sample selection due to endogenous failures of the intervention

As described in Section 7.1, our sample selection procedure is structured in two stages. Our main concern is with the ex-post inclusion of four communities initially excluded from the optimization procedure performed in date December 2, 2016, but added to the sample in date February 2, 2017.

By excluding the four TUs where the project failed “endogenously”, as well as by adding them ex-post to the optimal sample selected through the procedure described in Appendix A, we might bias our estimates, most likely in opposite directions. Therefore, two natural robustness checks are to include and exclude these four TUs.

In our favorite specification we correct models 1 and 2 by using weights obtained from the ex-ante probability of each community to be selected in the optimal sample. In order to derive the weights we repeat our optimization procedure 1,000 times on the full sample of TUs in the CCLPG program, and for each TU we calculate the probability to be included in the optimal sample. We expect the coefficient estimated from this weighted regression to lie between the upper bound and the lower bound estimated by, respectively, excluding or including the four TUs where the project failed “endogenously”.

9.4 Dealing with multiple inference

We recognize the risk of multiple inference when testing secondary and exploratory hypotheses.

When testing the hypotheses outlined in Section 8.2.3 we treat each hypothesis separately and we estimate models 1 and 2 by varying the dependent variable. We acknowledge the risk of finding false positive results, and we will give low weight to findings with only one significant coefficient, unless precisely estimated. In order to be conservative, we present results with unadjusted pvalues as well as pvalues adjusted for multiple testing.

When estimating the heterogeneous effects described in Section 8.2.2 we deal with the risk of multiple inference by following two strategies. First, we present results from a model fully augmented with all relevant interactions and we test jointly the significance of the interaction coefficients. Second, we adopt a conservative approach and report the estimates from models with one interaction included at the time with both unadjusted and adjusted pvalues.

We follow similar strategies to correct the exploratory analysis.

In order to adjust pvalues for multiple inference we follow Anderson (2008) and employ, within each set of secondary or exploratory hypotheses, the Benjamini-Hochberg false discovery rate correction.

9.5 Robustness checks

9.5.1 Correcting inference

The main rationale for our non-random sample selection is small sample bias reduction, a valid concern posed by our sample size consisting in less than one hundred communities. However, it requires to correct standard errors and pvalues for inference.

As robustness check we present our estimates with bootstrapped pvalues. We implement a two-step bootstrapping procedure. First, we obtain N bootstrapped samples by replicating with replacement our optimization procedure for sample selection (Appendix A). Second, for each bootstrapped sample estimate the treatment effect and obtain standard errors and pvalues from this distribution.

9.5.2 Dealing with sample selection due to installation failures

As explained in Section 7.1, we excluded from the sample of communities for this project five TUs in Deuli Union where installation failed due to hydro-geological constraints.

Installation failures are unlikely to be correlated with our main outcome variables, nor with the CCLPG treatment assignment, which is random by design. However, they are a function of geography: treated communities where installations failed due to hydro-geological constraints are mainly concentrated in the South-Eastern side of Deuli Union. As a result, control and treated communities selected for the lab-in-the-field experiment are not equally distributed in the area: treated communities are mainly from the North-Western side of Deuli Union, while control TUs are homogeneously spread in the Union. This might imply that treated and control communities selected for our project in Deuli Union are not perfectly comparable.

As robustness check we estimate our main specifications without communities in Deuli Union and assess whether our main findings are robust to their inclusion/exclusion.

9.5.3 Varying the set of controls

As robustness check we add two different set of controls to models 1 and 2.

The first set of controls includes the same pre-treatment variables as used for sample selection (Appendix A).

The second set of controls is selected using Lasso algorithm in order to find the best set of controls, i.e. the set of controls that minimizes the standard errors of the main variable of interest T_c . We perform Lasso separately for the three main hypotheses: since each hypothesis is formulated on a different outcome variable, we allow the best set of controls to vary with the hypothesis under consideration. We select the best set of controls for each outcome variable from the following set of variables:

- Equality/inequality experimental treatment for the group;
- Individual and household characteristics including: gender, education, age, household leadership status, household network, arsenic contamination at the main water source, bacteria contamination at the main water source, Muslim, poverty score, household size, participation to a collective

action to provide a new public safe water source, decision-making process for a new public safe water source, WTP (cash) for installation of a new public safe water source in most preferred location, WTP (cash) for installation of a new public safe water source in location serving most of the people in the community, WTP (time) for installation of a new public safe water source in most preferred;

- Community characteristics including: TU size, number of clusters, number of anchors, share of arsenic contaminated water sources, share of bacteria contaminated water source, distance to the closest pharmacy (min), distance to the closest health clinic (min), distance to the closest growth centre (min);
- Enumerator fixed effects;
- For H2.1 only: Initial individual endowment for next round.

9.5.4 Sensitivity to sample selection

Due to the small sample size, our main results might be driven by few influential observations. We assess whether our findings are robust to the exclusion of one community at the time. We report the distribution of the estimated coefficients of interest from this exercise as well as the maximum absolute change in the estimated coefficients relative to their standard errors.

9.6 Self-selection into the lab-in-the-field experiment

Enumerators follow a precise procedure in order to invite households to the lab-in-the-field experiment. However, invited households endogenously choose whether or not to take part to the experimental session, as well as the participating household member(s). Moreover we cannot enforce that players that accepted to participate ultimately take part to the experimental session.

We test the balance between treated and control group in the sample of players participating to the lab (Section 9.6.1). We also examine at which stage potential unbalances occur, testing for systematically differences between control and treated communities in the sample of individuals successfully invited to the experimental session (Section 9.6.2), and for differential attrition from the study (Section 9.6.3).

We will interpret the main findings in light of the results from this analysis. In case we reject the null hypothesis that the self-selection process into the lab is equivalent in control and treated villages, we will also present estimates using probability score matching.

9.6.1 Balance of participants to the lab-in-the-field experiment

We test whether players taking part to the experimental session differ systematically in control and treated communities.

We estimate the following model on the final sample of participants to the experimental session:

$$T_c = \alpha + \gamma X_c + \eta d_c + \epsilon_c \quad (4)$$

where X_c is the set of individual and household characteristics averaged at community level. We use the same set of control variables that we used for the selection of treated and control TUs (Appendix A).

We report the F-test on the null hypothesis that all coefficients are jointly zero as well as the regression table with all estimated coefficients.

9.6.2 Balance among individuals that accepted to participate to the lab-in-the-field experiment

We test whether individuals that accepted to participate to the lab-in-the-field experiment differ systematically in control and treated communities. This will help us to understand whether there is any unbalance originating at the stage of households invitation, when individuals decide on whether to accept or reject to participate in the study.

We perform this test by estimating model 4 on the sample of individuals that accepted to participate to the experimental session (including those that ultimately did not attend it).

9.6.3 Attrition from the lab-in-the-field experiment

We test whether the drop-out rate from the study is significantly related to the CCLPG treatment status by estimating the following model:

$$A_{ic} = \alpha + \beta T_c + \eta d_c + \epsilon_{ic} \quad (5)$$

where A_{ic} an indicator equal to 1 if player i in community c accepted to participate to the experimental session but ultimately did not. Standard errors are clustered at community level. We present results from two specifications with and without the set of control variables outlined in Appendix A.

We test $H_0 : \beta = 0$ against the alternative $H_a : \beta \neq 0$ to determine whether attrition from the lab-in-the-field experiment is related to CCLPG treatment status.

9.7 Data transformation

9.7.1 Questions with limited variation

We do not use in the empirical analysis any variable for which more than 97% of observations carry the same value. In the event that the omission decisions result in the exclusion of variables that characterize one or more hypotheses, the affected hypotheses are not tested. In the event that the omission decisions result in the exclusion of controls, these controls are not included in any robustness check.

9.7.2 Procedure for addressing missing data

We treat “Do not know” and “Refused to answer” answers as missing. The only exception is for questions that check understanding on the “Contribution task” in the “After the lab” survey, for which we treat “Do not know” answers as wrong answers.

We do not perform any imputation for missing data from item non-response.

We expect to have some missing data for variables collected during household or individual surveys, but not for variables collected during the experimental session. For questions asked during the individual survey before the experimental session, we check whether item non-response is correlated with the CCLPG treatment status and if it is, we construct bounds for our treatment estimates that are robust to this. We exclude from the empirical analysis variables for which we have a significant number of missing values.

9.7.3 Outliers

We do not exclude any observation from the final dataset used for the empirical analysis. The risk of outliers in the main outcome variables is minimal by design of the lab-in-the-field experiment. However, we do investigate the existence of outliers from the experimental data as well as from the CCLPG data. In case of a significant share of outliers, we test whether the presence of outliers is correlated with the

CCLPG treatment status and if it is, we construct bounds for our treatment estimates that are robust to this.

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A Appendix: Selection of control and treated communities

Within the CCLPG project, treated communities are further randomly assigned to different contribution requirements in terms of co-funding the project: (i) cash contribution; (ii) labour contribution; (iii) waiver. By design of the CCLPG project, we have 1/4 of control TUs and 3/4 of treated TUs (Table 7).

Table 7: Sample size for CCLPG

Union name	Control	Cash	Labour	Waiver	Total
Deuli	8	8	7	8	31
Saidpur	9	9	10	10	38
Balua	4	5	5	4	18
Mokamtala	9	9	9	9	36
Shibgonj	2	2	2	2	8
Maidanhata	4	4	3	4	15
Roynagar	1	1	1		3
Kichak	1	1	2	2	5
Total	38	39	39	39	155

In our initial design, as in date December 2, 2016, we select 92 communities for the project, 35 control and 57 treated communities. We excluded from the sample: (i) five TUs were installations failed due to hydro-geological constraints;¹¹ (ii) one TU where the community was not interested in holding the meeting; (iii) two TUs where the community did not reach an agreement during the community meetings; (iv) one TU where installation failed for one tubewell and cash contributions failed for the other tubewell.¹² We base this first screening on the available information on the success/failure of the program in date December 2, 2016. At that time we already conducted the community meetings in all TUs in Saidpur Union, and partially in Deuli Union (13 out of 23 communities), Mokamtala Union (11 out of 27 communities) and Balua Union (8 out of 14 communities).

We select the communities where to conduct the lab-in-the-field experiment in order to maximize the balance between the treatment and control group on a set of pre-intervention observables. The random sampling procedure is stratified at Union level. Within each Union we enroll for the lab-in-the-field experiment a balanced number of control and treated communities (Table 8).

¹¹In case of TUs where we offered to install two tubewells, we include those cases where the installation succeeded in one of the two locations.

¹²An additional constraint is that we impose to select in all random samples one TU (V941870001) where on date 02/12/2016 we already invited households to the experimental session, and we drop one TU (V941870012) where we conducted the pilot.

Table 8: Sample size for lab-in-the-field experiment

Union name	Control	Cash	Labour	Waiver	Total
Deuli	5	2	2	2	11
Saidpur	9	5	5	6	25
Balua	4	2	2	2	10
Mokamtala	9	5	5	5	24
Shibgonj	2	1	1	1	5
Maidanhata	4	2	2	2	10
Roynagar	1	1	1		3
Kichak	1	1	1	1	4
Total	35	19	19	19	92

The sample selection is based on an optimization procedure, conditional on the CCLPG treatment assignment. First, we draw 1,000 random samples from the full sample of communities eligible for the lab-in-the-field experiment. The random sampling is stratified by Union and CCLPG treatment status in order to correspond to the scheme represented in Table 8. Second, we select the best sample as the one with the highest pvalue from the F-test on the balance of pre-intervention observables between treated and control communities. The balance test is performed using the following set of pre-intervention observables, aggregated at community level:

- TU size;
- number of clusters;
- share of arsenic contaminated water sources;
- share of bacteria contaminated water sources;
- number of offered water sources if treated;
- average poverty score (2\$ poverty line);¹³
- average willingness to take part to a collective action for the construction of a new public water source;
- share of households reporting that the decision on the construction of a new public water source in their village should be taken by unanimity;
- share of households reporting that the decision on the construction of a new public water source in their village should be taken by majority;
- share of households reporting that the decision on the construction of a new public water source in their village should be taken by the government;
- share of households reporting that the decision on the construction of a new public water source in their village should be taken by village leaders;

¹³The poverty score is the Progress out of Poverty Index (PPI), which uses answers to simple questions about a household's characteristics and asset ownership in order to compute the likelihood that the household is living below 2\$ poverty line. We refer here to the construction of the PPI for Bangladesh. Further references can be found here: <http://www.progressoutofpoverty.org/>.

- average self-reported willingness to pay (cash) for a new public water source in own's favourite location;
- average self-reported willingness to pay (cash) for a new public water source in the best location for the community;
- average self-reported willingness to pay (time) for a new public water source the in best location for the community;
- average network size;
- number of leader households;
- distance to the closest pharmacy;
- distance to the closest health clinic;
- share of community members with no education;
- literacy rate.

In date February 2, 2017, we reviewed the sample selection. As our local staff gained more experience in introducing the project to the communities and in involving participants in the experimental session, we re-evaluated the decision to exclude from the lab-in-the-field experiment communities where the project failed “endogenously”: (i) one TU where the community was not interested in holding the meeting; (ii) two TUs where the community did not reach an agreement during the community meetings; (iii) one TU where installation failed for one tubewell and cash contributions failed for the other tubewell. We added these fours TUs to the 92 communities selected with the optimization procedure performed in date December 2, 2016.

Our final sample consists of 96 communities. In Table 9 we show the pvalues from pairwise ttests between the resulting control and treated groups for the set of pre-intervention observables used to identify the optimal random sample.

Table 9: Balance tests of covariates between treatment and control group

	Control group - Mean (s.e.)	Treated group - Mean (s.e.)	pvalue	Observations
Number of clusters	1.10 (0.31)	1.31 (0.29)	0.315	96
Average household size	3.95 (0.09)	3.91 (0.09)	0.550	96
Number of anchors	1.11 (0.15)	1.18 (0.14)	0.528	96
Average poverty score - 2 USD	81.51 (2.30)	80.97 (2.17)	0.737	96
Participation to a collective action to provide a new public safe water source	0.98 (0.01)	0.97 (0.01)	0.932	96
Decision on a new public safe water source - unanimity	0.77 (0.04)	0.73 (0.04)	0.168	96
Decision on a new public safe water source - majority	0.42 (0.04)	0.43 (0.04)	0.593	96
Decision on a new public safe water source - government	0.09 (0.03)	0.09 (0.03)	0.936	96
Decision on a new public safe water source - village leaders	0.23 (0.06)	0.27 (0.06)	0.245	96
Decision on a new public safe water source - ngo	0.17 (0.06)	0.22 (0.06)	0.203	96
WTP (cash) for installation of a new public safe water source in most preferred	247.49 (34.94)	252.97 (32.95)	0.821	96
WTP (cash) for installation of a new public safe water source in location servin	104.49 (16.49)	110.49 (15.54)	0.600	96
WTP (time) for installation of a new public safe water source in most preferred	10.37 (4.39)	8.90 (4.14)	0.627	96
Share of bacteria contaminated water sources	0.57 (0.04)	0.56 (0.04)	0.618	96
Share of arsenic contaminated water sources	0.68 (0.07)	0.73 (0.07)	0.382	96
TU size	117.35 (17.87)	125.40 (16.85)	0.516	96
Average network size	2.76 (0.13)	2.86 (0.13)	0.270	96
Number of leaders	2.69 (0.12)	2.70 (0.12)	0.852	96
Distance to the closest pharmacy (min)	18.82 (1.93)	18.84 (1.82)	0.988	96
Distance to the closest health clinic (min)	26.81 (2.70)	26.65 (2.54)	0.934	96
Share of not educated people	0.33 (0.03)	0.35 (0.03)	0.330	96
Literacy rate	0.57 (0.03)	0.57 (0.03)	0.677	96

Note: Standard errors are shown in parentheses. Column 5 reports the pvalues from pairwise tests of the mean difference between treatment and control group, from a regression of the outcome variable on indicators for the two groups (with Union fixed effects and no constant).

B Appendix: Scripts

B.1 Introduction of the project to the community

We are working for a NGO called NGO Forum for Public Health, and collaborating with researchers from Stockholm University, Sweden.

NGO Forum is conducting an arsenic mitigation program in the region. As part of that project, some months ago we tested for bacteria and arsenic all sources of drinking water in this village. Moreover, we conducted an interview with some households in this village. Remind the community people about the project and the treatment status of the village, and the progress of the project.

We now selected your village for another related project, which is called “Community Decision Making Project”. The aim of this new project is to study how communities take decisions in rural Bangladesh.

What we learn from this study will help us and other organizations to improve the design of programs, like the arsenic mitigation program we are conducting in this region. This may help other communities like your own.

We randomly selected 21 households for this project, and we will invite one man and one woman per household to participate to an experimental session. Their tasks will take approximately 4-5 hours, and we will compensate participants for their time.

B.2 Invitation of participants

We are working for a NGO called NGO Forum for Public Health, and collaborating with researchers from Stockholm University.

NGO Forum is conducting an arsenic mitigation program in the region. As part of that project, some months ago we tested for bacteria and arsenic all sources of drinking water in this village. Moreover, we conducted an interview with some households in this village. We now selected your village for another related project.

We conducted a public lottery in order to decide which villages were going to receive the intervention and the possibility to construct a new public source of safe water.

Control villages: Your village was assigned to the control group, however, we are working in other nearby villages in your union in order to provide access to safe water.

Treated villages: Your village was assigned to the treatment group. We already conducted in your village the community meeting, where your community decided on where to build the new source of safe drinking water.

B.3 Informed consent

You have been asked to participate in a research study conducted by Serena Cocciolo and Selene Ghisolfi from the Institute for International Economic Studies, Stockholm University, in cooperation with NGO Forum. The purpose of the study is to learn about how groups of people who live in communities like yours take decisions.

The study is composed of an interview today and participation in an experimental session tomorrow. We expect that the interview today will take about 15 minutes, and the experimental session tomorrow will last for 4 to 5 hours. During the experimental session tomorrow you will be asked to take part in three decision-making exercises with other people from your village. Tomorrow we will explain in details the rules of the tasks you will take part in.

You were randomly selected as a possible participant in this study given your household has been previously interviewed for a related project conducted in your village on arsenic mitigation in rural Bangladesh. Please consider the following information before deciding if you consent to participate in this study.

- Participation to this study is voluntary. You have the right not to answer any question, and to stop the interview at any time or for any reason, or to leave the experimental session at any point in time.
- You will be compensated for the participation in this study. At the end of the experimental session tomorrow you will receive a payment which depends on your decisions during group exercises, and we will explain the details of it tomorrow. You can expect to receive between 200 and 400 BDT. The risks associated with this study are minimal.
- The information we will collect during interviews and during the experimental session will be confidential. We will take very good care of your information and no one who is not connected with the project will have access to your personal information, like your name. We will only use your personal information, like your name, in carrying out this project, and if we use information from the survey in the future we will remove your name and change your location so that no one can recognize you.
- We would like to record the experimental session. We will not record the session if you do not grant permission for doing it. You have the right to revoke recording permission at any time.

This project will be completed by April 2017. All interview recordings will be stored in a secure work space until 1 year after that date. The tapes will then be destroyed.

B.4 General introduction to the lab

Welcome everybody and thank you for coming.

This experiment is conducted by researchers from Stockholm University in cooperation with NGO Forum. NGO Forum is conducting in this region a related project in order to provide safe drinking water to communities in this region highly affected by arsenic.

We conduct this experiment in order to study how communities take decisions in rural Bangladesh. The results from this study will help to develop policies that can better serve rural villages.

This experimental session will last around 3 hours and you are going to complete 3 different tasks. At the end you will receive a reward, which will depend on the decisions taken by yourself and your group peers during all the 3 exercises.

You will complete each task in groups. The groups will be different for each task. At the beginning of each exercises we will describe exactly your task. Everything contained in these instructions and

everything you hear in this session is an accurate representation of this experiment. Be sure to ask any questions that you may have during this instruction period, and ask for assistance, if needed, at any time.

You will complete the tasks using tokens. At the end of each round we will record how many tokens you have gained. The more tokens you have earned, the higher will be your final reward.

Each token will be exchanged for 5 takas. We will also reward your participation with a constant show-up fee of 30 takas.

You will be involved in three group tasks.

For the first two exercises, you will first complete a TRIAL round to familiarize with the rules, and then you will complete the REAL round. Only the REAL round will count to determine your final reward. You will complete the third task only once, without TRIAL.

At the end of the session, we will reward all participants according to the sum of tokens you obtained for each task. In order to maximize your winnings, remember to complete each task at your best throughout the whole session!

Throughout the experiment we will use lotteries in order to guarantee the fairness of the experiment for all participants. All the relevant steps are clearly documented, and follow scientific and academic standards. None of these procedures is related to gambling.

You are required to keep a tidy and calm behaviour. Any misbehaviour will be punished with the exclusion from the project and you will not receive any reward. You are explicitly not allowed to:

- Make physical threats of any kind or verbally abuse other players;
- Steal or hide tokens from your group or from the other group members;
- Remove, exchange or lose your ID codes;
- Suggest how to play to people outside your group;
- Agree to share compensations after the experiment;
- Ask other participants how much they have earned when the experiment has ended.

B.5 Contribution task

In this exercise you will start with a number of tokens of your property. You will extract a color, and your tokens will be of that color. The extracted color determines your number of initial tokens and you cannot change it. You will be assigned the same number of tokens in the TRIAL and in the REAL round. Each token has the exact same value, regardless of the color. We will also distribute 30 white tokens.

We will also distribute a timer per group.

Please do not touch the tokens nor the timer until we give you the start.

[Enumerators distribute individual and group tokens. Enumerators distribute the timers and explain how to operate it.]

Imagine now that the marked central area represents a common project you can undertake together with your group mates. Investing money in this common project results in doubling your investment. Your aim is to decide how much of your colored tokens you want to invest in this common project, and simultaneously how to divide among your group the whole amount of the project, which is double the sum of what each of you invested.

To give you a real-life example, imagine that your group has decided to build a new mosque/temple and that a donor has accepted to co-fund it. Then, your group has to decide who is contributing to the mosque/temple, and also where to place it. When you place the mosque/temple the group members who are close to it will be happier than the ones who are far from it.

In practice, during this exercise, any of you can decide to contribute any number of your own colored tokens to the project by putting them in the central area. By doing this you will be allowed to take the same number of white tokens and put them in the central area as well. This is our way to show how the investment in the common project doubles.

In the same way, you can also remove tokens of your color from the central area. When you do this, you must also remove the same number of white tokens.

There must always be the same number of colored (no matter what color) tokens and white tokens in the central area.

You will also decide how to distribute all the tokens in the central area (both the white ones and the colored ones). In order to distribute tokens, you must put the tokens in front of the person you want to give the common tokens to, but still keeping them in the central area.

Contributions to the common project are fully voluntary. However, you will have to agree with your group mates on how to divide the tokens in the central area between yourselves.

You cannot place the colored tokens of another person in the central area, if she does not want to. And you cannot remove from the central area the colored tokens of someone else, if she does not want to.

Similarly, none can take your colored tokens and put them in the central area if you do not want to. And none can take your colored tokens from the central area and place them outside the central area if you do not want to.

You have maximum 20 minutes to reach a final agreement. In order to reach a valid agreement all group members should agree with it. If at the end of the 20 minutes you have not reached an agreement, you will lose all the white tokens and keep just the colored tokens you were given initially. After 20 minutes you will not be allowed to touch the tokens or negotiate anymore.

In case you reach an agreement before 20 minutes, raise your hand and signal that your group has reached a final decision on the distribution of the tokens. One enumerator will come to attend your group.

When you complete the task, or when the time is over, stop the timer by pressing on the “START/STOP”

button.

The enumerators will accept a distribution only if everyone agrees with it. Moreover, they will check that the number of white tokens is the same as the total number of colored tokens.

The enumerator will record the sum of the tokens, both inside and outside the central areas. This represents your result for the round.

The enumerator will reorganize all the tokens as at the beginning. In the REAL round you will receive the same number of tokens of same color. You cannot keep any token from the TRIAL to the REAL round.

In order to clarify the rules, we will now give you some examples:

- Control question 1:
If everyone in the group contributes all his/her tokens, at the end you will have 60 tokens to split across your group. Please raise your hand if this is right.
[Verify EVERYONE has their hand raised. If someone did not understand, clarify.]
- Control question 2:
If everyone in the group contributes no tokens, at the end you will have no tokens to split across your group. Please raise your hand if this is right.
[Verify EVERYONE has their hand raised. If someone did not understand, clarify.]
- Control question 3:
If everyone in the group contributes just 5 tokens, at the end you will have 30 tokens to split across your group. Please raise your hand if this is right. [Verify EVERYONE has their hand raised. If someone did not understand, clarify.]

You will complete this task two times. The first time is a TRIAL, for you to learn the rules. The second time is the REAL round, and the number of tokens will be used to calculate your final reward. We start now with the TRIAL round, you will complete the REAL round after this.

Remember to press the button “START/STOP” when you complete the task or the time is over.

B.6 Redistribution task

In this exercise you will start with a number of tokens of your property. You will extract a color, and your tokens will be of that color. The extracted color determines your number of initial tokens and you cannot change it. You will be assigned the same number of tokens in the TRIAL and in the REAL round. Each token has the exact same value, regardless of the color. We will also distribute 30 white tokens in the central area.

We will also distribute a timer per group.

Please do not touch the tokens nor the timer until we give you the start.

[Enumerators distribute individual and group tokens. Enumerators distribute the timers and explain how to operate it.]

Your task is to agree with your group mates on how to distribute the white tokens among yourselves.

You can take the white tokens from the center and distribute them in the marked central area, in front of the member of your group you want to assign them to. Anyone in your group can move the white tokens. You can always touch and distribute all the white tokens, and you must leave them in the marked central area. You cannot put your own colored tokens in the central area or give them to other players in the group.

To give you a real-life example, imagine that someone has decided to donate to your group to build a new mosque/temple. Then, your group has to decide where to place it. When you place the mosque/temple the group members who are close to it will be happier than the ones who are far from it.

You have 20 minutes to reach a final agreement on how to split the white tokens. After that, you will not be allowed to touch the tokens or negotiate anymore. If at the end of the 20 minutes you have not reached an agreement, the whole group will lose all the white tokens and everyone will just keep the initial colored tokens.

In case you reach an agreement before 20 minutes, raise your hand and signal that your group has reached a final decision on the distribution of the tokens. One enumerator will then come to attend your group.

When you complete the task, or when the time is over, stop the timer by pressing on the “START/STOP” button.

The enumerators will accept a distribution only if everyone agrees with it. Moreover, they will check that colored tokens have not been distributed among players.

The enumerator will record the sum of the tokens, both inside and outside the central areas. This represents your result for the round.

The enumerator will reorganize all the tokens as at the beginning. In the REAL round you will receive the same number of tokens of same color. You cannot keep any token from the TRIAL to the REAL round.

In order to clarify the rules, we will now give you some examples:

- Control question 1:

Your group can decide to split the tokens equally among you. Since the total number of tokens to share is 30, this means that everyone of you can have 10 tokens more than what you started with. If everyone in your group agrees with this distribution, this can be done. Please raise your hand if this is right.

[Verify EVERYONE has their hand raised. If someone did not understand, clarify.]

- Control question 2:

Your group can decide to split the tokens such that at the end of the task everyone has the same number of tokens, either colored or white. This means that some people will have more white tokens, and some people less white tokens. If everyone in your group agrees with the distribution, this can be done. Please raise your hand if you think this is right.

[Verify EVERYONE has their hand raised. If someone did not understand, clarify.]

You will complete this task two times. The first time is a TRIAL, for you to learn the rules. The second time is the REAL round, and the number of tokens might be used to calculate your final reward. We start now with the TRIAL round, you will complete the REAL round after this.

Remember to press the button “START/STOP” when you complete the task or the time is over.

B.7 Participation task

As the instructions are identical for Task 1 and Task 2, for simplicity in the next paragraphs we describe it referring to Task 1 only.

B.7.1 Scripts for the field supervisor

- With all participants:

We are now starting the third part of Task 1. For this part we have formed new groups, different from the groups you just played with. Each group face the same situation as in the Task 1.

We selected randomly one person per group to play this part of the task. This person to decide how he/she wants that her group will take decisions. We will explain the details to each participant later.

During Task 3 of the experimental session, some of the groups might play again Task 1. This will depend on the choices made by the group representative in this part of the task.

According to the choice expressed by the group representative, some groups will play again, and some others will not. In all cases, all of you will receive receive some payment for Task 3.

It is important for all of you to know that the persons selected for this first part of the task should feel free to choose whatever they prefer. At the end you should not ask them which choices they took. Also, you should know that it will not be possible for anyone to understand from the final results of the task which choices they took.

We will now tell you who should stay for this part of the task.

- With only participants selected for the task:

Consider that each group face the same situation as in Task 1. Remind the rules for Task 1.

This time, you have been extracted in order to decide how this group decision will be taken.

The first option is to play again the bargaining stage as in the previous round. This means that you will seat again with your group members and will bargain until you reach a common agreement. You will be paid for Task 3 according to the decision taken with your new group.

The other option is to not participate in the decision. In this latter case, we will impose a decision. We will assign to your group the agreement taken by another group in the previous REAL round of Task 1 that we just played. We will do this assignment using a lottery. Each person in your group will receive the final number of tokens obtained by the person in the assigned

group with the same color. In this way we will define your payment for TASK 3. For example the person with the yellow tokens in your new group will receive the same number of tokens obtained by the person with the yellow tokens in the assigned group. This means that, in case you will not play again with your group, you can expect to receive the same number of tokens as a standard player with your same colour in Task 1. This outcome will be definitive and it will not be possible to change it.

According to your choices, your group might play again Task 1. In case of playing again, you and your group will play during the third round.

Each of you will complete this part of the task with one enumerator.

For this part of the task there will NOT be a trial round. The decisions you will take are final.

Remember that we will keep secret all your answers. The other group members will never know your choices at this stage.

Remember that we already formed new groups, but you do not know the identity of your new group peers.

The rule under which you will play the last round will depend on your choices. Therefore, it is always better for you to carefully pick the option you truly prefer.

In order to clarify the rules, we will now give you some examples:

- Control question 1:
Please raise your hand if you think that the following sentence is correct: “You will be asked to choose between, on one side, performing the Task 1 again and, on the other side, be assigned the outcome of another group.”
[Verify EVERYONE has their hand raised. If someone did not understand, clarify.]
- Control question 2:
Please raise your hand if you think that the following sentence is correct: “In both cases, if you play again Task 1, and if you do not, you will always receive some payment for TASK 3. The two payments might be different.”
[Verify EVERYONE has their hand raised. If someone did not understand, clarify.]

B.7.2 Scripts for the enumerators

I will now present you different choices in which you have to choose between two alternatives.

The first alternative will always be repeat Task 1 as you have just done with new group peers. If you take part again in Task 1, you will be in group with different team mates than before.

The second alternative will be to NOT repeat Task 1 with new group peers.

What will happen in Task 3 will depend on your answers. Before Task 3 we will extract one choice, and the choice you made in that case will be final.

Each choice can be extracted. Therefore, it is always better for you to tell me your true answer. The lottery guarantees that no one will be able to understand your choices. And I will keep secret all your answers. Therefore you can feel free to express your true opinions.

When you choose whether you prefer to complete Task 1 again with new group peers or not you might think at different factors. For example:

- Do you remember how much did you get in the real round you just completed? How much?
[Remind the correct answer.]
- In the previous two rounds, did you enjoy completing Task 1(2) with your group?
- Consider to play again Task 1(2) with your newly assigned initial tokens. How much do you think you will be influential in the group in order to determine the final outcome?

Elicitation procedure of WTP and beliefs:

- Choice 1:
The first alternative is to complete Task 1 again with new group peers.
The second alternative is to not complete Task 1 with new group peers.
Remember that in case we will extract this choice, your new group will complete Task 1 again or not according to your answer. Your decision will be final.
- Choice 2-6:
The first alternative is to complete Task 1 again with new group peers AND lose 1-5 token.
The second alternative is to not complete Task 1 with new group peers.
In case you choose to first alternative, you will complete Task 1 with your initial tokens. We will deduct 1-5 token (5-25 BDT) from your final total compensation.
Remember that in case we will extract this choice, your new group will complete Task 1 again or not according to your answer. Your decision will be final.
- Choice 7-11:
The first alternative is to complete Task 1 again with new group peers AND win 1-5 tokens.
The second alternative is to not complete Task 1 with new group peers.
In case you choose to first alternative, you will complete Task 1 with your initial tokens. We will add 1-5 token (5-25 BDT) to your final total compensation.
Remember that in case we will extract this choice, your new group will complete Task 1 again or not according to your answer. Your decision will be final.
- Guess under the participatory option:
Consider your initial tokens. Imagine to complete again Task 1(2) with new group members. How many tokens IN TOTAL do you think you will get?
- Guess under the group-extraction option:
Imagine you do not complete again Task 1(2), and instead receive the outcome of a player with your initial tokens from another group. How many tokens IN TOTAL do you think you will get? You will win 30 takas if you answer correctly to this question!!

B.8 Spectator tasks

You will be playing two trial rounds, in each round you will have different initial endowments. You will play to learn the rules of the game.

Your final decision on the redistribution will not be taken into account for determining your final payoff, but we will record how much you have contributed and a fourth randomly chosen person who is also among your peers in this room will decide how to split the total amount your group has created with the contributions among you. This person will know how much you have contributed, but not what was the redistribution decided by your group in the end. You do not know who this person is and s/he will not see you playing.

In turn, after you have played the game, each of you will be asked separately how to split the amount among another group which is also now playing in the room. You will be told how much each person in the other group has contributed to the amount but not how they decided to split among themselves. You do not know who is in this other group.

C Appendix: Individual survey before the experimental session

Script for intro:

“In the next questions we will ask you some questions about your preferences and opinion. There will be no correct answer! We are only interested in what are your personal preferences and opinions. So you can feel free to give us your true answers.”

- Think about situations when your household have to take a decision about an important purchase (e.g. furniture). Are you usually involved in these kind of major decisions for the household?
Options: I decide alone; I am involved in the decision; I am not involved in the decision; Don't know; Refused to answer.
- Please tell me how much you agree with the following statement: “Generally speaking, most people can be trusted.”
Options: Strongly agree; Agree; Neither agree nor disagree; Disagree Strongly disagree; Don't know; Refused to answer.
- Please tell me how much you agree with the following statement: “In life, people are rewarded for their efforts.”
Options: Strongly agree; Agree; Neither agree nor disagree; Disagree Strongly disagree; Don't know; Refused to answer.
- Now I will briefly describe some people. Please indicate for each description whether that person is very much like you, somewhat like you, not like you, or not at all like you: “This person is very careful in trying to avoid risks. For instance, when taking farming decisions (men), when cooking (women), when deciding about health, when in traffic, etc.”
Options: Very much like me; Like me; Not like me; Not at all like me; Don't know; Refused to answer.
- Now I will briefly describe some people. Please indicate for each description whether that person is very much like you, somewhat like you, not like you, or not at all like you: “It is important for this person to help the people nearby, to care for their well-being.”
Options: Very much like me; Like me; Not like me; Not at all like me; Don't know; Refused to answer.
- Now I will briefly describe some people. Please indicate for each description whether that person is very much like you, somewhat like you, not like you, or not at all like you: “This person is very good in negotiating with other people: he/she is not afraid about expressing his/her opinion, even when in disagreement with other people, and he/she is able to express his/her own opinion in a convincing way, and he/she is often able to make other people reconsider their position.”
Options: Very much like me; Like me; Not like me; Not at all like me; Don't know; Refused to answer.
- Please state whether you agree or disagree with the following statements about an hypothetical construction of a public infrastructure, for instance a mosque/temple: “The richest people in the village should pay more of the cost of the construction.”
Options: Strongly agree; Agree; Neither agree nor disagree; Disagree Strongly disagree; Don't know; Refused to answer.
- Please indicate whether you agree or not with the following statement: “If there was a village meeting in order to decide about an issue in my community (e.g. building a new road, school, temple/mosque, tubewell, etc), I would participate in the village meeting.”

Options: Strongly agree; Agree; Neither agree nor disagree; Disagree Strongly disagree; Don't know; Refused to answer.

- Please indicate whether you agree or not with the following statement: "I think people should have a say about decisions regarding their community."
Options: Strongly agree; Agree; Neither agree nor disagree; Disagree Strongly disagree; Don't know; Refused to answer.
- Please indicate whether you agree or not with the following statement: "If someone does me a favour, I am prepared to return it."
Options: Strongly agree; Agree; Neither agree nor disagree; Disagree Strongly disagree; Don't know; Refused to answer.
- Please indicate whether you agree or not with the following statement: "If somebody puts me in a difficult position, I will do the same to him/her."
Options: Strongly agree; Agree; Neither agree nor disagree; Disagree Strongly disagree; Don't know; Refused to answer.
- We have paired you with another person in your village. You do not know the identity of this person, and the other person does not know your identity. I am gifting you 50 BDT. The other person does not know about it. If you wish, you can send part of your 50 BDT to this person. In any case, the other person will never know your identity nor your choice. If you decide to gift any of the 50 BDT to this person, she will receive it tomorrow, together with the reward from the experimental session. Equally, you will receive the amount you decide to keep tomorrow, together with the reward from the experimental session. Please tell me now how many takas you wish to keep out of the 50 BDT.
Answer: report integer.

D Appendix: Individual survey after the experimental session

Script for intro:

“Thank you for your participation in the study!

In conclusion, we would like you to ask you few questions on your perceptions of the games. All your responses will be kept confidential: we will not share your answers with anyone outside the research team.

You will receive your compensation from the games after this short survey. The answers in this short survey will not change your compensation.”

- How much are you satisfied with your outcome in the 1st round?
Options: Very satisfied; Satisfied; Neither satisfied or dissatisfied; Dissatisfied; Very dissatisfied; Don't know; Refused to answer.
- How much are you satisfied with your outcome in the 2nd round?
Options: Very satisfied; Satisfied; Neither satisfied or dissatisfied; Dissatisfied; Very dissatisfied; Don't know; Refused to answer.
- How much are you satisfied with your outcome in the 3rd round?
Options: Very satisfied; Satisfied; Neither satisfied or dissatisfied; Dissatisfied; Very dissatisfied; Don't know; Refused to answer.
- After which round were you most satisfied with your outcome?
Options: Task 1; Task 2; Task 3.
- What is the maximum amount all your group could have won in the contribution task?
Answer: report integer.
- How could you reach this maximum amount? (do not probe)
Options: We could have won the maximum if everyone had contributed everything; Other; Don't know; Refused to answer.

E Appendix: Piloting activities

We conducted four pilots in preparation of this project, described in the following sections.

E.1 First pilot

We conducted the first pilot in date November 11th, 2015, in Shibgonj, Bogra region, with 24 players, the enumerators working for the baseline survey for the CCLPG project.

We run this pilot using the finalized version of the “Redistribution task” and “Contribution task”. We tried a simplified version for the “Participation task”, with the following main modifications relative to the final version: (i) we elicited willingness to pay for all players, rather than for one player per group; (ii) we presented to each player only one possible scenario in terms of task and price (iii) each player expressed his preference by her/him self, without the help of an enumerator. We realized that this elicitation procedure is too complicated in our context. Therefore, we decided to not elicit willingness to pay for all players, but only for one player per group, assisted by one enumerator. This reduces the number of players for which we will elicit WTP for participation, but it has the advantage to improve substantially the quality of our data.

E.2 Second pilot

We conducted the second pilot in date January 30th, 2016, with 12 students from the Shibganj College, in Bogra region.

During this second pilot we tested the final design of the elicitation procedure for the “Participation game”, with successful results. The experimental session was conducted by our local staff, under our supervision.

E.3 Third pilot

We conducted the third pilot in July 2016, in three rural villages initially enrolled in the CCLPG program but further excluded because of low arsenic contamination: Lökkikola Uttar and Kheruapara (Deuli Union) and Radakantupur (Saidpur Union).

During this third pilot we tested the sample selection procedure described in Section 7.2 for inviting households to the experimental session, the general logistics and organization of the lab, the use of tablets for data collection during the experimental session, and the instructions of each task with a sample of participants similar to the one we will involve in the actual project. We conducted the experimental session with the assistance of 4 enumerators, in charge of the organization of the lab and the data collection.

We received positive feedback regarding participants’ understanding of the instructions, women participation in the experimental session, and the general reception in the communities of the activities carried out in the lab. In all three villages no concern was raised due to the perception of the games as gambling.

We slightly adapted the experimental design according to few observations from this pilot. We decided to conduct the individual surveys during the house-to-house visits to invite participants to the lab, rather than the same day of the lab as we originally planned. We reviewed some details of the implementation in order to make the tasks as simple as possible, including some visual tools and real-life examples in the instructions. We finalized the timing of each tasks, with the “Participation task” right after the group bargaining task it refers to.

E.4 Fourth pilot

We conducted the fourth pilot in November 2016, in three rural villages: two villages initially enrolled in the CCLPG program but further excluded because of low arsenic contamination, Mathura (Saidpur Union) and Medenipara (Shibgonj Union); one village assigned to the CCLPG treatment, Habibpur (Saidpur Union).

In each village we implemented the final procedure described in Section 3, with the final team of enumerators and field supervisor. We introduced the “Spectator tasks”, that we did not pilot before. After the pilot we carefully reviewed and finalized the guidelines on how to introduce the project to the communities and to participants, how to obtain verbal consent, the role of the field supervisor and the enumerators during all stages of the lab, and few practical considerations in order guarantee confidentiality and prevent interference from other community members not participating in the lab.