Understanding institutional persistence: Exposure to democratic decision-making and the value of autonomy and democracy

Pre-registered report and pre-analysis plan

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Abstract

Communities whose local institutions are non-democratic often experience democratic or inclusive institutions when they participate in development programs. However, despite professing higher pro-social values in surveys afterwards, they typically do not go on to adopt these institutions themselves. Why not? We identify three possible explanations: reported preferences may only reflect experimenter demand effects; communities may still simply prefer their own institutions; or vested interests may resist institutional change. To evaluate the relative importance of these three explanations, we elicit incentivized preferences over different decision-making processes for a community project and then determine whether these preferences differ in communities who have previously been exposed to democratic and inclusive decision-making during a randomized controlled experiment. Our results will shed light on the mechanisms behind institutional persistence.

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Purpose of this document

This document constitutes a pre-registered report for an evaluation of the impact of past exposure to imported democratic institutions on preferences over different decision-making processes. The different decision-making processes correspond to approaches to take decisions during a safe drinking water provision program in rural Bangladesh.

We carry out two waves of safe drinking water projects in randomly-selected communities from a study population. During the first wave, our partner NGO imposes a democratic and inclusive decision-making process. After this first wave, we conduct an elicitation procedure which measures household preferences over different types of decision-making process. During the second wave, we use preferences measured during the elicitation procedure to determine which decision-making process we will implement. The real-world consequences at stake during the second wave incentivize truthful revelation of preferences during the elicitation procedure.

Both waves of safe drinking water projects and all data collection activities are complete. To prepare this document, we have conducted descriptive analyses of the full dataset **blind to treatment status** with respect to experience with democratic and inclusive institutions during the first wave of safe drinking water projects. We adopt this approach because our data on preferences are entirely novel, meaning that we had only limited prior information about the distributions of key outcome variables. This approach allows us to fully explore these novel data before specifying how to measure changes in these preferences.

The purpose of this document is twofold. First, we describe household preferences over decision-making processes. Second, we pre-specify the hypotheses to be tested, along with the empirical approach we will adopt to test those hypotheses, in our evaluation of the impact of exposure to democratic and inclusive decision-making processes. These hypotheses were also pre-registered (RCT registration: AEARCTR-0002709). This document was written before testing any of the pre-specified hypotheses described in this document.

We expect to conduct additional exploratory analyses. When reporting these results, we will indicate that they were not pre-specified.

1 Introduction

A long-standing belief in development practice is that exposure to democratic or inclusive decision-making can induce local institutional reform. However, empirical evidence suggests that institutional reform is rarely so straightforward. Exposure to democratic processes may successfully create effective inclusive institutions that *can* be used, but communities do not necessarily choose to adopt these institutions, often defaulting instead to their own pre-existing institutions. In this study, we make progress on understanding why communities choose—or do not choose—to adopt more inclusive institutions by measuring preferences over different approaches to decision-making and then evaluating how exposure to democratic and inclusive decision-making affects these preferences.

Experience with democratic or inclusive decision-making might in principle lead to changes in local institutions through several channels, for instance by encouraging dialogue between different groups, building social cohesion, or fueling a learning process regarding the potential redistributive gains from inclusive institutions. The empirical evidence, however, is mixed. Exposure to democratic and inclusive decision-making does affect self-reported pro-social values and norms (Avdeenko and Gilligan, 2015; Ibáñez and Rao, 2005; Labonne and Chase, 2011) and behavior in laboratory settings (Fearon et al., 2009), but it does not generally seem to affect behavior in real-world situations (Casey et al., 2012; Humphreys et al., 2012).[1] Fearon et al. (2015) argue that these apparently divergent patterns can be explained if exposure to democratic processes successfully creates functional new institutions, but communities do not adopt those institutions.[2]

Why might communities not adopt these new institutions, despite professing higher pro-social values? One explanation is reporting bias: communities exposed to democratic processes through contact with external organizations may just report higher pro-social values because of experimenter demand effects, despite their underlying preferences remaining unchanged (Mansuri and Rao, 2013). A second explanation is that communities still simply prefer their own pre-existing institutions, either because they expect them to yield better outcomes or because participation in collective decision-making processes is costly in terms of time or social conflict (Mansuri and Rao, 2004). These factors may outweigh any intrinsic preference *for* democratic institutions.[3] A third explanation relates to the inherent persistence of non-democratic institutions, which may arise if dominant groups resist institutional

[1] A potential exception is Björkman and Svensson (2009) who find that participation in community meetings led to persistent changes in management of local health facilities, but only when the intervention was combined with an information provision treatment (Björkman et al., 2017).

[2] Beath et al. (2013) similarly find that local elected councils function effectively several years after their creation as part of a community-driven development program in Afghanistan, but only when specifically called upon by external agencies.

[3] For example, deliberative processes may create a sense of legitimacy over resource allocations (Alatas et al., 2012; Beath et al., 2017; Olken, 2010).

reform when it is not in their own interest (Acemoglu and Robinson, 2000, 2008; Anderson et al., 2015).

In this study, we make progress on understanding the reasons for institutional persistence. We focus on preferences over institutions, for two reasons. First, we work in a context characterized by high levels of institutional persistence (see e.g. Hassan et al., 2016; Wong, 2012). A necessary though insufficient criteria for institutional change is that some households prefer the new institutions to the old ones (see e.g. Ostrom, 2001). Looking directly at preferences allows us to detect these precursors of institutional change even in a context where we expect institutional reform to be rare. Second, it allows us to understand how preferences over institutions vary across households and in particular to understand whose preferences are reflected in local institutions.

The context for our study is rural Bangladesh, where millions of households lack access to safe drinking water because of microbial contamination of surface water and naturally-occurring arsenic contamination in groundwater. Safe drinking water sources are expensive and typically must be subsidized and provided at the community level. Working with a partner NGO, we implement two waves of safe drinking water projects, each in a randomly selected subset of 171 study communities. Communities may receive the program in both waves, one wave, or none. In each wave, key decisions need to be taken in each community, most importantly where to locate any new source.

During the first wave of projects, these decisions are taken using a consensus-based approach, which the partner NGO imposes on communities as a requirement of participation in the program. This approach contrasts with local informal institutions, which are typically non-democratic and are often dominated by a small number of powerful elites. Communities treated under the first wave are thus exposed to democratic and inclusive institutions. Cocciolo (2019) shows that exposure to democratic and inclusive institutions does not appear to lead to any change in local institutions in the study area, consistent with previous literature from other contexts and with the other evidence that characterizes rural Bangladesh as an environment where institutions are persistent.

During the second wave of projects, we determine how decisions will be taken using community preferences over different decision-making processes. To measure these preferences, we carry out an elicitation procedure in which we offer households a series of choices between three different decision-making processes, asking them to choose which they would prefer, should their community be selected to receive a project in the second wave. We conduct this elicitation procedure in all communities after implementing the first wave of projects. Households are incentivized to truthfully reveal their preferences during the elicitation procedure in anticipation of the real-world consequences at stake during the second wave of projects.

The study design allows us to achieve two empirical objectives. First, we describe household preferences over different approaches to decision-making. This document provides a full account of this descriptive analysis. Second, exploiting random assignment of communities to the first wave of safe drinking water projects, we will evaluate whether past exposure to imported democratic practices alters household preferences over different approaches to decision-making. This document pre-specifies our approach to this evaluation. All analyses we have carried out in preparing this document use the full sample, blind to treatment status with respect to the first wave of safe drinking water projects.

We measure preferences over three alternative decision-making processes: 1) decision-making using the community's own pre-existing **local institutions**; 2) decision-making using **imported institutions**, specifically a consensus-based approach to decision-making which imposes representation requirements and nominally broadens both participation and influence over decisions taken; and 3) decision-making by **external agents**, who are employees of our partner NGO and who, while ostensibly benevolent, may lack information that the community has or have a different objective function. The second decision-making process is the same as the one that our partner NGO imposes during the first wave of safe drinking water projects. Both local and imported institutions feature autonomy, defined as communities retaining the right to take their own decisions. The imported institutions are more democratic and inclusive than local institutions.

The large majority of households (71%) rank imported institutions first out of the three decision-making processes. We interpret this as demonstrating preferences both for autonomy, and for democracy and inclusiveness. However, preferences are heterogeneous. A substantial minority (10% of households) prefer to delegate decision-making to outsiders rather than have their communities decide internally, irrespective of the decision-making process. A large minority (34% of households) rank their own local institutions last out of the three decision-making approaches on offer. If we aggregate community preferences using pairwise majority rule, *no* community would choose their own local institutions out of the three on offer and 11% of communities rank their own local institutions last.

During the second wave of safe drinking water projects, we offer to install subsidized safe drinking water sources. Communities who wish to install wells are collectively required to contribute the remaining costs of installation. To extract an explicit relative valuation of the decision-making processes during the elicitation phase, we offer households choices between decision-making processes at different subsidy levels. On average, households are willing to give up 790 BDT (9.4 USD) in subsidies to take decisions under imported institutions rather than delegate to the external agents and 772 BDT (9.1 USD) to take decisions under imported institutions as opposed to their own local institutions. Both values are equivalent to increasing the required community contribution from its baseline level by about 26% or equivalently by about 2.6 times the daily unskilled wage rate.

The disconnect between individual preferences over institutions and local decision-making customs is striking and strongly suggests that current local institutions do not reflect majority preferences. Potentially marginalized groups are more likely to prefer delegation to external agents. Potentially influential groups are more likely to prefer autonomy, but are divided as to whether they prefer imported institutions, with constraints, to local institutions: the self-identified rich prefer local institutions, while those identified by others as community leaders and network focal points prefer the more democratic imported institutions.

There is some evidence that reported preferences are sensitive to experimenter demand effects. We experimentally manipulate whether households report their preferences before or after being informed that real-world consequences are at stake. The results provide a lower bound on the extent of experimenter demand effects, because some households may continue to misreport their preferences even if they know that real-world consequences are at stake. Incentivization slightly weakens reported preferences for the decision-making processes associated with our partner NGO: the consensus-based imported institutions and decision-making by external agents.

The second objective of this study is to evaluate whether exposure to imported democratic and inclusive institutions alters preferences over the different decision-making processes. As discussed above, this document pre-specifies our approach to this analysis. Communities treated under the first wave of safe drinking water projects have acquired direct personal experience with democratic and inclusive institutions via participation in decision-making under the imported institutions process. Since communities are randomly assigned to treatment during the first wave of safe drinking water projects, we can attribute any differences in preferences between treatment and control communities to the causal effect of exposure to imported democratic institutions.

Exposure to democratic institutions might affect preferences over different decision-making processes in three ways. First, it might result in differences in expected household project benefit from the safe drinking water project. Second, it might result in differences in expected social benefit. Third, households exposed to democratic and inclusive institutions might have learned about their intrinsic costs and benefits.[4] We also measure effects on proxies for expected private benefit (expected household access to a new well) and expected social benefit (predicted take-up at the expected source location) under each of the decision-making processes. These results will allow us to evaluate whether effects on decision-making processes are correlated with effects on expected private and social benefit.

The central innovation in our study is to provide communities with a systematic choice between alternative decision-making processes. To our knowledge, no previous study or development program offers communities such a choice.[5] More generally, the main contribution

[4]Costs may include time costs or social costs such as the risk of community conflict. Benefits may include intrinsic value. Households may need to experience participatory and inclusive practices and/or autonomy in order to learn about their intrinsic value; or, consistent with theories of habit formation, may develop a taste for such practices with experience. Similarly, Fujiwara et al. (2016) establish that there is habit formation in voting and argue that the most likely mechanism is an increase in the consumption value of voting. Altruistic individuals may care about both private and social intrinsic value.

[5]Other studies vary decision-making processes to evaluate the consequences for project outcomes (e.g. Beath et al., 2017; Madajewicz et al., 2019; Olken, 2010). More generally, community-driven development (CDD) programs allow communities to take choices, but not always to decide the institutional arrangements

of the study with respect to previous literature is to provide the first incentivized valuation of real-world decision-making processes. Our results will contribute to three literatures.

First, we contribute to the literature on how agents value decision-making processes. Previous studies either measure how agents value decision-making processes in artificial settings, where agents mostly interact with anonymous partners, meaning that social welfare preferences are irrelevant,[6] or how citizens value their own voting rights in a given political environment (Güth and Weck-Hannemann, 1997). The closest related study is Cocciolo (2019), a lab-in-the-field experiment in the same context. Cocciolo uses a similar elicitation procedure to show that exposure to democratic institutions increases the value of group autonomy in a face-to-face bargaining exercise. This study extends the results in Cocciolo (2019) to real-world decision-making and to the value of democracy as well as autonomy.

Second, we contribute to the literature on participatory approaches to development. Many, but not all, studies find that more inclusive approaches increase self-reported satisfaction with and perceived legitimacy of project decisions.[7] However, there is little incentive for households to truthfully reveal their evaluation of a project after its implementation. These ex-post valuations could thus be highly susceptible to reporting bias through experimenter demand effects. Our study contributes to this literature by eliciting truthful revelation of preferences regarding participatory decision-making processes, by providing suggestive evidence on the extent of reporting bias, and by evaluating whether reporting bias changes in response to exposure to democratic approaches.

Finally, our results will shed light on the reasons for institutional persistence. We will interpret our results in the light of a conceptual framework which relates preferences over institutions to institutional change. Our results will provide insights into the role that exposure to democratic institutions can play in fomenting demand for democratization and whether changes in demand are likely to lead in turn to adoption of new institutions. These results will have important policy implications regarding whether and how democratic institutions can be successfully introduced and sustained.

The remainder of this document is organized as follows. Section 2 describes the context for the study and the safe drinking water program. Section 3 describes the conceptual framework. Section 4 explains how we measure preferences over decision-making processes. Section 5 describes the data. Section 6 defines the study population and describes the sample. Section 7 describes preferences over the three decision-making processes, evaluates the reliability of our measures of preferences, describes how preferences vary across groups within communities, and how preferences relate to beliefs about project outcomes. Section 8

for how choices will be taken. Some approaches to CDD delegate decision-making to local institutions e.g. one of the models implemented in Madajewicz et al. (2019). While these approaches permit communities to spontaneously adopt democratic institutions, they do not generally do so (e.g. Madajewicz et al., 2019).

[6]e.g.Bartling et al. (2014); Bolton et al. (2005); Fehr et al. (2013); Owens et al. (2014).

[7]Olken (2010), Alatas et al. (2012) and Beath et al. (2017) find that this is the case; Madajewicz et al. (2019) finds no systematic preference for more inclusive approaches.

confirms that groups assigned to treatment and control during the first wave of safe drinking water projects are comparable at baseline. Section 9 pre-specifies the hypotheses we will test when we evaluate whether exposure to democratic institutions affects preferences and section 10 the empirical tests we will implement to test those hypotheses. Section 11 concludes.

2 Context

The context for this study is a program to provide community sources of safe drinking water in rural Bangladesh, in a region in which many water sources are contaminated with arsenic. The community water sources supply arsenic-safe drinking water. Exposure to arsenic in drinking water leads to a number of serious negative health impacts, including up to an additional 1 in 10 lifetime risk of cancer at high contamination levels (Smith et al., 2000). The program comprises a package of subsidies and technical advice to construct new sources of safe drinking water (Cocciolo et al., 2019a).

The safe water sources are provided to communities for collective use since they are relatively expensive to install. Tubewell installation in the study areas costs on average 60,000 BDT (approximately 600 US$). On average, installed wells are moderately effective in improving access to safe drinking water. The program impacts correspond to each installed well eliminating arsenic use at source (at the WHO threshold) for about 12 households, in a context in which 69% of households use arsenic-contaminated sources at baseline (Cocciolo et al., 2019b).

During the first wave of projects, we offered 1 or 2 safe sources of drinking water, depending on the size of the community, and we randomly varied subsidy levels between 90% and 100% of installation costs. We also randomly varied whether community contributions were required in cash or labour.[8] During the second wave of projects, we offered 1 safe source in all cases, with a subsidy level that varied between 92% and 98% of installation cost. We describe how we determine the final subsidy level in each community during the second wave in Section 4. In both waves, any community wishing to install a well first needed to secure permission from a private or institutional landowner to install a well on their land.

Several decisions must be taken during implementation, but the most critical one is where any new water sources will be installed.[9] This decision is crucial because it determines who among the community will have access to the new water source. In particular, take-up of new sources drops sharply with distance, because households dislike walking far to collect water. At baseline, households used water sources very close to their homes, on average less than half a minute walking distance away. Take-up rates of new sources decline steeply with

[8]We report the results of varying the subsidy level on project impact in Cocciolo et al. (2019a).

[9]Communities also decide whether to install 1 or 2 water sources, if given the choice; how to divide any required contribution between households; and which households will take responsibility for maintenance of any new source.

distance and are negligible at more than 5 minutes walking distance (Cocciolo et al., 2019b).

We focus on three approaches by which decisions could be taken. Under the **external agents** model, project staff, who are not community members, take project decisions, using the geographic distribution of arsenic contamination, population density, and technical site suitability to choose broadly accessible, potentially high impact sites for tubewell installation. Under the **local institutions** model, communities take project decisions using their own traditional decision-making processes. Under the **imported institutions** model, communities take project decisions by unanimous consensus during community meetings with requirements for representation by women and the poor. These meetings are moderated by project staff to ensure compliance with the decision-making rules. The imported institutions are designed to reduce the likelihood that influential groups or individuals can co-opt the decision-making process and to ensure that everyone has the nominal right to express their opinions. We implement the first wave of safe drinking water projects under this model. During the second wave of safe drinking water projects we offer households a series of choices between these three approaches to decision-making.

Both local and imported institutions feature greater autonomy than is normal in the local context. Villages in Bangladesh do not have any formal jurisdiction over the provision of local public goods and services. Decisions about local public goods and services are taken by higher levels of local government or the local offices of ministries/government agencies.

The imported institutions model is also considerably more inclusive that informal local institutions. At baseline, 64% of households reported that they were not usually not involved in taking decisions regarding their community and only 6% had attended a village meeting in the last 6 months. When informal decision-making takes place, decision-making processes are typically restricted to elites and influential individuals. Women play only a restricted role, if any.

Madajewicz et al. (2019) compare the impacts of a similar set of decision-making processes on project outcomes in safe drinking water projects in two other upazilas in rural Bangladesh.[10] Madajewicz et al. (2019) show that the imported institutions model leads to larger project impact, compared to either local institutions or external agents. The advantage of imported institutions over local institutions appears to arise from a reduced risk of elite capture. The advantage of imported institutions over external agents appears to arise from an improved ability to use local information. Thus, in a similar context, the imported institutions model does appear to have advantages for increasing project impact.

Almost all communities participated in the consensus-based decision-making process and successfully agreed on a location. One community declined to participate in the program

[10] In the interests of exposition, we use different labels to describe the decision-making processes to those in Madajewicz et al. (2019): the external agents model in this paper corresponds to their top-down model; the local institutions model corresponds to their community participation model; and the imported institutions model corresponds to their regulated community participation model.

entirely. One community participated in the decision-making process, but did not reach a consensus regarding well location.[11] On average, about half of all households were represented by at least one individual at the community meeting, implying that they gained direct personal experience with the consenus-based decision-making process.

Among treated communities who successfully completed the consensus-based decision-making process, 67% then went on to successfully secure land and coordinate contributions (if required) for at least one tubewell. The most common reason that communities did not install tubewells, accounting for the majority of cases, was failing to raise the contribution under the cash contribution requirement. In other cases, communities could not identify a suitable site.[12] Take-up rates were therefore much lower under the cash contribution requirements. Among communities who successfully completed the decision-making process, 21% successfully secured land and coordinated contributions for at least one tubewell under the cash contribution, while 87% did so under the labour contribution requirement and 98% did so under the contribution waiver (Cocciolo et al., 2019a).

Rural Bangladesh is an ideal environment in which to study institutional persistence. A recent wave of official reforms aimed to improve governance through decentralization and citizen engagement.[13] However, despite formal compliance, implementation of these reforms has in practice been constrained by pre-existing power structures and by the clientelistic relationships that govern local communities (Hassan et al., 2016). For example, Buchmann (2019) reports that often only the elites, local leaders, party members or politically connected households are informed about and invited to participatory fora. Others, particular those from marginalized groups, are discouraged from attending because the fora are only nominally participatory and participation thus leads to no material change in outcomes. Cocciolo (2019) confirms that exposure to democratic and inclusive institutions does not lead to any reported changes in local governance practices in the present study context.

3 Conceptual Framework

We can conceptualize the expected value that an individual i associates with a decision-making process j as being a function of several components, similar to those in the Downsian

[11]If communities did not reach consensus at the first meeting, we arranged up to two additional meetings. In practice, only one community failed to reach an agreement in one or two meetings. This community then declined to hold a third meeting. Otherwise, 93% of communities agreed on tubewell location(s) during the first community meeting and 7% did so at a second community meeting.

[12]Tubewell installation also failed in some cases for hydrogeological reasons, despite the community completing all the project requirements. The sample for this study largely excludes these cases, as we discuss in Section 6.2.

[13]e.g. the Right to Information Act (2009) and the Union Parishad Act (2009).

calculus of voting (Downs, 1957):

$$V_{ij} = f\Big(\mathbb{E}\left(\text{private benefit}_{ij}\right), \theta_i \times \mathbb{E}\left(\text{social benefit}_{ij}\right), \mathbb{E}\left(\text{net intrinsic value}_{ij}\right)\Big) \tag{1}$$

Individuals prefer decision-making process j to decision-making process k when $V_{ij} > V_{ik}$.

Ceterus paribus, V_{ij} is increasing in each of its arguments. Individuals associate higher value with a decision-making process when they expect to gain more from it themselves, which in this context means having a safe source of drinking easily accessible to their household. Conditional on their own expected benefit, they associate higher value with a decision-making process if they expect social benefit—which means other households gaining access to safe drinking water—to be higher and if other households' welfare enters into their utility function, which it does in the formulation above if they have an altruism parameter $\theta_i > 0$. Expected value also increases with the net expected intrinsic value of participation in the process. This term can be positive or negative, reflecting both intrinsic value associated with decision-making processes—such as legitimacy or the intrinsic value of autonomy or procedural fairness—and intrinsic costs, such as time costs or the risk of social conflict.

We could further conceptualize net intrinsic value as arising from both private and social intrinsic value, again weighted by the altruism parameter θ_i:

$$\begin{aligned} \mathbb{E}\left(\text{net intrinsic value}_{ij}\right) = g\Big(&\mathbb{E}\left(\text{net private intrinsic value}_{ij}\right), \\ &\theta_i \times \mathbb{E}\left(\text{net social intrinsic value}_{ij}\right)\Big) \end{aligned} \tag{2}$$

In practice, however, we will not be able to empirically separate these two components.

Assume that the individual-specific costs of replacing institution k with institution j are C_{ijk}. These costs may include both private and social costs, again weighted by the altruism parameter.

There are two general preconditions for institutional change in a community (following Ostrom, 2001). First, communities with pre-existing institutions k will only adopt a new institution j if $V_{ij} > V_{ik}$ for at least some i in the community. Second, communities will only adopt the new institution if $V_{ij} - C_{ijk} > V_{ik}$ for all members of a sufficiently large coalition of individuals within the community to effect institutional change. The size of the required coalition depends on the community, which individuals join the coalition, and the type of institutional change. To replace a regime of elite control with democratic institutions, for example, the costs of change could be low for the ruling elite but prohibitively high for those outside of the ruling elite.

More specifically, if exposure to democratic institutions (imported institutions, in our terminology) is to lead to institutional change away from local institutions, then three progressively more restrictive conditions must be met:

i) $V_{i,imp}$, the value of imported democratic institutions, must increase, for at least some individuals;

ii) the increase in $V_{i,imp}$ must be sufficiently large to lead to a change in preference ordering between imported and local institutions, for at least some individuals;

iii) the increase in $V_{i,imp}$ must further be sufficiently large to lead to a change in whether $V_{i,imp} - C_{i,imp,loc} > V_{i,loc}$ for a sufficiently large coalition of individuals to effect institutional change.

Clearly, iii) implies ii), which in turn implies i).

The puzzle outlined in the introduction is that communities report higher preferences for fairness and other features of democratic institutions after exposure to democratic institution (higher $V_{i,imp}$) but we find very limited evidence for institutional change. We outlined three possible explanations. The first is that measures of $V_{i,imp}$ in previous literature are biased. If so, then a simple explanation for the apparent puzzle is that $V_{i,imp}$ does not really change in response to exposure to democratic institutions, meaning that i) is not met. The second possible explanation is a violation of ii): $V_{i,imp}$ may increase for some individuals, but the increase is insufficient to reverse the preference ordering between imported and local institutions. The third possible explanation is a violation of iii). Even if ii) is met, there may not be a sufficiently large coalition of individuals for whom the increase in benefits associated with a transition from local to imported institutions exceeds the transition costs. We return to the question of how our empirical analyses will help us evaluate the relative importance of these three explanations in Section 9.

The three explanations we lay out are not exhaustive. For example, another possible explanation for the puzzle is that different institutions are applicable to different decisions, and in particular, that imported institutions are not transferable to problems outside of the narrow context of development projects. In our case, we shut down this particular channel by ensuring that the institutions communities are exposed to are transferrable to the scenarios in which we elicit preferences over institutions.

4 Eliciting valuation of decision-making processes

The elicitation procedure is designed to incentivize households to truthfully reveal their valuation of decision-making processes. The decisions which will be taken relate to the safe drinking water project described in Section 2. We conduct the elicitation procedure after implementing the first wave of safe drinking water installation projects and before assignment to treatment for the second wave of projects.

During the elicitation procedure, we inform households that we will carry out this second wave of projects and that their community may be randomly selected to receive one of these projects. We present each participant with three sets of pairwise choices between the three decision-making processes, also described in Section 2, in the following order: (i) imported institutions vs external agent; (ii) local institutions vs external agent; (iii) imported institutions vs local institutions. While in principle we could infer preferences for the third pair from preferences over the first and second pairwise comparisons, eliciting preferences pairwise allows us to verify transitivity of individual preferences, providing a useful check on the validity of our approach. We inform households that their preferences over decision-making processes will be used to determine how decisions for the second wave of projects will be made.

For each pairwise comparison between two decision-making processes, we elicit a differential willingness to pay (WTP) for one process compared to the other process. To do this, we offer households a series of choices between decision-making models paired with different subsidy rates. For example, we might offer a choice between A) local institutions with a required community contribution of 3,000 Bangladeshi taka (BDT) and B) external institutions with a required community contribution of 4,000 BDT. Choosing one decision-making process over another decision-making process paired with a lower subsidy rate implies a positive differential willingness-to-pay (WTP) for the first decision-making process, and vice versa. It is important to note that the WTP does not correspond to an amount of money that the individual is willing to personally sacrifice to implement the decision-making model of their choice but instead to the amount of the project subsidy that the individual is willing to sacrifice on behalf of the community.

The series of choices that we offer holds the community contribution requirement associated with one of the decision-making processes fixed at 3,000 BDT and varies the community contribution requirement associated with the other process between 1,000 and 5,000 BDT. For comparison, the daily unskilled wage rate is 300 BDT. This design allows us to pin down the differential WTP for each individual (within an interval) while still only presenting individuals with a reasonable number of simple pairwise choices. Table 1 shows the full list of choices presented to each respondent for each pairwise comparison between decision-making processes.

Offering one option at a fixed contribution and one at a varying contribution has the advantage of simplicity and clarity, but we were concerned that responses might be sensitive to which model we offered at a fixed contribution and which we offered at a varying contribution level.[14] We therefore randomly varied, at the community level, which process was associated with the varying contribution requirement and which process with the fixed con-

[14]There are a number of potential motivating concerns. There could have been strong complementarity between subsidy levels and decision-making processes. For example, if raising larger community contributions requires participation of wealthy households, and they will only contribute in exchange for power over decisions, a household might strongly prefer inclusive institutions at low contribution levels but local institutions at higher contribution levels. Thus there was no reason to assume symmetry of preferences with respect to relative prices i.e. that a household who preferred decision-making process i with subsidy level 2,000 BDT to decision-making process j with subsidy level 3,000 BDT should also prefer process i with subsidy level 3,000 BDT to process j with subsidy level 4,000 BDT. We were also concerned that offering one option at a fixed price could induce default effects.

tribution requirement for each pairwise comparison.[15] We describe the stability of reported preferences across these changes to the elicitation procedure in Section 7.2.

Enumerators use cards showing simple vignettes, as shown in Figures 1a to 1c, to represent the decision-making processes. These were paired with colour-coded cards showing the required community contributions, as shown in Figure 2, to illustrate the choices on offer. The entire elicitation procedure took on average a little under 16 minutes, with a further 5 minutes required to ask follow-up questions on household beliefs.

Truthful revelation of preferences is incentivized by the fact that real-world consequences were at stake during the second wave of safe water source construction programs. We randomly select the communities that will receive the second wave of safe drinking water projects at public lottery meetings, held after the elicitation procedure.[16] We emphasise the random nature of assignment in order to lessen the likelihood that respondents believe their answers could influence assignment and thus answer strategically. Assignment of the first wave of safe drinking water projects was also by public lottery, so all communities have direct, recent and salient experience with random allocation. For the selected communities, we additionally randomly extract one of the pairwise choices—between decision-making processes and subsidy levels—presented to that community. We then implement the preferred option in their community, aggregating preferences across individuals by majority rule. For example, say we extract the choice between A) local institutions with a community contribution level of 3,000 BDT and B) imported institutions with a community contribution level of 4,000 BDT. If 60% of households in the community chose option B when faced with the same decision, we implement the program under the terms in option B.

This design makes the elicitation procedure incentive compatible and strategy-proof. There is no incentive for respondents to untruthfully report their preferences, especially given that respondents can always choose to simply not answer our questions. The time costs of participating in the elicitation are minimal, given that households who participate have already agreed to participate in a household survey. Each community has a positive probability of receiving a safe water source project during the second wave. Respondents should truthfully report their preferences, as long as they believe there is a non-zero probability that their preferences influence how the intervention will be implemented during the second wave. During the elicitation procedure, we give households a full explanation of how we will use their reported preferences to determine how the intervention will be implemented, providing illustrative examples. We stress that every respondent's answers can influence how the future intervention will be implemented. To evaluate whether or not households believe their preferences can influence how the intervention is implemented, we obtain two measures

[15]We vary this at the community level because we later aggregate preferences at the community level, requiring us to present all households in the same community with the same choices.

[16]We hold the public lottery meetings at the union level (a local administrative unit equivalent to a local council). We invite representatives from each community to the public lottery meetings, along with local authorities. Appendix A provides additional details on the lottery procedures.

of respondents' beliefs about their pivotality, which we discuss in Section 7.2.

Concerns about social desirability bias or experimenter demand effects cannot be eliminated completely. To affect reporting of preferences, such effects must be more salient to the respondent than the real expected gains from answering truthfully i.e. the increased likelihood of having a safe drinking water project implemented under their preferred conditions during the second wave. To evaluate whether knowing that real-world consequences are at stake affects reporting of preferences, we randomly vary whether we inform households that real-world consequences are at stake before or after they report their preferences during the elicitation procedure. If we inform them that real-world consequences are at stake after the elicitation procedure, we give them an opportunity to revise their preferences in the light of the new information.

Implementing pairwise majority rule to aggregate preferences is preferable from a welfare perspective to alternative ways of aggregating preferences which would ensure incentive compatibility—such as randomly extracting a single household and implementing their preferences for the entire community—because it lessens the likelihood of imposing a very unpopular choice on communities and on our field staff.[17] The design also ensures that no choices expressed by any participant are revealed at any stage of the experiment, preventing confounding factors due to peer pressure or reputation concerns and minimizing risks for individuals involved in the study. It was not feasible to cost-effectively elicit an individual WTP without compromising the anonymity of individual responses.[18]

Communities who received a safe drinking water construction program in the first wave may place a lower value on a new safe drinking water construction program. However, the elicitation procedure holds constant the mean value of the safe drinking water program and measures the difference in values when the program is implemented under different decision-making processes. Additionally, the number of water sources we are able to fund is small compared to the size of the communities, meaning that there is typically still considerable demand for additional sources.

5 Data collection

The primary source of data for this study is the data on preferences over decision-making processes collected during the elicitation procedure. Figure 4 shows the project timeline. We

[17] The counterargument is that by imposing majority rule we prime households to think about democratic alternatives to local decision-making. However, none of the decision-making models we offer corresponds to any form of majority rule. In practice, we show in Section 7.2 that this cannot affect our results.

[18] It would be difficult to implement a single individual's choice as a dictator and collect the required payment from the individual while maintaining individual anonymity. Additionally, with the individual approach we would only have been able to reveal the extracted choices at the public lotteries, and not the final terms. Also, using the subsidy level as the benchmark allowed us to measure positive and negative WTPs without asking for payments from poor households or having to fund an initial cash transfer. As we noted above, we also preferred to aggregate preferences using majority rule for ethical reasons.

implemented the first wave of safe drinking water projects in treated communities between October 2015 and August 2017. Enumerators carried out the elicitation procedure between February and July 2018, alongside a more general follow-up survey. Random assignment to the second wave of drinking water projects and implementation of these projects took place progressively in unions in which the elicitation procedure was complete, beginning in May 2018.

Enumerators conducted the elicitation procedure with the household head, their spouse, or another adult representative of the household. Which household member responded on behalf of the household depended on the availability of household members at the time of the survey. Participation in the surveys and elicitation was always conditional on giving oral informed consent. The interviews and elicitations were conducted in Bengali. Given the low literacy rate in the communities involved in our study,[19] the face-to-face design was crucial in order to ensure full understanding of all participants. Appendix B reports the scripts of the elicitation procedure.

We also collected baseline data in all study communities before implementation of the first wave of safe drinking water projects.[20] We use these baseline data to verify comparability of communities assigned to treatment and control during the first wave of safe drinking water projects in Section 8.

We have several additional sources of data upon which we will be able to draw in our exploratory analyses.[21] We have a georeferenced census of water sources in all study communities at baseline, including measures of water quality. We also draw on rich data describing the implementation process during both waves of safe drinking water projects, for example regarding whether households participated in the decision-making process, contributed to water sources installed, or used installed sources. Finally, our field team in Bangladesh conducted focus group discussions in all communities in which we implemented safe drinking water projects during the second wave under local institutions or decision-making by external agents. These qualitative data allow us, in particular, to collect descriptive information about how decisions are taken under local institutions when communities preferred to do so.

6 Study population and sample

This section provides an overview of the study population and sampling procedure. Appendix D provides further details.

[19] In our sample, 41% of household heads have no education, and 76% cannot read.

[20] We collected baseline data for the majority of communities in the study between August 2015 and February 2016. In two unions, we recruited communities to the study later, collecting baseline data in March and April 2017, after additional funding allowed us to extend the sample size.

[21] More details on data are given in Appendix C.

6.1 Study population

Our study is located in north-western Bangladesh, in Shibganj and Sonatala Upazilas in Bogra District and in Gobindaganj Upazila in Gaibandha District. Since our experiment is designed around the provision of arsenic-safe sources of water, we enrolled communities with high levels of baseline arsenic contamination.[22] The relationship between arsenic contamination and income varies across different regions of Bangladesh, with different studies finding correlations of opposing signs between assets and arsenic contamination (Buchmann et al., 2019; Madajewicz et al., 2007). In our study area, arsenic contamination is inversely correlated with measures of income including assets. However, as we show in Cocciolo et al. (2019b), the sample in our study is quite representative of the rural Bangladeshi population in general.

6.2 Sample

The final sample consists of 165 communities, 123 of which received treatment under the first wave of safe drinking water projects and 42 in the control arm. Each community comprises a geographically contiguous group of between 50 and 250 households, defined using local administrative lists and spatial information from field visits and satellite imagery.

The sample in this study differs in one respect from the original randomized controlled trial (Cocciolo et al., 2019a). The original sample consisted of 171 communities, of which we randomly assigned 129 to receive the first wave of safe drinking water projects and 42 to control. The difference arises because we were unable to carry out the elicitation procedure in 6 of the 129 communities in which we carried out the first wave of projects. In these 6 communities, we attempted to install safe tubewells during the first wave of projects and failed because of local hydrogeological conditions. As a result, the chances of successfully installing a safe water source in future were extremely small, and asking these communities about preferences regarding a future safe water source was not a meaningful exercise.

Tubewell infeasibility occurs either because a rocky layer prevents local drillers from accessing safe drinking water or because sandy layers make excavations unstable, preventing installation of the tubewell pipe. Drillers can only determine tubewell feasibility by attempting installation, and tubewell feasibility is uncorrelated with local household and community characteristics.[23] However, there is spatial correlation in tubewell feasibility. As a result, excluding the 6 communities where tubewells were not feasible from the treated arm potentially introduces geographical imbalance between treated and control groups. To address this potential imbalance in our main specification, we weight communities where no installation was attempted by the local probability of having at least one successful installation,

[22] We recruited and screened communities based on baseline levels of arsenic, continuing recruitment until we had reached our target sample numbers.

[23] Appendix E provides further details.

had we attempted installation.[24] Figure 3 shows the distribution of treatment and control communities along with their tubewell feasibility-adjusted weights.

We collected data in a random sample of households, stratified by household characteristics to oversample households from some special populations such as community leaders, network focal points, and households which contributed to water sources during the first wave of safe water source projects. In total, we conducted the elicitation procedure with 7,673 households in the 165 communities. The weights we use in our main analyses, in addition to accounting for tubewell feasibility as we describe above, also account for heterogeneity in sampling rates within and between communities, so that each community counts equally in summary statistics.

In the descriptive analysis in this paper, we make one additional sample restriction based on data quality checks. As we discuss in Section 7.2, a natural data quality check is the rate of intransitivity implied by the preferences expressed by households, since it is unlikely that individual preferences are truly intransitive. We find that two enumerators (of a total of 25 who worked on the elicitation procedure) recorded preferences with rates of unambiguous intransitivity of 79% and 87% respectively. These enumerators did not participate in the full training process, having been hired at a later stage to replace enumerators who left the project. We regard this as providing an indirect and unintended check on the quality of the training provided. In the descriptive analysis, we exclude data from these two enumerators, because we do not believe that data collected by these enumerators contains meaningful information about preferences, and including this data substantially alters the distribution of preferences. We pre-specify our approach to households reporting intransitive preferences for the evaluation of the effects of exposure to democratic institutions in Section 10.1.[25]

[24]Intuitively, we would like to exclude from the analysis those communities where we would not have successfully installed tubewells, had we attempted installation. We cannot observe this, but we can calculate the local probability of success. Weighting the communities in which we did not attempt installation by this probability recovers the sample which in expectation has the same properties as the sample where we attempted installation. This approach is conceptually similar to taking the average over a series of matched samples. Appendix F provides further details on how we calculate these weights along with the sampling rates discussed in the next paragraph. We also pre-specify a robustness check to evaluate sensitivity to alternative approaches to dealing with this sample restriction in Section 10.4.

[25]A team of 20 enumerators conducted the elicitation procedure and follow-up survey. A total of 5 enumerators left the project and were replaced during the project. Among the 20 enumerators who participated in the full training, the median rate of unambiguous intransitivity was lower than 2.5% and no enumerator had rates of intransitivity higher than 14%. Excluding data from the two enumerators with low quality data output implies dropping 621 observations, with a remaining sample of 7,052 observations.

7 How do households value decision-making processes?

7.1 Average preferences

Preferences at equal community contribution requirements Column 1 of Table 2 tabulates the distribution of preference profiles when the two decision-making processes are offered at the same price. Most households express preferences for autonomy over decision-making: 81% of households have preference profiles that rank imported institutions above decision-making by external agents and 63% rank local institutions above decision-making by external agents. However, 79% of households also prefer imported institutions to local institutions. This last finding is striking because nothing in the project rules prevents communities from adopting these imported institutions–or any other constraints on the decision-making process—themselves.

Of the full sample, 71% rank imported institutions first and only 8% rank it last. Decision-making by external agents is ranked last by 56% of households, although 10% rank it first, meaning that they prefer delegating decision-making to outsiders to both of the community-based decision-making processes. Preferences for local institutions are more heterogeneous: 17% rank this process first but 34% of households rank it last.

In column 2 of Table 2, we aggregate household preferences to the community level by pairwise majority rule and shows the distribution of preference profiles across communities. Almost all communities (more than 99%) collectively rank imported institutions first, implying that no community would select their local institutions if they chose institutional arrangements by (pairwise) majority rule. Most communities (88%) collectively rank decision-making by external agents last. A substantial minority (11%) of communities collectively rank their local institutions last out of the three institutions on offer.

One community has intransitive preferences as defined by pairwise majority rule, a rare real-world example of a Condorcet cycle.[26] In this community, households overwhelming rank local institutions above decision-making by external agents. However, the community is almost equally split between individuals who rank imported institutions above and below decision-making by external agents, and similarly for individuals who rank imported institutions above and below local institutions. As a consequence of these divided preferences, majority rule preferences at zero price difference are intransitive.

Preferences at varying community contribution requirements Figure 5 describes the demand curves for each pairwise choice across the full range of relative prices offered, defined as the difference between community contribution requirements. The demand curves show that preferences are more strongly polarized than can be seen from simply examining

[26]The rate of incidence of Condorcet cycles coincides surprisingly well with other estimates of how frequently Condorcet cycles occur (e.g. Tideman, 2006).

the preferences at equal subsidy levels.

The x axis shows the relative price. A relative price of zero implies that both options were offered at a community contribution of 3,000 BDT. A relative price of 2,000 BDT implies that the first option was offered with a community contribution requirement 2,000 BDT larger than the community contribution requirement associated with the second option.[27]

For each demand curve, the y intercept shows the fraction of households who choose the first option at its most favourable relative price, when it is offered at a community contribution of 2,000 BDT less than the second option. The y intercept is always less than one because some households always choose the second option, regardless of the difference in community contribution requirements. The lower the y intercept, the greater the fraction choosing the second option at any relative price. For example, looking at the *Imported vs External* curve, the y intercept is 0.93, implying that 7% of households reject the imported institutions model in favour of decision-making by external agents at any of the relative prices offered.

Demand falls as the relative price increases, but the the slope of the demand curve varies distinctly. Around the zero relative price—at which both decision-making options are offered with the same contribution requirement—the demand curve slopes sharply downwards. This shows that a group of households is highly price sensitive, choosing whichever option is cheapest unless the options are offered at the same subsidy level. The demand curves are essentially flat at more extreme relative prices.

The level of the demand curves on the right hand side of the graph measures the fraction of respondent households who always choose the first option regardless of the relative price, even when to do so will require that the community raise an additional 2,000 BDT in community contribution. For example, looking again at the *Imported vs External* curve, the demand curve levels off at 0.44, implying that 44% of households choose the imported institutions model in favour of decision-making by external agents at any of the relative prices offered.

Examining the joint distributions of the demand curves reveals further polarization. Among households, 36% always choose the cheapest option offered; 33% always choose imported institutions, regardless of the alternative or the relative price; 5% always choose local institutions; 4% always choose decision-making by external agents; and 3% always reject decision-making by the external agents when it is offered. Together, these starkly-defined preference profiles account for 81% of the study population.

We can summarize the preferences shown in Figure 5 as a pair-specific differential will-

[27] As discussed in Section 4, we randomly assigned at the community level which model was assigned to the varying price and which model was assigned to the fixed price. The demand curves pool the data, so that for each pairwise comparison the first model was offered at the varying price in half the observations and the fixed price in half the observations. A relative price of 2,000 BDT can thus correspond either to a price of 5,000 BDT compared to a price of 3,000 BDT or to a price of 3,000 BDT compared to a price of 1,000 BDT.

ingness to pay (WTP) for one decision-making approach compared to another.[28] If positive, the WTP corresponds to the amount of project subsidy a household is willing to sacrifice in order to take decisions by their preferred approach. If negative, the WTP corresponds to the amount of additional project subsidy a household would require in order to accept decision-making by the less preferred approach. Recall that the WTP does not correspond to an amount of money that the individual is willing to personally sacrifice to implement the decision-making model of their choice but instead to an amount of money that the individual is willing to sacrifice on behalf of the community. The average differential WTPs in the sample population are 790 BDT for imported institutions compared to decision-making by external agents; 772 BDT for imported institutions compared to local institutions; and 400 BDT for local institutions compared to decision-making by external agents.[29]

7.2 How reliable are our measurements of preferences?

Intransitivity A natural measure of data quality is the incidence of intransitivity in individual preferences, since true preferences are unlikely to be intransitive. As shown in column 1 of Table 2, 2.6% of households report preferences that imply intransitivity at zero price difference.[30] A larger fraction (4.6%) report unambiguously intransitive preferences when we incorporate information about relative prices.[31] At least 95% of the sample therefore report internally consistent preferences, suggesting good understanding of the elicitation procedure by both enumerators and respondents.

Stability Another natural data quality check is the consistency of reported preferences across manipulations of the elicitation process. As discussed in Section 4, we randomly assigned, at community level, which model was assigned to a fixed community contribution

[28]Appendix G provides more detail on how we calculate the WTPs.

[29]The differential WTPs are not additively transitive. Additive transitivity would require that the WTP for option A over option C be equal to the WTP for option A over option B plus the WTP for option B over option C. The primary reason is censoring of individual household WTPs. For example, say a household prefers A to B at any subsidy level, B to C at any subsidy level, and A to C at any subsidy level. These preferences are internally consistent, but our study design means that we measure WTPs of at most 2,000 BDT for each pairwise choice, when the household's preferences clearly imply that the true WTP for A vs C should be larger than the true WTP for A vs B.

[30]This is partly an artefact of our design, which did not allow households to report indifference between two options. Intransitivity at equal prices is therefore potentially consistent with true indifference between the three models i.e. a household's true preferences are $A \sim B \sim C$ but they report $A \succ B \succ C \succ A$ because we did not allow them to report indifference. In addition, as discussed in Section 6.2, we exclude from the descriptive analysis data collected by two enumerators that did not take part in the full training process and for which we have concerns about their understanding of the WTP elicitation procedure and therefore the quality of the data collected.

[31]For example, households which report preferring A to B at any contribution requirement, B to C at any contribution requirement, and C to A at any contribution requirement; or A to B at any contribution requirement, B to C at any contribution requirement, but accept whichever model has the lowest contribution requirement out of A and C.

requirement and which model to the varying contribution requirement. We find that associating a model with a varying contribution requirement substantially reduces demand for that model across the whole relative price distribution.[32] The strongest effects are seen for preferences over two alternatives at equal contribution requirements and correspond to around an 11-13% fall in relative demand for the decision-making model associated with the varying contribution requirement.

The strength of these differences is somewhat surprising. Our original concern was primarily that associating one option with a fixed contribution requirement might induce respondents to perceive that option to be the default and predispose them to choose it.[33] However, the respondent must make an active choice for each pair, implying that there is no real default answer. An alternative possibility is that the first response anchors the responses throughout the rest of the elicitation. We began the elicitation with the model associated with the varying option presented at its highest relative price. Since households are more likely, on average, to reject models associated with higher relative prices, households may have anchored on this first decision throughout the elicitation procedure. The strong effects at equal relative prices suggest that a substantial proportion of the response to framing occurs in households which are actually indifferent between the two options.

These effects show that at least some of the study households report preferences in a way that is sensitive to framing, although the large majority of households do not seem to respond to which model is associated with the varying or fixed contribution requirement. Since we randomly assigned which option was assigned to the varying contribution requirement for each choice, these framing effects do not affect the average preferences we describe. We discuss how we deal with these framing effects in the impact evaluation in Section 10.1.

Beliefs about pivotality The incentive for households to truly reveal preferences in this study depends on whether or not they believe there is a chance that their choices are pivotal. To evaluate whether this is the case, we ask households about their beliefs regarding two measures of pivotality. We ask each household one question about pivotality beliefs for one randomly-extracted choice between decision-making processes and community contribution requirements.

The first measure of pivotality asks whether or not households agree with the statement that their choice could be decisive in determining how the project gets implemented, a measure of pivotality previously used in Duffy and Tavits (2008). A very large majority of households either agree (75%) or strongly agree (14%) with this statement, perhaps because in the introduction to the WTP elicitation enumerators stress that household choices can influence how extra projects will be implemented, that every vote will be counted, and that

[32]Appendix Figure L1 shows the effects on average WTP and on demand at equal prices; Appendix Figure L2 shows effects across the whole price distribution.

[33]For evidence on default effects, see e.g. Madrian and Shea (2001).

household choices are important. The second measure of pivotality asks households how closely they think the choice will be determined in their community.[34] In contrast to the reported beliefs on pivotality, only a small fraction of households (8%) believe the choice will be closely determined in their community. Instead, the large majority believe that the community will strongly favour one option or the other. Beliefs about closeness are correlated with beliefs about pivotality, and both are correlated, albeit more weakly, with actual marginality in the decision taken.[35]

These results provide tentative support for the validity of our approach in terms of eliciting truthful revelation of preferences, although they also suggest that households may not be very good at predicting the choices of other households in their communities. Such uncertainty is not necessarily a disadvantage: with more uncertainty about aggregate preferences, each individual voter is more likely to believe that there is some positive probability that their vote will be pivotal.

Reporting bias To evaluate the extent to which incentivization affects reporting of preferences, we randomly assigned households to one of two incentivization treatments: either we informed them *before* the elicitation procedure that their choices could have real-world consequences, or we conducted the elicitation procedure as a hypothetical exercise and only informed them afterwards that their choices could have real-world consequences. For the latter group, we then offered them the opportunity to revise their preferences in the light of that information.[36]

We find only a limited response to incentivization. Households who knew their choices had real-world consequences at the time of responding were weakly more likely to favour local institutions over both imported institutions and decision-making by an external agent. Mean effects on WTP and demand at equal relative prices are not statistically significant. However, households who knew their choices would have real-world consequences were modestly more likely to prefer local institutions to imported institutions and decision-making by external agents at any relative price.[37] These results suggest that the effect of incentivization—if anything—is to decrease over-reporting of preferences for the institutional arrangements associated with our local partner (the imported institutions model and decision-making by external agents, who are NGO staff).

The fact that the response to incentivization was modest may suggest only a limited

[34]Specifically, we ask whether they believe that a large majority of households will favour one option over the other, or whether groups of approximately equal size will favour the two options.

[35]See Appendix H for more details.

[36]No household who was informed that their decisions could have real-world consequences after replying to the elicitation procedure wished to revise their answers. Few households overall reported preferences but declined to have their preferences used to determine outcomes for their communities (0.16%) and that rate did not respond to whether or not households reported their preferences before or after incentivization.

[37]Appendix Figure L1 shows the mean effects; Appendix Figure L3 shows the effect over the full range of relative prices.

degree of reporting bias. However, the effect of incentivization on reporting is only a lower bound on the extent of reporting bias. Not all households will respond to incentivization, and the modest response could also be consistent with households not caring much about the incentives at stake.[38] The limited response to incentivization does however rule out the possibility that reported preferences are affected by any priming effect of discussing aggregation of preferences by majority rule. In the evaluation section of this paper, we will test whether the response to incentivization changes in response to exposure to democratic decision-making via the first wave of safe drinking water projects.

Time audit data and error rates We recorded time audit data during the elicitation which enables us to track the time taken for each step of the process. We also coded error messages which appeared when respondents gave answers that suggested unusual preference reversals.[39] The time audit data reveals whether or not these error messages were triggered. Error messages were triggered in less than 1% of elicitation procedures, suggesting that households usually gave internally consistent responses from the beginning. These data also show that enumerators improved performance with experience, recording lower rates of intransitive preferences over time. On the other hand, we reassuringly do not find any systematic relationship between experience and other response characteristics.[40]

7.3 How do preferences vary with household characteristics?

We can distinguish between two groups of potential interest in terms of understanding how preferences vary across individuals. The first group consists of those whom we would ex-ante expect to be marginalized in community decision-making processes. The second group consists of those whom we would expect to be influential in those same decision-making processes.

Figure 6 shows how individuals in these groups compare to the rest of the sample with respect to both WTPs and preferences when decision-making processes are offered at equal contribution requirements.[41] Each point estimate shown describes how households in a given group differ from the remainder of households. Each estimate is from a separate regression. All characteristics we examine are either predetermined (e.g. religion, gender, class) or measured using baseline data.[42]

[38]Alternatively, enumerators might not have strictly followed the scripts they were provided with, meaning that there may have been limited compliance with the experimental variation in which incentivization took place before or after the WTP elicitation. The time audit data is however consistent with enumerators correctly following instructions.

[39]For example, if they selected option A when its relative price was 2,000 BDT but option B when the relative price of option A was 1,000 BDT.

[40]See Appendix Tables L1 and L2.

[41]Appendix Figures L4 to L11 show differences in preferences across the full distribution of relative prices for each of the comparisons shown in Figure 6.

[42]The characteristics we discuss in this section are correlated with one another and with other household

Potentially marginalized groups Potentially marginalized groups almost all report lower WTP across all decision-making pairs, where we construct the pairs so that a more positive WTP implies a higher value of autonomy and/or democracy. However, they differ as to whether the lower WTPs reflect higher sensitivity to community contribution requirements or weaker preferences for autonomy and democracy.

Female respondents, despite considerably lower WTPs, report largely similar preferences to men at equal contribution requirements, although they are more likely than male respondents to prefer delegating decision-making to external agents when the alternative is local institutions.[43] Women are less likely to hold extreme preferences (choose one model at any relative price) for either extreme position across all three pairwise choices (Appendix Figure L4).[44] The lower incidence of extreme preference types suggests that differences in WTP across genders arise primarily from lower willingness to give up subsidies rather than different preference rankings across decision-making processes. One piece of anecdotal evidence provides a possible explanation: in one community where we implemented one of the first wave of safe drinking water projects, women took a very active role in decision-making and location selection, but men then refused to raise the cash contribution, meaning that the project ultimately failed. If women do not control household finances, and controlling finances is necessary to implement desired decisions, then this may explain their sensitivity to changes in subsidy level.

Poor households,[45] unlike women, show systematically different preferences across the full range of relative price levels (Appendix Figure L5). Across all relative price levels, poor households are more likely to be willing to delegate to an external agent and are also more likely to prefer local institutions to imported institutions. Poor households may anticipate having limited de facto influence over community decisions under either local or imported institutions and may therefore prefer not to incur the costs of participation in the consensus-based approach. Alternatively, the social or time costs of participating in imported institutions may be higher for the poor.

Households whose household heads did not complete any education are more or almost

characteristics. Appendix Table L3 tabulates the pairwise relationships between these characteristics and Appendix Table L4 shows how they correlate with other household characteristics measured in our survey data.

[43] We did not specifically target gender of the respondent. Households where women respond to the survey and the elicitation are poorer and less likely to be leaders or involved in community decision-making.

[44] The lower average WTPs shown in Figure 6 arise from changes in the distribution of these extreme preference types. For example, around 2% fewer women than men choose external institutions over imported institutions at any relative price and around 7% fewer women than men choose imported institutions over external institutions. The increase in the extremes is balanced by a relative increase in the fraction of women who choose the cheapest option regardless of relative price. The overall decline in WTP is driven by the fact that the decrease in those who strongly prefer external institutions to imported institutions is smaller than the decrease in those who strongly prefer imported institutions over external institutions.

[45] Defined as those who self-identify as poor or very poor, although results are similar if we define poverty relative to an assets index.

equally willing to delegate to external agents across all prices (Appendix Figure L6). They have similar preferences to educated households at equal subsidy levels over local and imported institutions but are less likely to have extreme preferences in either direction.

Households belonging to a religious minority in their community,[46] unlike other potentially marginalized groups, do not exhibit lower WTPs nor indeed any significantly different preferences across institutions, at any subsidy levels. Also unlike other marginalized groups, they do not systematically differ from the rest of the population on most observable characteristics, although they are less likely to self-identify as high income.

Potentially influential groups All potentially influential groups exhibit stronger than average preferences for autonomy. However, they differ regarding whether or not they prefer constraints on the decision-making process. Households who self-identify as high income are more likely to prefer to retain local institutions than adopt imported institutions.[47] In contrast, community leaders — those that at least two other households reported at baseline to be leaders in the community — prefer imported institutions to local institutions. Network focal households—those whom at least three households in our baseline sample reported as being part of their social networks—have similar preferences to non-network focal households, although they are less willing to delegate to external agents when imported institutions are the alternative. Those who are involved in decision-making at baseline exhibit similar qualitative patterns to community leaders.

There are several potential explanations for these heterogeneous preferences regarding democratic institutions. There may be multiple types of influential individuals in the community, including both benevolent elites, who favour more inclusive institutions, and controlling elites, who prefer traditional decision-making processes over which they can exert influence. Potentially influential individuals could also have different beliefs about how constraints on decision-making in the imported institutions model actually affect their influence. Some types of influential individuals could even believe that they stand a better chance of influencing outcomes under imported institutions than under local institutions, if local institutions are dominated by traditional elites.

7.4 What explains household preferences?

To understand households' preferences over the three alternative decision-making processes, we elicit beliefs about expected private benefit and expected social benefit under each approach to decision-making. We proxy for private benefit with the distance between the

[46] These are primarily non-Muslim households in majority Muslim communities, although some are Muslim households in majority non-Muslim communities.

[47] About 3% of households self-report as high income. Households who belong to the top 5% of wealth as measured by an asset index have preferences that are closer to those of community leaders and network focal points.

household and the site they believe will be chosen under a given decision-making process for the new water source, as well the amount that the household would expect to contribute individually towards the community contribution under that decision-making process. We proxy for social benefit using a measure of the relative potential social benefit that could be realised if a well were installed in the expected site.

A key input is household beliefs about which locations would be chosen under a given decision-making process. For each household, we have reliable data about their beliefs about expected location under one decision-making process. Households identify expected locations by placing a sticker on a laminated map, generated using satellite imagery superimposed with local landmarks. Enumerators then record the expected location by placing a pin on an interactive map interface.[48] We also ask households how much they would expect to contribute towards the community contribution requirement under a given decision-making process.

Figure 7 summarizes beliefs about expected private benefit, and how they vary across decision-making processes and with preferences. To evaluate how they correlate with preferences, we categorize households into five mutually exclusive groups depending on the strength of their preferences for a given decision-making process.[49]

The median distance between a household and the expected location for a well is 54m, corresponding to less than a minute in walking time. Averaged across decision-making processes, both the median and modal household report expecting to contribute 100 BDT when the required community contribution takes its baseline value of 3,000 BDT.[50]

Expected distance to the well varies little across decision-making processes, perhaps because of the zero sum nature of the location decision: a well that is nearer to one household is further from another. There are no significant correlations between the amount households expect to contribute and the decision-making process.

It is natural to expect households who prefer a decision-making process to expect that decision-making process to yield well locations closer to their home. This is in general true, with the systematic exception of those who strongly prefer a given model. These households expect more distant locations. We interpret this as indicating that factors other than individual expected distance to the site play a role in determining these strong preferences.

[48]We use these maps throughout the household survey, with households initially identifying their homes on the maps to ensure that they orientate themselves correctly.

[49]The five groups are: those who strongly prefer a model, meaning that they prefer it to both other models at any subsidy level; those who weakly prefer a model, meaning that they rank it first out of the three at equal subsidy levels but not at all subsidy levels; those who we label as having mixed preferences, meaning that they rank the model in the middle of the three; those who weakly dislike the model, ranking it last; and those who strongly dislike a model, meaning they prefer any other model offered at any subsidy level.

[50]Few households (less than 0.5%) report planning not to contribute at all, suggesting some social pressure to reporting intending to contribute, and a similarly small proportion of households report expecting to pay the full contribution. In contrast, relatively few households actually contributed during the first wave of water source installation projects; the mean number of cash contributors was 12.5 households per water source.

On the other hand, it is ambiguous how expected household contributions towards a fixed community contribution requirement should correlate with preferences. Households may be willing to contribute more if they expect a decision-making process to yield a preferred location or under a decision-making process that they prefer for other reasons.[51] Conversely, households may prefer a decision-making process because they think it will induce others to contribute, meaning that they will need to contribute less. We find that households who strongly prefer a process are willing to contribute more and those who strongly dislike a process expect to contribute less. However, households who weakly prefer a model expect to contribute less than households who weakly dislike a model or have mixed preferences towards it.[52]

Our proxy measures of social benefit compare predicted take-up at the expected location to the distribution of predicted take-up across the full range of potential sites in the community.To be specific, we use a revealed preference approach to predict take-up if a well were constructed in every possible location choice in a given community, across a 20m grid.[53] We then compare the predicted number of users at the household's expected location to the distribution of predicted numbers of users, constructing two metrics: a measure which captures the percentage of the maximum possible predicted number of users achieved by the household's expected location; and ii) a rank measure, which takes the value 1 if a household's expected location has the highest possible predicted number of users in the community and 0 if the household's expected location has the lowest possible predicted number of users in the community.

Our proxy measures of social benefit are imprecise, in that they assume that take-up rates are homogeneous regardless of land ownership or other household characteristics apart from exposure to arsenic contamination. They also do not account for the fact that households may place different weights on different households in the community. However, they have the desirable property that they are invariant to changes in maximum feasible social benefit, which might vary as a direct consequence of treatment under the first wave of safe drinking water projects.

Figure 8 summarizes beliefs about expected social benefit. Neither measure varies systematically across decision-making process. Nor is higher expected social benefit systematically correlated with stronger preferences for a given decision-making process. Indeed, for both local institutions and external institutions, the relationship between expected social benefit is U-shaped. Households may expect local institutions to deliver populist outcomes, while ex-

[51]Households who expect wells to be built closer to their home report expecting to contribute more towards the community contribution requirement. See Appendix Figure H5.

[52]An alternative explanation for this pattern is that richer households tend to have more extreme preferences and also report expecting to contribute more.

[53]We predict take-up using observed take-up rates from wells installed during the first wave of safe drinking water projects, accounting for variation in take-up rates with distance to the well and baseline arsenic contamination. See Appendix H3 for more details.

pecting external agents to favour smaller, possibly more marginalized groups. Whether these beliefs translate into preferences for or against these institutions may depend on whether a household belongs to the majority or to the minority.

Figures 9 and 10 show how beliefs about private and social benefit with household characteristics. Beliefs about distance between the household and the expected location do not systematically vary with household characteristics, with the exception of women, who expect more distant locations under imported institutions, and religious minorities, who expect closer locations under external agents. Potentially marginalized groups expect to contribute less and potentially influential groups expect to contribute more towards the community contribution. Potentially marginalized groups expect relatively limited variation in social benefit with decision-making processes, although religious minorities expect lower social benefit under external agents. Potentially influential groups, in contrast, mostly expect local institutions to yield locations with lower social benefit than the two alternatives, and the differences are strongest for high income households. These results suggest differences between marginalized and influential groups in the extent to which they expect different decision-making processes to yield different outcomes.

Beliefs about household and social benefit do not clearly explain preferences across different decision-making processes or heterogeneity in these preferences across groups. For example, households who strongly prefer imported institutions do not expect them to yield higher social benefit and expect them to yield lower private benefit. Religious minorities also do not systematically prefer decision-making by external agents to other decision-making processes, despite expecting them to yield locations which are closer to their own households. With the framework in Section 3 in mind, these results suggest that preferences over decision-making processes are likely to be influenced by the net intrinsic value associated with the decision-making process itself.

8 Balance tests

We randomly assigned 3 out of every 4 communities originally enrolled in the study to receive the first wave of safe drinking water projects at public lottery meetings. We held one public lottery meeting in each of eight unions, and two public lottery meetings in one large union. Assignment to treatment is thus stratified by union. We invited representatives from study communities and local government to the meetings. We used public lottery meetings to assign treatment to ensure transparency and legitimacy for project staff. Assigning treatment by public lottery during the first wave also has an additional advantage in the context of this study, which is that it provides communities with salient recent experience with random assignment. This experience makes it easier for households to understand what is at stake during the elicitation procedure.

Figure 3 shows that the distribution of treatment and control communities is geographically well balanced. Recall that we could not implement the elicitation procedure in 6 communities in the treatment arm, because local hydrogeological conditions prevented installation of tubewells. Asking households in these communities about a future project—which would also likely fail—was not a meaningful exercise. Our main strategy for dealing with these excluded communities is to apply weights in communities where we did not attempt installation corresponding to the probability of at least one successful installation, as we describe in Section 6.2.

In Appendix I, we confirm that treatment and control communities for which we have data on preferences over decision-making processes are statistically equivalent at baseline, after weighting to account for the probability of tubewell installation failure.[54] The statistical equivalence at baseline suggests both that the initial randomization was correctly implemented and further that the approach we use to deal with missing data from the 6 communities where tubewells were not feasible successfully maintains balance between treatment and control. As a result, we can compare preferences in communities who received the first wave of safe drinking water projects to preferences in communities who did not receive the first wave of safe drinking water projects, and attribute any differences to the causal effect of treatment.

9 Hypotheses

9.1 Main hypotheses

We test two hypotheses which evaluate how exposure to democratic and inclusive decision-making affects preferences over decision-making processes. Our first hypothesis is most central to our overarching research goal—shedding light on the reasons for institutional persistence—and posits that:

Hypothesis 1 (H1) *Exposure to democratic and inclusive decision-making increases the value of imported institutions relative to local institutions.*

We interpret evidence in support of H1 as indicating that exposure to democratic and inclusive decision-making increases the value that agents place on constraining community decision-making with rules designed to ensure inclusiveness and limit elite capture (value of democracy). H1 may not hold if, instead, exposure to democratic decision-making leads agents to learn about the costs of the consensus-based approach in terms of effort or conflict, or about the difficulty of constraining elite capture. In this case, the relative value of local

[54]By design, we do not have baseline data from all households included in the elicitation procedure. This is because the follow-up sample oversamples populations of interest, and we used baseline information as well as implementation records to identify the populations of interest. Among the households surveyed at baseline, attrition rates were very low and did not vary with treatment status (Cocciolo et al., 2019a).

institutions could increase. We discuss how we will interpret the results of H1 further in Section 9.5.

Our second hypothesis tests another mechanism via which exposure to democratic and inclusive decision-making could affect preferences over decision-making processes. The consensus-based approach to decision-making also features more autonomy than is normal in the local context. Exposure to democratic institutions could thus lead agents to place more value on retaining community decision rights. We thus also posit that:

Hypothesis 2 (H2) *Exposure to democratic and inclusive decision-making increases the value of imported institutions relative to decision-making by external agents.*

We interpret evidence in support of H2 as indicating that exposure to democratic and inclusive decision-making increases the value that agents place on retaining community decision rights (value of autonomy). H2 may not hold if, instead, exposure to democratic decision-making leads agents to learn about the costs of the consensus-based approach or the difficulty of constraining elite capture, or to change their beliefs about the quality of decisions taken by external agents. In these cases, the relative value of local institutions or decision-making by the external agents could increase.

We also measure pairwise preferences for local institutions over decision-making by external agents. If both H1 and H2 hold, the effect of exposure to democratic decision-making on these preferences is ambiguous. We will additionally report this effect, because the sign of this effect will provide information regarding the relative strengths of the effects estimated when we test H1 and H2. For example, if we find that the value of local institutions rises relative to decision-making by external agents, this suggests that the effect on the value of autonomy is greater than the effect on the value of democracy.

The main variables of interest are the pairwise WTP measures obtained from the elicitation procedure, representing the amount of project subsidy households are willing to sacrifice, or additional project subsidy they would require, to be indifferent between two different decision-making processes, calculated as discussed in Appendix G.

9.2 Secondary outcomes

Changes in distribution of preferences Effects of exposure to democratic and inclusive decision-making may be heterogeneous if H2 or H1 holds for some community members, but other community members reduce their WTP for imported institutions, for example via learning about their costs or limitations. If effects are polarizing, it is possible that effects on some households could be substantial but the net effect on WTP could be zero.[55]

[55] For example, if 10% of households switch their WTP from near zero to -3,000 BDT and 10% of households switch their WTP from near zero to 3,000 BDT.

We will therefore make one additional, more flexible test to evaluate changes in the distribution of WTPs. The test has two stages. First, we will conduct a Kolmogorov-Smirnov (K-S) test for equality across the demand curves. The K-S statistic corresponds in this instance to the maximum difference in demands between treated and control groups at any relative price point. Second, if we reject either of the primary hypotheses or the K-S tests at the 10% level for a given pairwise comparison, we will examine the full distribution of effects across all relative price points.

Beliefs about household and social benefits To provide suggestive evidence regarding the drivers of preferences over decision-making processes, we will evaluate effects on expected household and social benefit. In particular, we test whether exposure to democratic institutions results in systematic changes in expected household or social benefit.

To proxy for effects on expected household benefit, we measure the distance between the household and the site they believe will be chosen under a given decision-making process for the new water source,[56] as well as the amount they expect to contribute individually towards the community contribution under that decision-making process. To proxy for changes in expected social benefit, we evaluate how the expected location compares to locations chosen to maximize the expected number of users, using the measures we discuss in Section 7.4.

We will interpret these results in light of the framework in Section 3. If we find an effect of exposure to democratic institutions on preferences for decision-making processes, but no corresponding effect on expected private or social benefit, we will conclude that the most likely explanation for the change in preferences concerns changes in the net intrinsic value placed on the decision-making process itself.

Response to incentivization We will evaluate whether exposure to democratic decision-making affects the response to incentivization. Specifically, we will test whether the effect of assignment to incentivization before or after the elicitation procedure varies between treated and control communities. However, we will be cautious in interpreting the results of these tests. A larger effect of incentivization is compatible with a greater extent of reporting bias. On the other hand, a larger effect of incentivization could also imply less reporting bias if higher reporting bias is associated with lower responsiveness to incentives. Alternatively, treatment could affect the salience of the incentives. We will interpret the results of these analyses with these different potential explanations in mind.

[56] In our analysis, we will use the log distance, to reduce sensitivity to outliers in measured distance which arise through errors in the GPS measurements.

9.3 Heterogeneous effects

The effects of exposure to democratic decision-making may be heterogeneous across groups. In particular, the effects may differ between potentially marginalized and potentially influential groups. Which groups respond and how they respond is important for interpreting the results in terms of our original goal, to shed light on the reasons for institutional persistence, as we discuss in Section 9.5.

We will test heterogeneity in the effects corresponding to the main hypotheses using WTP and a K-S test for the eight groups—comprising four potentially marginalized groups and four potentially influential groups—identified in Section 7.3. In each case, we will test the null hypothesis that the effect in the group of interest is equal to the effect in the rest of the population.

For those groups for whom we reject this null hypothesis at the 10% level, either via the WTP comparison or the K-S test, we will evaluate heterogeneous effects across the full distribution of relative prices. We will also evaluate whether there are heterogeneous changes in beliefs among these groups.

9.4 Exploratory analyses

We expect to carry out additional exploratory analyses which we will use to interpret the results of the pre-specified analyses. We will clearly identify all exploratory analyses in the final report.

These may include evaluations of other experimental treatments implemented in the study communities during the first wave of safe drinking water projects. For example, treated communities were randomly assigned to three contribution requirements: (i) cash contributions, where communities are required to co-fund the installation costs; (ii) labour contributions, where communities are required to provide labour during installation, equivalent in nominal value to the required cash contribution; (iii) a contribution waiver, where the new water source is installed for free.[57] The experimental comparison between the cash contribution group and the other treatment arms is potentially illuminating, because take-up was much lower under the cash contribution arm than under the other two treatment arms, despite the labour contribution having the same nominal value as the cash contribution arm. Comparing the cash contribution arm to the other two treatment arms may help us understand whether the effects of exposure to democratic decision-making differ depending on the eventual outcomes of the decision-making process, or whether learning about the difficulty of raising community cash contributions makes respondents more sensitive to changes in subsidy level.

[57] In a separate study (Cocciolo et al., 2019a), we evaluate the impact of community contributions on the decision-making process, the quality of decisions taken by the communities, use of safe water sources and household water quality.

Further, in a cross-randomized treatment relative to the first wave of safe drinking water projects, our project staff provided non-binding tubewell location suggestions at the start of the community meetings before opening the floor to more general discussions. We can therefore test how beliefs and valuation of decision-making processes vary with past exposure to decisions taken by our project staff.

We also have additional rich data on baseline community and household-level characteristics. In particular, we have rich geospatial data on the locations of households relative to local arsenic contamination patterns and relative to meeting sites. Using these data, we can generate predictions of chosen installation sites and meeting sites under the first wave of safe drinking water projects. These predictions allow us to create instruments for different types of exposure to the democratic and inclusive institutions adopted under the first wave of safe drinking water projects, and therefore evaluate heterogeneous effects along these dimensions.

9.5 Understanding institutional persistence

We will interpret our results in the light of the puzzle we outlined in the introduction and the conceptual framework in Section 3: exposure to democratic institutions appears to increase reported pro-social preferences, but does not appear to translate into institutional change. We outlined three possible reasons. First, apparent changes in preferences may be artefacts of reporting bias. Second, changes in preferences may be real but insufficient to change the preference ordering of local and imported institutions. Third, changes in preferences may be sufficient to change the preference ordering of local and imported institutions, but those changes in preferences may not outweigh the costs of institutional change for a coalition of individuals sufficiently large to realise institutional change. In particular, the cost of displacing elite control may be prohibitively high for those outside of the ruling elites. We will evaluate whether our evidence supports these three explanations as follows.

First, we evaluate to what extent reported preferences appear to be affected by experimenter demand effects. We compare incentivized to non-incentivized reported preferences and evaluate whether this relationship changes in response to exposure to democratic institutions, as discussed in Section 9.2, subject to the caveats outlined in that section. If we find changes in non-incentivized preferences in response to exposure to democratic institutions, but not in incentivized preferences, we will conclude that reporting bias may be a key factor in explaining the puzzle.

Second, we evaluate whether exposure to democratic institutions increases demand for imported institutions and whether any increase in demand is sufficient to change the ranking of preferences for local and imported institutions for any community members. Specifically, returning to the conceptual framework outlined in Section 3, we measure whether $V_{i,imp} > V_{i,loc}$ by examining which decision-making process households choose when the decision-making processes are offered at the same community contribution requirement. We also measure a proxy for the intensity of preferences $V_{i,imp} - V_{i,loc}$ by estimating the differential WTP between the two decision-making processes. We evaluate whether previous exposure to democratic and inclusive institutions changes preferences favour of imported institutions by testing H1. If preferences do not change in favour of imported institutions or the preference change is insufficiently large to reverse the average order of ranking of $V_{i,imp}$ and $V_{i,loc}$, we will conclude that preference changes are simply too small to affect rank ordering of institutions.

Third, if we find preference changes in favour of imported institutions, we will combine these results with our descriptive results to understand whether the results are consistent with changes in preferences being insufficient to outweigh costs of institutional change. First, we will evaluate whether local institutions reflect majority preferences or not i.e. what percentage of each community prefers the imported institutions model to local institutions. Second, we will evaluate which individuals change their preferences in response to exposure to democratic institutions, using the heterogeneity analyses described in Section 9.3. Together, these analyses will allow us to learn who appears to control local institutions and whether changes in preferences are concentrated among these individuals, or others. If local institutions do not reflect majority preferences and increased demand for imported institutions is concentrated among groups whose preferences are not reflected in local institutions, we will interpret this as evidence that the main explanation for institutional persistence relates to the costs of institutional change, and in particular, to the costs of displacing elite control of local institutions.

If we observe other constellations of results that we did not anticipate, we will indicate that the pattern of results was unexpected and we will interpret the results in the light of the conceptual framework in Section 3. Our results may not be conclusive.

10 Empirical analysis

10.1 Estimation of treatment effects

We will test H1 and H2 using the following specification:

$$y_{hc} = \alpha + \beta T_c + \boldsymbol{Z_c \gamma} + \epsilon_{hc} \tag{3}$$

where y_{hc} is an outcome variable of household h in community c for imported institutions relative to either local institutions (H1) or to decision-making by external agents (H2); T_c is an indicator for assignment to the first wave of safe drinking water projects; $\boldsymbol{Z_c \gamma}$ is a vector of stratification controls; and β is the main coefficient of interest. We will test our main hypotheses H1 and H2 using two-sided tests on the main coefficient of interest β.

The vector of control variables, $\boldsymbol{Z_c}$, contains lottery-union fixed effects, which account for stratification in assignment to treatment, and a dummy variable for whether or not the elicitation listed imported institutions with a fixed or varying price. We include this additional dummy variable because this condition was randomly assigned at the community level, but not stratified with respect to treatment. Following Lin (2013), Imbens and Rubin (2015) and Gibbons et al. (forthcoming), we demean all the variables in $\boldsymbol{Z_c}$ and include the interaction between the controls and the treatment dummy, meaning that β estimates the average difference between treated and control villages.

The exact nature of exposure to democratic decision-making depends on: (i) the community decision on whether to receive the safe water source intervention; (ii) individual and household decisions about participation in 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 will provide the intent-to-treat effect.

We use the same equation to test for differences in secondary outcomes. To construct the K-S statistics accounting for stratification and fixed/varying prices, we estimate the differences in demands at every relative price using Equation 3 and take the maximum absolute value of these differences across all nine relative prices.

Inference As treatment is assigned at the community level, we report standard errors clustered by community. The number of clusters in the control group with respect to the first wave of safe drinking water projects is relatively small (42). We therefore conduct inference by randomization-based inference (RBI). Specifically, we reshuffle treatment status at each treatment assignment lottery 2,000 times and estimate the distribution of $\hat{\beta}$ under the null hypothesis of no treatment effect. We compare each estimated $\hat{\beta}$ under the observed treatment assignment to the distribution of the $\hat{\beta}$ under the null to generate p values. Similarly, we construct the distribution of the K-S statistic under the null hypothesis and compare the K-S statistic under the observed treatment assignment to this distribution.

Sample restrictions As we note in Section 6.2, a subset of households record unambiguously intransitive preferences. These households appear not to have understood the elicitation procedure or to have been interviewed by an enumerator who may not have understood the elicitation procedure correctly. Including these households in the analyses likely attenuates the estimated effects, since their responses are unlikely to constitute meaningful information, making it more difficult to distinguish the treatment effect from zero. In the full sample, there are 11% of such households, implying a non-trivial attenuation of the main effects with no corresponding increase in power.

We will test whether the rates of intransitivity differ by treatment status. If the rates of transitivity do not differ by treatment status, we will exclude these households from the main analysis. To be conservative, we will evaluate if the rates of transitivity differ by treatment status using a threshold p value of 0.2. We note that we do not expect rates of intransitivity to respond to treatment, because rates of intransitivity do not systematically vary with household characteristics or with experimental variations in the elicitation process (Appendix D5). However, if the rates of intransitivity do differ by treatment status, we will include them in the main analysis and report our main results both including and excluding the households with intransitive preferences.

Weights We use weights as described in Section 6.2 and in more detail in Appendix F. These weights account for oversampling of some populations, for any potential imbalance caused by excluding the 6 treated communities where we attempted to install wells and failed because of local hydrogeological conditions, and otherwise ensure that all study communities count equally in summary statistics.

10.2 Multiple hypothesis testing

Our main concern with multiple hypothesis testing is in our heterogeneity analyses, where we test for heterogeneous effects across four sets of potentially marginalized individuals and four sets of potentially influential individuals. Further, the household characteristics on which we divide the sample are correlated. We therefore additionally report p values that control the family wise error rate within each of the groups (potentially marginalized individuals and potentially influential individuals), following Westfall and Young (1993).

10.3 Power calculations

We take two approaches to power calculations. We summarize the results here and provide further details in Appendix J. First, we use the analytical expression reported in Hemming et al. (2011) for a randomized evaluation with a fixed number of individuals per cluster and a different number of clusters per treatment arm. Using the real intra-cluster correlation in the dependent variables of interest, we calculate minimum detectable effects (MDEs) for hypotheses H1 and H2 of respectively, 131 and 122 BDT. These MDEs correspond to effect sizes of 0.11 and 0.10 times the standard deviations of the outcomes, respectively. These MDEs are considerably smaller than those we anticipated when we designed the study, because the intracluster correlations are lower than we expected.

The second approach we take uses our real data, simulates assignment to treatment and a plausible treatment effect 500 times,[58] in each case estimating the parameter β and the K-S statistic from the specification described in Section 10.1. The empirical distribution of $\hat{\beta}$ follows a standard t-student distribution, and therefore we calculate the MDEs using the empirical standard deviations of $\hat{\beta}$. Intra-cluster correlation is modelled implicitly via the true intra-cluster correlation in the follow-up data. To estimate the MDEs using the K-S

[58] We used effect sizes from Cocciolo (2019).

statistics, we also simulate the distribution of the test statistic under the null. We obtain the MDEs using the thresholds for 95% significance level and for 80% power from, respectively, the simulated distribution under the null and under the expected effect size. We use these thresholds in order to calculate the MDEs. The MDEs for H1 and H2 are, respectively, 153 and 145 BDT. The analysis based on the K-S statistics has MDEs of 5 percentage points, anywhere in the distribution of relative prices.[59]

10.4 Robustness checks

We pre-specify six robustness checks we expect to carry out with respect to the main hypotheses.

First, we note in our data quality checks that data quality varies across enumerators. We will replicate our main results including enumerator fixed effects to rule out the possibility that enumerator quality could be correlated with treatment.

Second, our main analysis addresses the exclusion of treatment units where tubewells are not feasible using balancing weights. We will replicate our main results using an alternative approach to this problem by simply excluding the union where 5 out of 6 exclusions were made.[60]

Third, we will omit the oversampled populations and estimate effects only in the simple random sample of the population.

Fourth, we will estimate the main effects using control variables, using the Lasso algorithm to select the optimal set of controls i.e. the set of controls that minimizes the standard errors of the estimated treatment effect.[61]

Fifth, we will implement an alternative approach to dealing with the intransitive cases in our WTP data. Because we elicit the full set of pairwise measures of WTP over all three decision-making processes, we can use these data to construct two alternative measures of WTP over any pair of decision-making processes: 1) we can construct the "direct" WTP by using the reported preferences for any process A over any other process B; and 2) we can infer the "indirect" WTP by using the reported preferences for A over the third process C, and the reported preferences for C over B. We then combine the direct and indirect measures of WTP to create an averaged measure of WTP.[62] The averaged measure of WTP reduces the extent of transitivity. We will replicate our main results using the full sample, without excluding intransitive preferences, with this alternative measure of WTP.

[59]We report conservative power calculations in this section based on the sample excluding households with unambiguously intransitive preferences.

[60]In Appendix I, we show that balance between treatment and control units is similar under this alternative approach. In this analysis, we will omit the balancing weights.

[61]We report in Appendix K the list of controls included in the Lasso algorithm.

[62]We will censor the inferred indirect WTP at an absolute value of 2000 BDT, for comparability with the direct measure of WTP, which is censored at an absolute value of 2000 BDT by construction.

Finally, we will report results which include the households with intransitive households, without the adjustment we describe in the previous paragraph, if we exclude them from the main analysis. As noted above, if the rate of intransitivity varies between treatment groups, we will present our main results both with and without the intransitive households.

11 Discussion

In this study, we measure individual preferences over different decision-making approaches for a future project to provide a safe source of drinking water for communities in rural Bangladesh. We measure preferences over three different decision-making processes: 1) decision-making by **external agents**, who are employees of our partner NGO, and who while ostensibly benevolent may lack information that the community has or have a different objective function; 2) decision-making using pre-existing **local institutions**, which are typically non-democratic, and often feature private decision-making by small numbers of local elites; and 3) decision-making using **imported institutions**, specifically a consensus-based approach to decision-making which imposes representation requirements and nominally broadens both participation and influence over decisions taken.

This document has described the elicitation process we used to measure individual preferences. We incentivize truthful revelation of preferences by using people's choices to determine how to implement a real project in a randomly extracted subset of communities. We find widespread preferences both for autonomy (choosing community decision-making approaches over delegating decisions to external agents) and for democratic and inclusive approaches to decision-making, although these preferences are more polarized. Wealthy elites are more likely to prefer non-inclusive institutions, although other types of community leaders support inclusive approaches. Marginalized groups are more likely to prefer to delegate decision-making to external agents.

We will also use these data to measure the effects of exposure democratic and inclusive institutions via an earlier wave of safe drinking water projects, implemented under the imported institutions described above. We randomly assigned the earlier wave of safe drinking water projects to study communities, enabling us to compare exposed communities to non-exposed communities and infer that differences between the two arise because of the causal effect of exposure. This document pre-specifies our approach to that analysis.

This study focuses on a specific example of a social problem, solving a local collective action problem to provide a safe source of drinking water, in a specific context, rural communities in Bangladesh. Whether households prefer different decision-making processes for different types of decisions and whether different types of communities prefer different decision-making processes will remain open questions, which we leave for future research.

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Figures

Figure 1a: Decision-making by External Agents

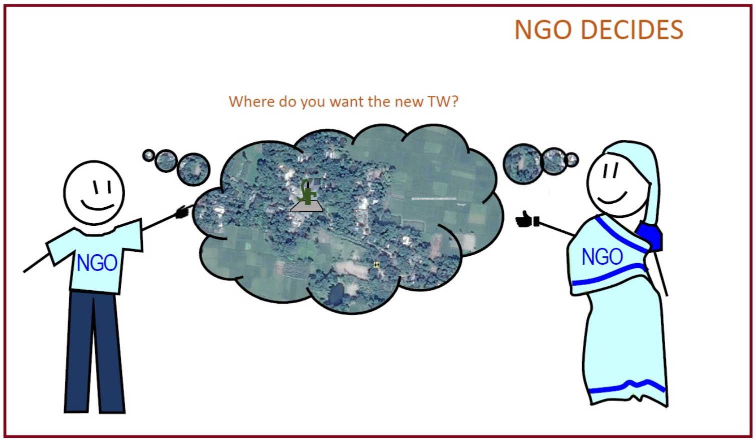


Figure 1b: Local Institutions

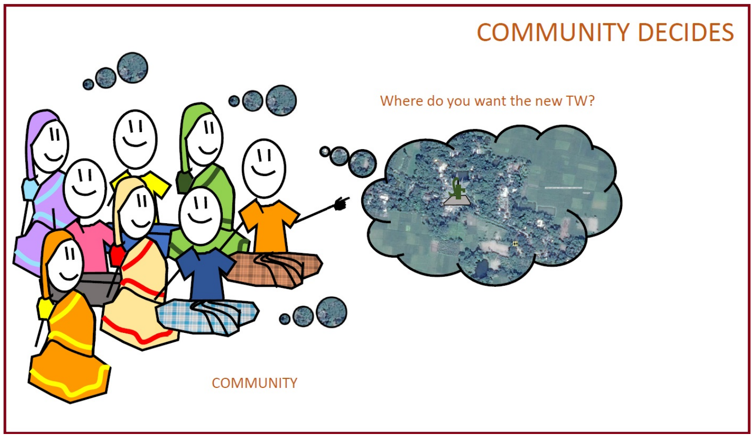
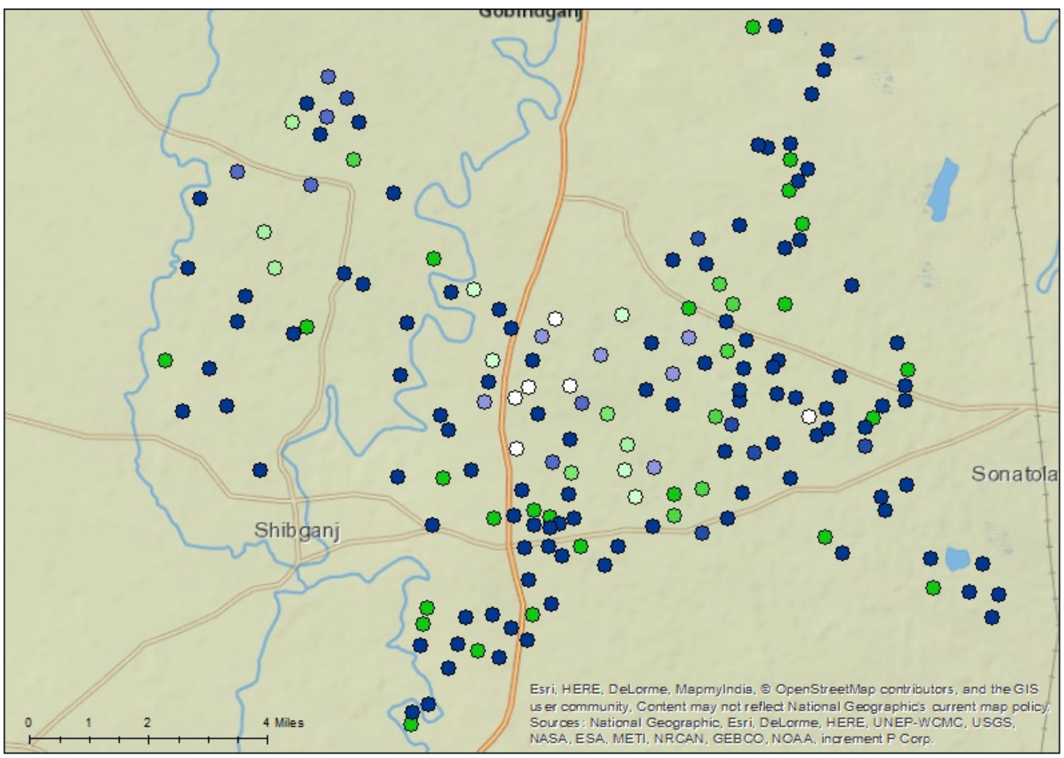


Figure 1c: Imported Institutions

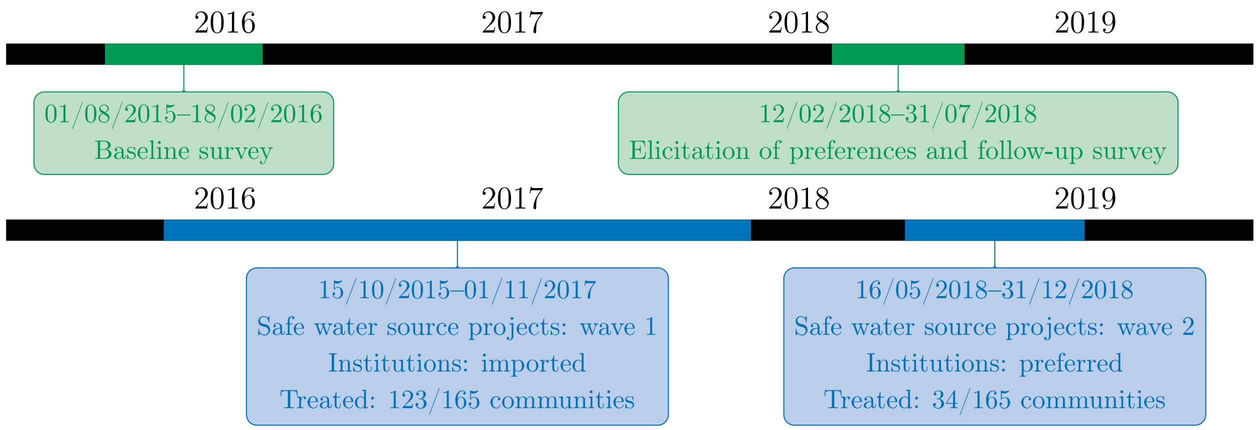
Figure 2: Elicitation procedure in the field

Figure 3: Map of treatment and control communities



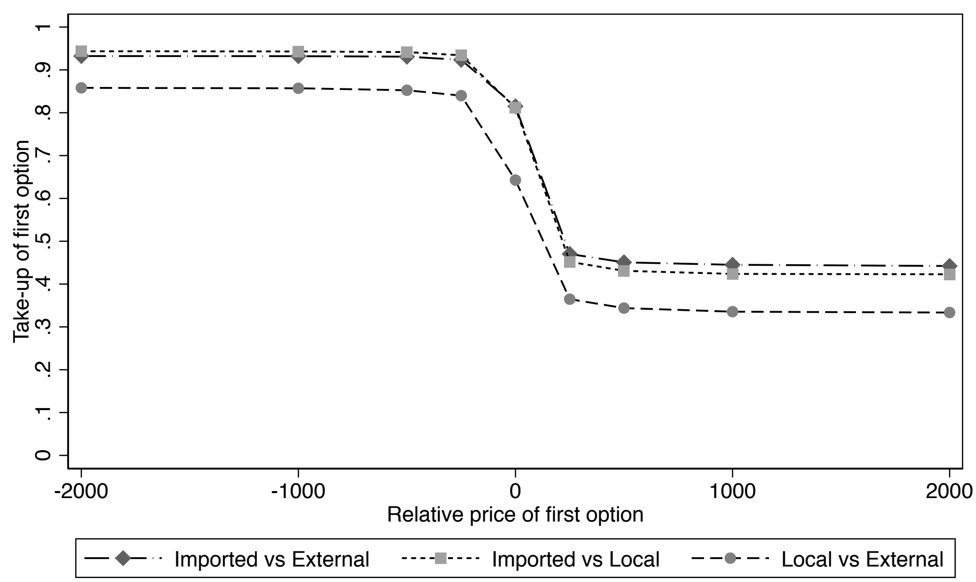
Notes: Map shows treatment and control communities. Treated communities are shown in white to blue; control communities are shown in green. Treated communities where 100% of installation attempts failed are excluded from this study and shown in white. For all other communities, colour intensity shows balancing weights, which are equal to one if at least one tubewell installation was successful and otherwise to the probability of at least one successful installation.

Figure 4: Project timeline



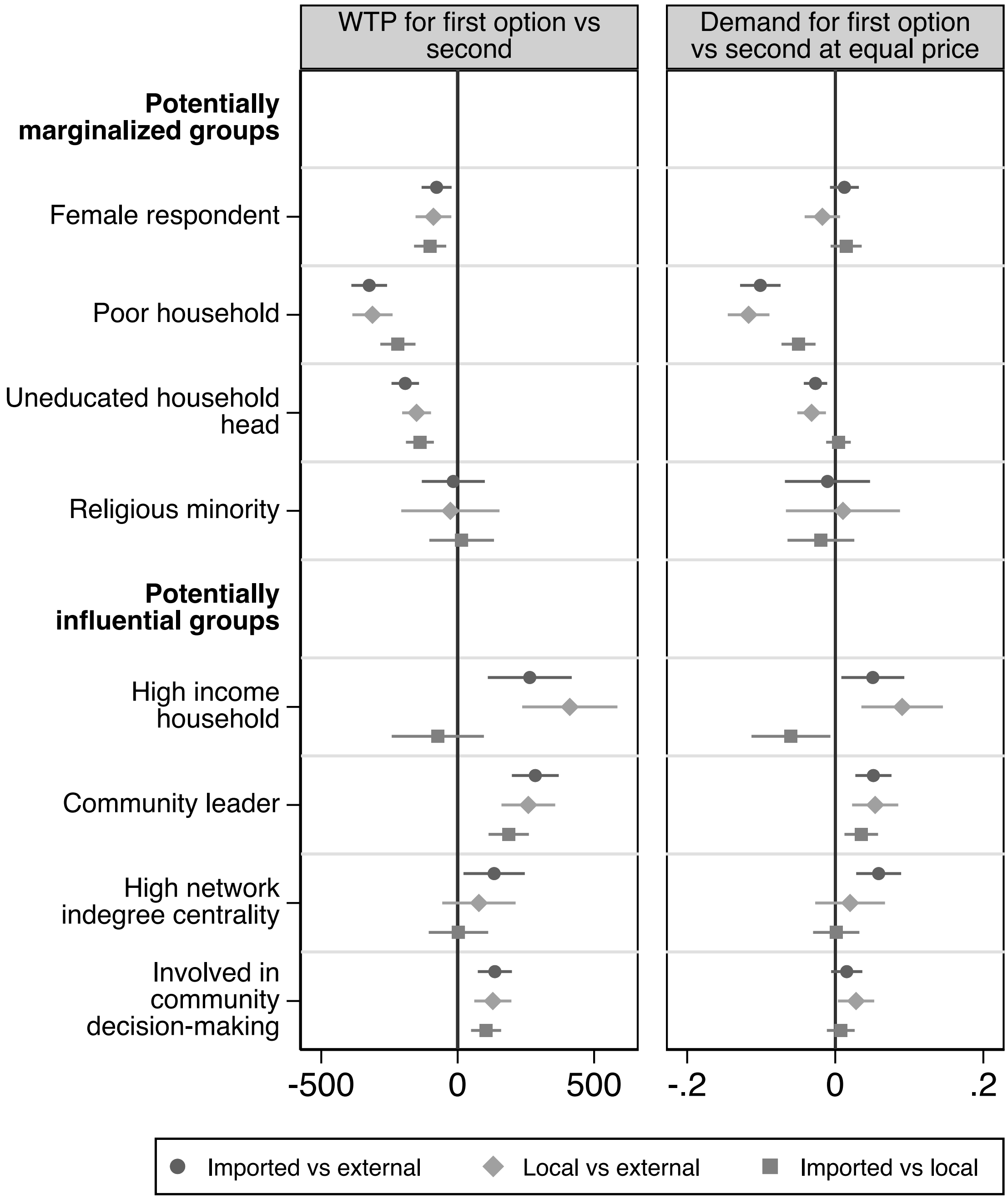
Notes: Data collection activities shown in green. Implementation activities shown in blue.

Figure 5: Demand curves



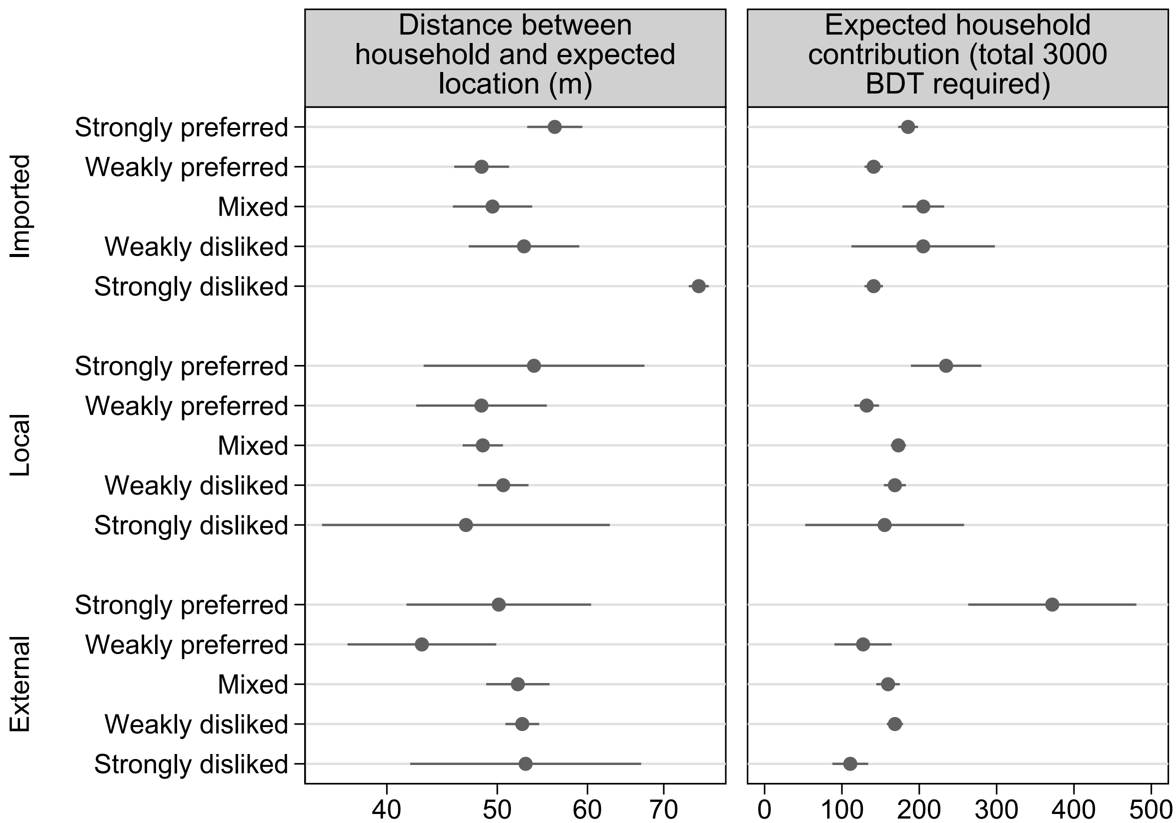
Notes: Graph shows take-up for for the first-listed option compared to the relative price, where relative price is defined as the difference in subsidy levels between the two options. Weights applied.

Figure 6: Heterogeneity of preferences across groups



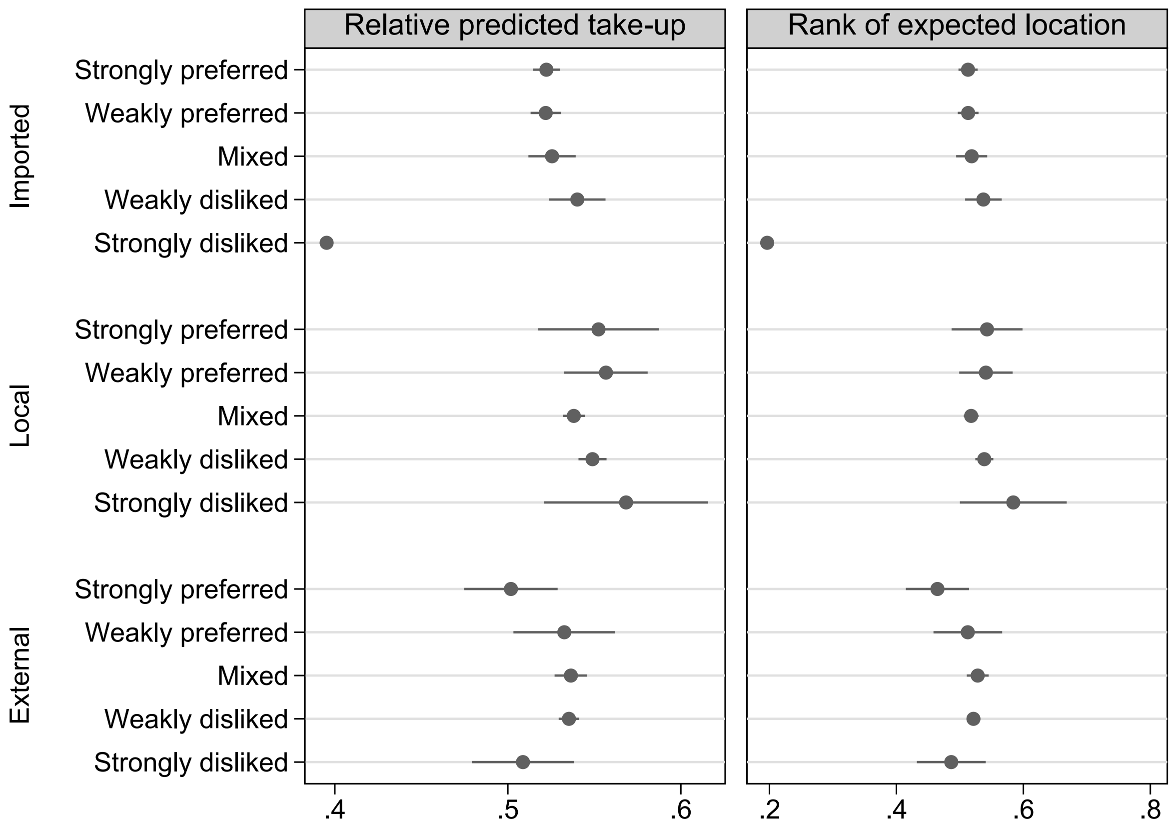
Notes: Graph shows regression-estimated difference in outcome variables and 90% confidence intervals from a regression on a binary measure of the household characteristic and union fixed effects. Household characteristics are either predetermined or measured at baseline. Weights applied. Standard errors clustered by community.

Figure 7: Expected private benefit by model and preference type



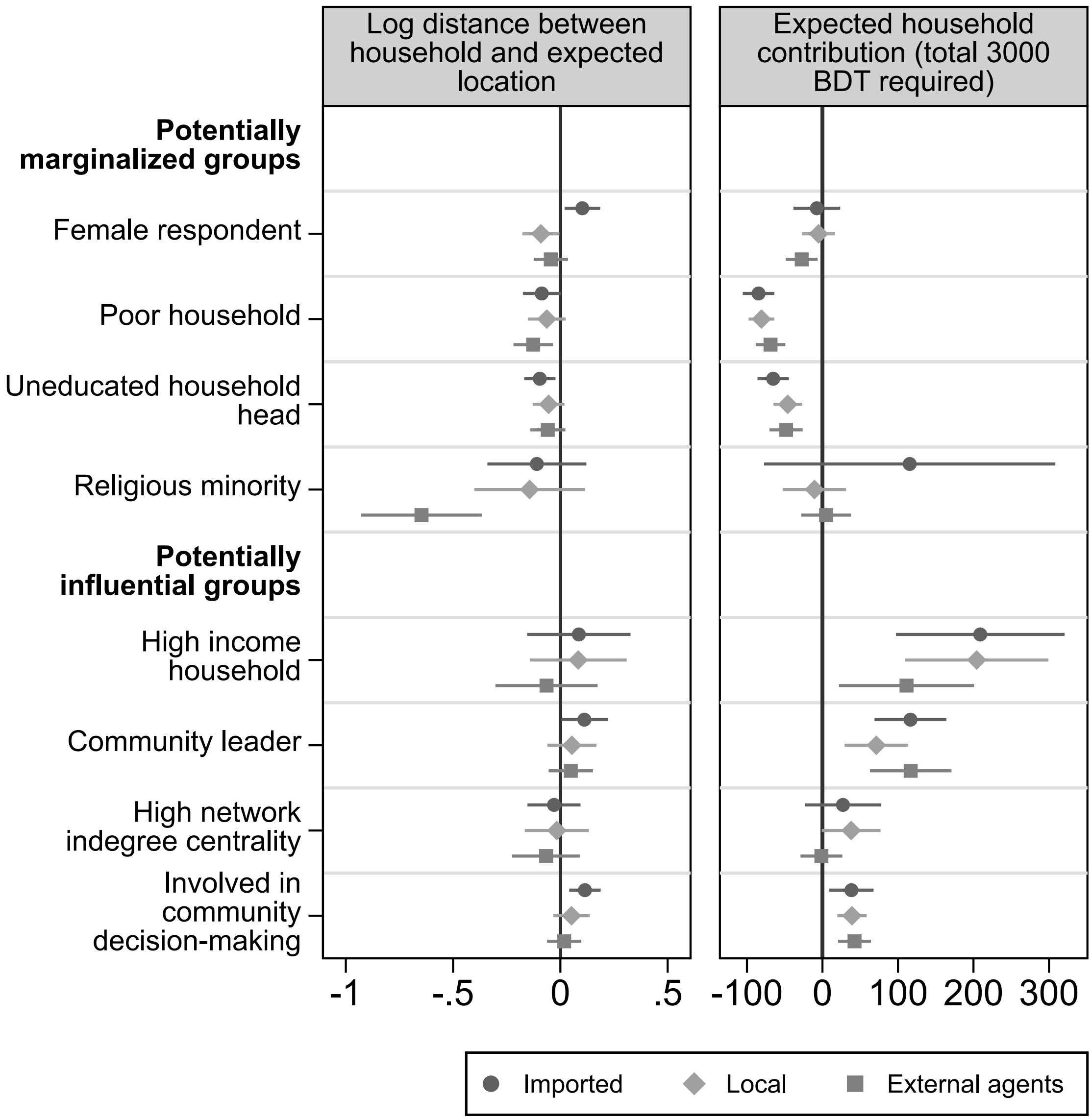
Notes: Graph shows mean value of outcome in listed group, accounting for union-fixed effects and clustering standard errors by treatment unit to estimate 90% confidence intervals. Weights applied.

Figure 8: Expected social benefit by model and preference type



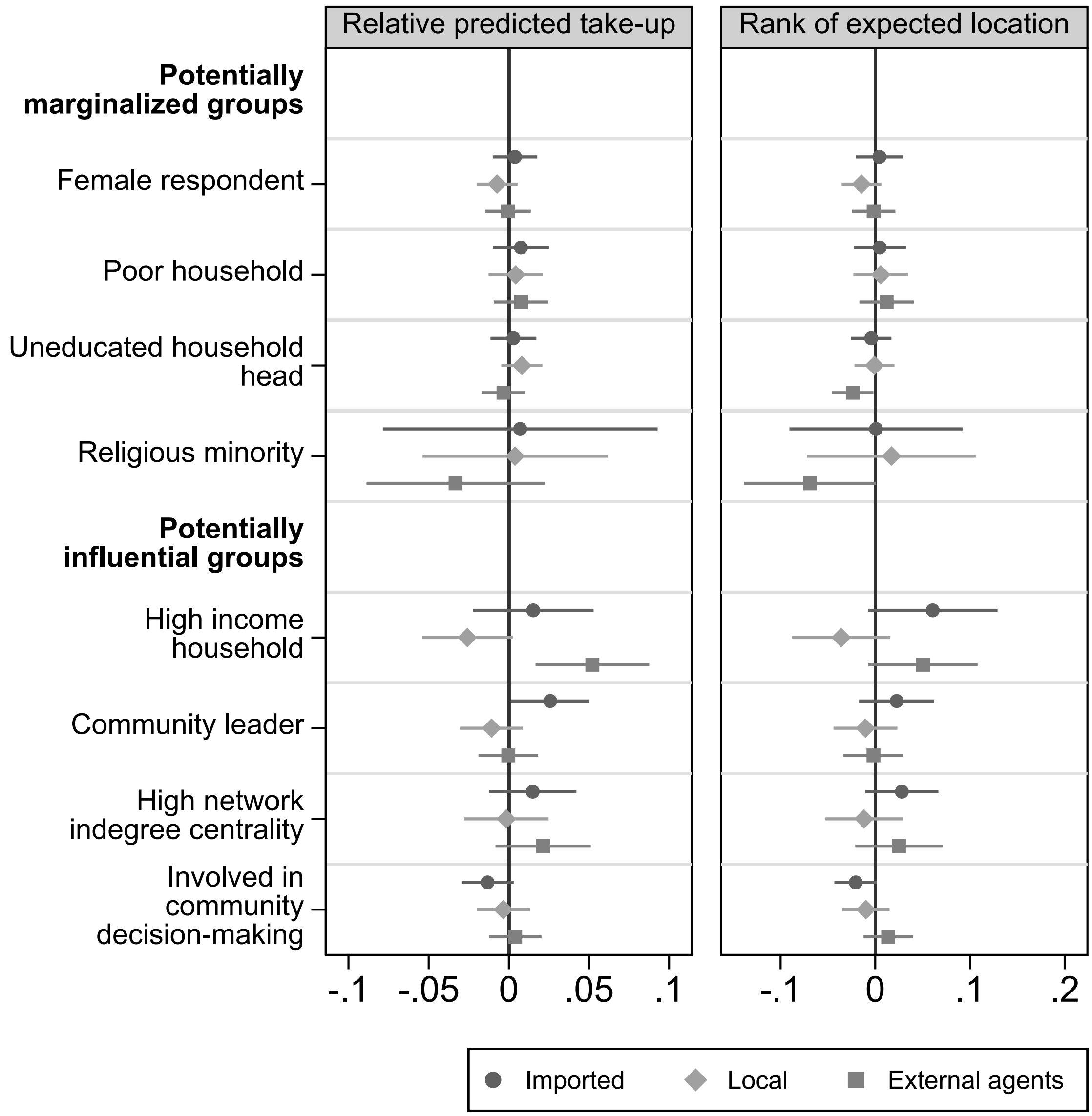
Notes: Graph shows mean value of outcome in listed group, accounting for union-fixed effects and clustering standard errors by treatment unit to estimate 90% confidence intervals. Weights applied.

Figure 9: Expected private benefit by household characteristics



Notes: Graph shows regression-estimated difference in outcome variables and 90% confidence intervals from a regression on a binary measure of the household characteristic and union fixed effects. Household characteristics are either predetermined or measured at baseline. Weights applied. Standard errors clustered by treatment unit.

Figure 10: Expected social benefits by household characteristics



Notes: Graph shows regression-estimated difference in outcome variables and 90% confidence intervals from a regression on a binary measure of the household characteristic and union fixed effects. Household characteristics are either predetermined or measured at baseline. Weights applied. Standard errors clustered by treatment unit.

Tables

Table 1: Price list

Option 1	Option 2
Process 1 + 5,000 BDT contributions	Process 2 + 3,000 BDT contributions
Process 1 + 4,000 BDT contributions	Process 2 + 3,000 BDT contributions
Process 1 + 3,750 BDT contributions	Process 2 + 3,000 BDT contributions
Process 1 + 3,500 BDT contributions	Process 2 + 3,000 BDT contributions
Process 1 + 3,000 BDT contributions	Process 2 + 3,000 BDT contributions
Process 1 + 2,750 BDT contributions	Process 2 + 3,000 BDT contributions
Process 1 + 2,500 BDT contributions	Process 2 + 3,000 BDT contributions
Process 1 + 2,000 BDT contributions	Process 2 + 3,000 BDT contributions
Process 1 + 1,000 BDT contributions	Process 2 + 3,000 BDT contributions

Table 2: Preference profiles at equal community contribution requirements

	Share of respondents (1)	Share of communities (2)
Imported > Local > External	45.9	88.2
Imported > External > Local	25.0	11.1
Local > Imported > External	9.6	-
Local > External > Imported	7.1	-
External > Imported > Local	8.5	-
External > Local > Imported	1.2	-
Intransitive	2.6	0.6
Observations	7051	165

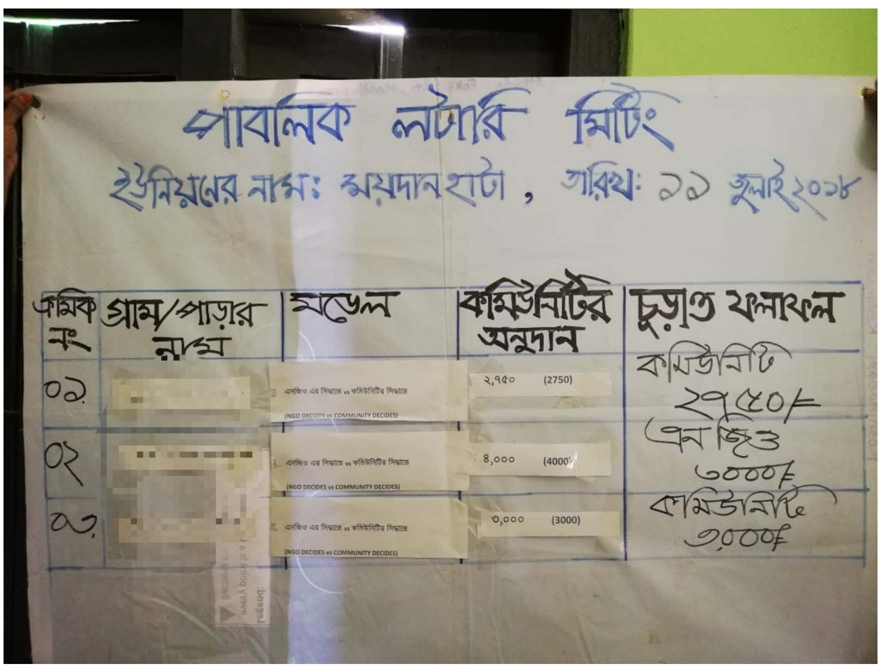
Notes: Weights applied.

Appendices

A Additional information on lottery procedure

For each community selected to receive the intervention, we extracted one pairwise choice out of the 27 presented to each participant in that community, and applied majority rule on the resultant choice between two combinations of decision-making processes and contribution requirements to determine the rules for implementation in that community. Specifically, during the public lottery we first extracted the name of the selected community from one box containing names of the candidate communities. Second, we extracted one pair of alternative decision-making processes from a box containing the three pairs. Third, we extract one contribution requirement, which was then associated with the option with varying contribution requirement, from a box containing the full range of alternative prices. We then posted each of these randomly extracted features — the name of community receiving the intervention, the pair of decision-making processes to choose between, and the community contribution requirement for the option with a varying requirement — on a board. Appendix Figure A1 illustrates.

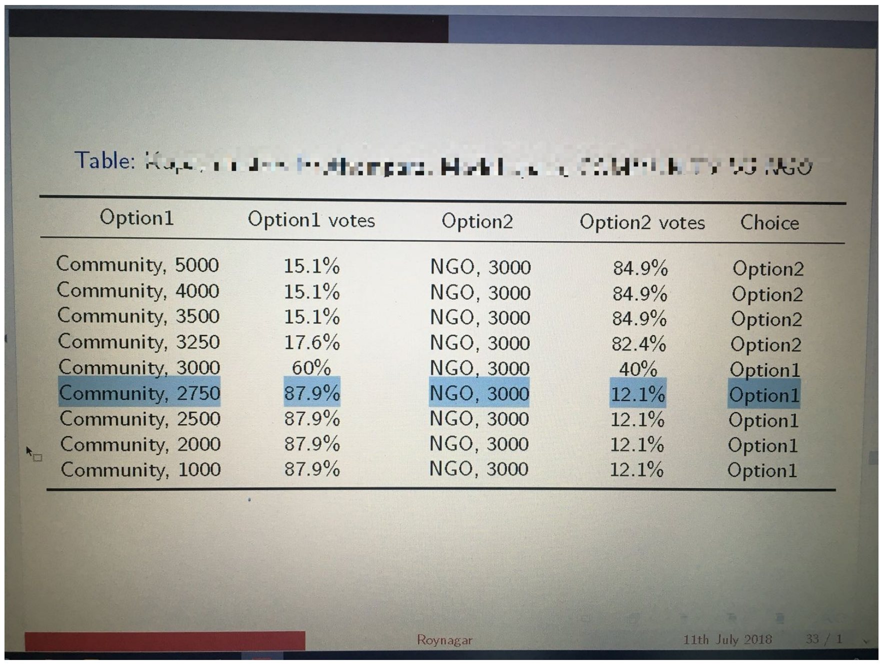
Figure A1: Example of "final results board" shown during public lottery meetings



For each selected community, we then showed a table summarizing the aggregate preferences in each community for each pair of options (Appendix Figure A2). We highlighted the relevant pairwise choice, explained the share of respondents in support of each option, and used these to determine the resultant final conditions — decision-making process and community contribution requirement — under which we would offer the safe drinking water program in the selected community. We add these final conditions to the results board shown in Appendix Figure A1, writing in Bengali.

At the lottery meeting, we pre-announced that in the event of a tie, we would randomly select which of the two extracted decision-making process-subsidy level combinations to implement. However, we did not extract any scenario corresponding to a tie at any of the lottery meetings.[63]

Figure A2: Example of table with aggregated preferences shown during public lottery meetings



Option1	Option1 votes	Option2	Option2 votes	Choice
Community, 5000	15.1%	NGO, 3000	84.9%	Option2
Community, 4000	15.1%	NGO, 3000	84.9%	Option2
Community, 3500	15.1%	NGO, 3000	84.9%	Option2
Community, 3250	17.6%	NGO, 3000	82.4%	Option2
Community, 3000	60%	NGO, 3000	40%	Option1
Community, 2750	87.9%	NGO, 3000	12.1%	Option1
Community, 2500	87.9%	NGO, 3000	12.1%	Option1
Community, 2000	87.9%	NGO, 3000	12.1%	Option1
Community, 1000	87.9%	NGO, 3000	12.1%	Option1

Under the first wave of safe drinking water projects, we already offered communities the opportunity to install new communal sources of drinking water. Therefore, within each union, we randomly select approximately 50% of the communities to receive the additional intervention from the control group, who did not receive the first wave of safe drinking water projects, and 50% from the group treated under the first wave of safe drinking water projects (Appendix Table A1). Because 3/4 of our study communities received a project under the first wave of safe drinking water projects, this implies that communities in the control group had a higher probability of selection during the second wave. Importantly, we did not reveal these differential probabilities of selection by treatment status to households at the time of the preference elicitation procedure.

[63]Of all possible scenarios, 6% resulted in a tie, but none of these cases was extracted at the lottery meetings.

Table A1: Eligible and selected communities for the additional water safety intervention

Union	Eligible communities		Selected communities	
Treated	Control	Treated	Control	Selected
Balua	4	14	2	2
Deuli	8	18	3	3
Kichak	1	5	1	
Kochasahar	3	8	1	1
Maidanhata	4	11	2	1
Mokamtala	9	27	3	4
Roynagar	1	2	1	
Saidpur	9	28	3	4
Shibganj	2	6	1	1
Shibpur	1	4	1	

Note that the design of the public lottery meetings required us to produce descriptive statistics on valuation of the three alternative decision-making processes, aggregated to the community level. We did not use the tables to make any comparison across communities, only to show them at the public lottery meetings. Importantly, the tables do not report the treatment status under the first wave of safe drinking water projects.

B Scripts

Appendix B contains an English translation of all the scripts used in the WTP and beliefs elicitation procedure.

B1 Informed consent to the overall interview

We are working for a NGO called NGO Forum for Public Health, and collaborating with researchers from Stockholm University. We are working on a research project in this region to provide access to safe drinking water. As you may know, your village is participating in this project.

As part of the research project, we want to ask you some questions about your household and in particular, about your access to safe drinking water. 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. There are no other risks to participating in this project.

The questions should take less than two hours. You do not have answer any questions if you do not want to. If you agree to listen to our questions, you can choose not to answer any question, and you can stop answering questions at any time. There are no penalties or loss of benefits to you if you don't answer any question, or if you ask us to stop asking questions. There are no other costs involved to answering our questions, beyond the time it will take you to do so. There is no direct benefit to your participation in this research survey by answering our questions. However, answering the questions could help us and other NGOs to design better projects in the future to provide safe drinking water in villages like this one.

If you have any other questions about this project, or would like to offer any input, please contact Ahasan Habib at NGO Forum on 01917232xxx.

Do you consent to proceed?

B2 Introduction to the elicitation procedure

Consider the hypothetical situation that your community will receive a project to install one new source of arsenic-free water.

In the next screens I will ask you your preferences on how project decisions on the installation of this new source of arsenic-free water should be taken. With project decisions we refer primarily to the decision of where to install this new source of arsenic-free water.

We will consider three different options on how to take these project decisions relative to the hypothetical installation of this new source of arsenic-free water:

- COMMUNITY CONSENSUS IN NGO STAFF PRESENCE
  Under the first option, project decisions will be taken by unanimous consensus during community meeting in the presence of NGO staff. The community will have to choose a site for installation of the new safe tubewell. Everyone will be invited to the community meeting, and the community meeting will take place only if all different social groups are represented.

- DECISIONS TAKEN BY COMMUNITY
  Under the second option, project decisions will still be taken by the community. NGO staff will still organize a meeting, but only to explain the project rules and contribution requirements. However, NGO staff will then conclude the meeting, and the community can take the decision about a site for installation of the new safe tubewell by itself, in any way it chooses. Project staff will not observe how the community takes the decision. And they will not tell you how you should make the decision. After one week, they will come back to the community and the community can let them know what you have decided. The only circumstance in which they will ask the community to change the decision is if the site the community chooses is not safe or possible for well installation.

- DECISIONS TAKEN BY NGO STAFF
  Under the third option, the NGO staff will choose a site for installation of the new safe tubewell.

Please notice that under all these options, the hypothetical extra project to install a new source of arsenic-free water is identical, and it consists on the possibility to install one new safe tubewell. In order to receive this hypothetical extra project, the community is required to make some cash contributions to co-fund the installation costs. The only difference is how decisions will be taken.

Please remember that we will keep secret all your answers. Other people in your community will never know your answers. So please feel free to report to me your true preferences.

Check understanding:

- Imagine the hypothetical situation that your community receives this extra project. Do you understand how decisions will be taken under the option COMMUNITY CONSENSUS IN NGO STAFF PRESENCE?
  Ask the participant to explain!
  [Enumerator explains again if answer is not correct]

- Imagine the hypothetical situation that your community receives this extra project. Do you understand how decisions will be taken under the option DECISIONS TAKEN BY THE COMMUNITY?
  Ask the participant to explain!
  [Enumerator explains again if answer is not correct]

- Imagine the hypothetical situation that your community receives this extra project. Do you understand how decisions will be taken under the option DECISIONS TAKEN BY NGO STAFF?
  Ask the participant to explain!
  [Enumerator explains again if answer is not correct]

B3 Informed consent to the elicitation procedure

Please notice that there is some chance that your community will be selected to receive for real a new project to install a new source of arsenic-free water. NGO will organize a public lottery in order to select the communities that will receive this extra project. We will select 34 communities to receive this new tubewell, among the communities that participated in this study.

In case your community is selected to receive this extra project to install a new source of arsenic-free water, we will take into account the answers you will give to the next questions in order to define how project decisions will be taken. Therefore, it is particularly important that you provide careful and truthful answers to the next questions, because they can influence how this extra project will be implemented!

Please notice that your preferences on the different decision-making processes will not increase or decrease that chances of your community to be selected for this extra project. The extra project to install a new source of arsenic-free water is identical, and it consists on the possibility to install one new safe tubewell. The only difference between the three difference decision-making processes is only how project decisions will be taken.

I will now present you different choices. For each choice you will have to choose between two alternative decision-making processes. Plus, there will be different contributions requirements associated to each alternative decision-making process. The contribution requirements will vary from 1,000 Bangladeshi takas to 5,000 Bangladeshi takas. I will ask your most favorite option for each choice.

Imagine the hypothetical situation that your community receives the extra project. How are we going to define the decision-making process to implement in your community? We will randomly select one choice: two alternative decision-making process and the contribution requirements associated to each of them. We will implement the option most favoured on average by the people in your community. Every vote will count. It is particularly important that you provide careful and truthful answers to ALL the next questions, because they can influence how this extra project will be implemented! Your choice is important!

Does the respondent have extra questions or doubt about how we will aggregate preferences to define the decision-making process to be implemented in case his/her village is selected for receiving the extra project? If yes: We don't survey everyone in the village. Some of the households we survey are chosen at random, some because of other reasons, for example because they are leaders in the community. We use the data from all the households we survey to estimate the average preferences within the community. For example, we use leader households to estimate the preferences of leaders in the community, and randomly chosen households to estimate the preferences of ordinary people in the community. Then we use the estimated preferences in each group to estimate the average preferences in the community as a whole. If you have any other questions about this procedure, please contact Ahasan Habib at NGO Forum on 01917232xxx.

Remember, because we will only use your answer to calculate the average preference in your community, noone will ever know your own individual choices. However, if for any reason you don't want us to use your answer in calculating the average preference in your community, please let us know. If you don't want us to use your answer, we will use only the responses of other households in this community to decide how to implement the project in the community. Should we use your responses to help us decide how to implement the project in the community?

B4 Check understanding of the elicitation procedure

- Now let me give you a concrete example. Imagine your community is selected to receive the extra project, and that the choice we select is:
  - Option 1: $process-1$ and community required to contribute $price-1$ BDT
  - Option 2: $process-2$ and community required to contribute 3000 BDT.

  Imagine that in your community we surveyed 40 households, selected at random from your community. Among them, 30 said they preferred Option 1 and 10 said they preferred Option 2. Which decision-making process we will implement in your community, Option 1 or Option 2?
  [$process-1$, $process-2$ and $price-1$ vary randomly for each participant.]
  [Enumerator explains again if answer is not correct]

- Are ALL your answers to the following questions important because each of them can influence how the project would be implemented?
  [Enumerator explains again if answer is not correct]

B5 Elicitation procedure

[The elicitation procedure is repeated three times, for each pairwise comparisons of processes. We always ask preferences in the following order:

- Top-Down process vs Consensus-based community participation process
- Unregulated community participation process vs Top-Down process
- Consensus-based community participation process vs Unregulated community participation process

For each pairwise comparison we randomly vary at community level which process is associated to the varying price and which process to the fixed price.]

[Enumerator uses cards to represent the decision-making processes and the required community contributions as shown in Figure 2]
The option with the varying price is: $process-1$. The option with the fixed price is: $process-2$.

- CHOICE 1: Do you prefer:
  - $process-1$ + contribution 5,000 BDT
  - $process-1$ + contribution 3,000 BDT
- CHOICE 2: Do you prefer:
  - $process-1$ + contribution 4,000 BDT
  - $process-1$ + contribution 3,000 BDT
- CHOICE 3: Do you prefer:
  - $process-1$ + contribution 3,500 BDT
  - $process-1$ + contribution 3,000 BDT
- CHOICE 4: Do you prefer:
  - $process-1$ + contribution 3,250 BDT
  - $process-1$ + contribution 3,000 BDT
- CHOICE 5: Do you prefer:
  - $process-1$ + contribution 3,000 BDT
  - $process-1$ + contribution 3,000 BDT
- CHOICE 6: Do you prefer:
  - $process-1$ + contribution 2,750 BDT
  - $process-1$ + contribution 3,000 BDT
- CHOICE 7: Do you prefer:
  - $process-1$ + contribution 2,500 BDT
  - $process-1$ + contribution 3,000 BDT
- CHOICE 8: Do you prefer:
  - $process-1$ + contribution 2,000 BDT
  - $process-1$ + contribution 3,000 BDT

- CHOICE 9: Do you prefer:
  - $process-1$ + contribution 1,000 BDT
  - $process-1$ + contribution 3,000 BDT

B6 Introduction to belief elicitation

[Enumerator uses cards to represent the decision-making processes and the required community contributions as shown in Figure 2]
Consider the following pairwise choice:

- Option 1: $process-1$ and community required to contribute $price-1$ BDT
- Option 2: $process-2$ and community required to contribute 3000 BDT

Remember that you said to prefer $preferred-option$.
[$process-1$, $process-2$ and $price-1$ vary randomly for each participant. $preferred-option$ is respondent's most preferred option under that processes and contribution requirement combination.]

B7 Pivotality

[Respondent asked one of the two questions below, randomly selected at the household level:]

- Do you think that people in your community will split approximately equally on this two options, or you think that one of the two options will be preferred on the other one by the large majority of people?
  Options: I think the first option will be largely preferred by people in my community; I think that the group of people preferring the first option will be approximately equal to the group of people preferring the second option; I think the second option will be largely preferred by people in my community; Don't know; Refused to answer.
- Please tell me how much you agree with the following sentence:
  "I think that, relative to this choice, my vote could be decisive in defining the most preferred option by the majority of people in my community" Options: Strongly agree; Agree; Neither agree nor disagree; Disagree; Strongly disagree; Don't know; Refused to answer.

B8 Expected tubewell locations

- Imagine that the community will receive the extra project under Option 1: $process-1$ and community required to contribute $price-1$ BDT.
  Can you indicate on the map where it will be chosen to install the new project tubewell? [Enumerator shows the printed map of the treatment unit.]
- Imagine that the community will receive the extra project under Option 2: $process-2$ and community required to contribute 3,000 BDT.
  Can you indicate on the map where it will be chosen to install the new project tubewell? [Enumerator shows the printed map of the treatment unit.]

B9 Expected household contribution

- Imagine that the community will receive the extra project under Option 1: $process-1$ and community required to contribute $price-1$ BDT.
  How much do you think that your household will contribute of the $price-1$ BDT?
- Imagine that the community will receive the extra project under Option 2: $process-2$ and community required to contribute 3,000 BDT.
  How much do you think that your household will contribute of the 3,000 BDT?

C Other sources of data

This study uses data collected via household surveys administrated before and after the implementation of the first wave of safe drinking water projects and through comprehensive documentation of all safe drinking water project implementation activities. Unless specified otherwise, data is collected on tablets and uploaded to SurveyCTO servers, allowing us to link household data across surveys and with the implementation data. This section provides further details on the other sources of data we use.

C1 Baseline survey

At baseline we carried out a census of all existing sources of drinking water and a household survey with a sample of randomly selected 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, engagement in collective actions or other activities carried out at the community level, and habits related to water collection and use.

C2 Program implementation

We closely monitored implementation of the safe drinking water programs. We collected data during all stages of program implementation, including household attendance and active participation in the community meetings (disaggregated by gender and age groups (16-34 and 35+)), the duration of the community meetings, proposals discussed during the community meetings, the final location of the new water sources, the distribution of financial/labor contributions within the community, whether communities successfully raised the required contributions, and technical information about the installation process.

C3 Follow-up survey

During the follow-up survey following the first wave of safe drinking water projects, we collected the same information as at baseline. In treated communities, we also collected information on household participation during different stages of project implementation, general feedback on the decision-making process, whether or not households used project tubewells, and household contributions to funding or maintenance of project tubewells.

D Recruitment and study population

The sample of communities and households used for this project is defined through several steps, illustrated in Appendix Figure D1.

D1 Eligibility

Our intervention is targeted towards communities exposed to arsenic contamination. Unfortunately, we had only limited data on arsenic contamination in the area when we began the study. We therefore used the data available to pre-select communities for inclusion and then used our own testing data to confirm eligibility.

We pre-selected a list of candidate villages for the intervention on the basis of contamination levels reported in the available sources of arsenic testing data. We had access to village-level data from the following data sources: (i) data from the Bangladesh Arsenic Mitigation Water Supply Project (BAMWSP), which included a large tubewells screening program conducted between 1999 and 2006; (ii) an assessment from the Department of Public Health Engineering (DPHE) of the most arsenic-contaminated villages in the Bogra region; (iii) data collected in 2008 from the Bangladesh Social Development Services (BSDS). We preselected as candidate villages for receiving our intervention all villages indicated by the DPHE as highly contaminated, or for which BAMWSP or BSDS data reported a share of arsenic contaminated tubewells equal to or higher than 30%. We confirmed this initial selection by testing for arsenic contamination a small sample of tubewells in the village.

D2 Treatment unit definition

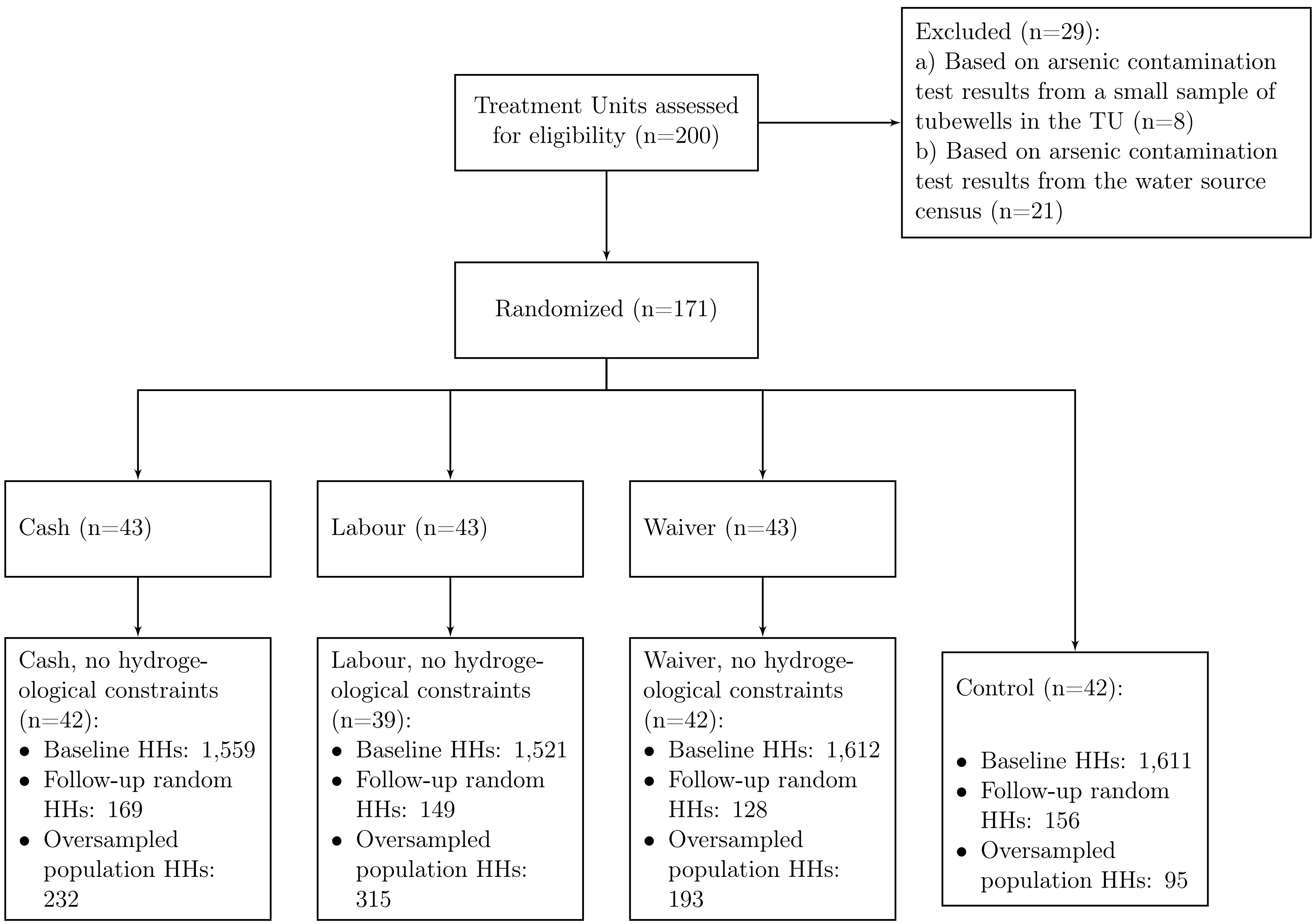
For the candidate villages identified using pre-existing information, we obtained the most updated list of resident households from administrative sources. For logistical reasons, we implement the program in geographically defined treatment units of between 50 and 250 households. We use the terms "treatment unit" and "community" interchangeably.

To define treatment units, we used available household administrative lists in order to obtain village sizes, exclude from the study villages with less than 50 households and divide larger villages into several smaller treatment units along natural boundaries. Following this process, we identified 192 candidate treatment units in 103 villages, of which 51 were divided in two or more treatment units. We conducted a full census of existing sources of drinking water in these candidate treatment units.

We used the water source contamination data in order to finalize the selection of the treatment units eligible for receiving the arsenic mitigation program. Specifically, we excluded from the study all treatment units with less than 15% of arsenic contaminated water sources. We further screened treatment units with less than 25% of arsenic contaminated water sources, including them in the program only if they presented a well defined cluster of contaminated water sources. To evaluate these treatment units with between 15% and 25% contamination, we reviewed the maps obtained from the water source census. We excluded treatment units where arsenic contaminated water sources were geographically scattered, because in these cases all households in the village already had a nearby source of arsenic-safe water.

The administrative data available to define treatment units was of limited quality. In particular, the administrative lists available sometimes did not correspond to the correct geographical location of households in the villages. As a result, we sometimes needed to redefine treatment units after collecting household-level data. We did this by reviewing the geo-coordinates of households and water sources collected during the baseline survey and via field visits by project staff. After several rounds of refinements we were largely successful in correctly defining treatment units with a comprehensive household list corresponding to a meaningful geographical entity. However, as a consequence of this process, the baseline household sample was not equally distributed across treatment units, with a larger number of households interviewed in some treatment units and a smaller number of households interviewed in others. At follow-up, we corrected for these disparities by adding to the baseline sample some randomly selected households in treatment units where at baseline we interviewed less than 40 households. In the analysis where we use the baseline sample, we correct for these disparities using sampling weights (Appendix F).

Figure D1: Sample definition



D3 Household sample

We randomly sampled households for inclusion in the study sample (Appendix Figure D1). We randomly selected a total of 6,905 households from the administrative lists, of which 6,303 were already interviewed at baseline. On average, this corresponded to 40 households per study community, after accounting for some adjustments which resulted from cases where we had to correct treatment unit definition after the baseline survey. Additionally, we oversampled households in some specific populations, sampling 100% of households that fell into one of three categories. We targeted 835 such households.

The three categories of households that we oversampled were:

- Households identified at baseline as community leaders by at least two other households in the same community.
- Households with high network centrality, defined as being listed by at least 4 other households in the same community as part of their social network.
- Households that played a particularly active role in program implementation during the first wave of safe drinking water projects, including households that contributed cash or labour towards the project, donated land for tubewell installation, or were elected to manage and maintain the new water source

D4 Attrition

We successfully completed the elicitation procedure in a very high percentage of the targeted households. The total number of households targeted at follow-up and for the elicitation procedure was 7,740. Enumerators completed the interview with 99.7% of the households selected to participate in the household survey and the elicitation procedure with 99.1% of them. Among households that we were able to successfully contact at follow-up, 99.9% agreed to the interview and completed the elicitation procedure.

We had baseline data on 6,303 of the targeted households. Among these households, the attrition rate between baseline and follow-up was 0.65%. The low rates of attrition between baseline and follow-up support our use of baseline community characteristics to validate our study design.

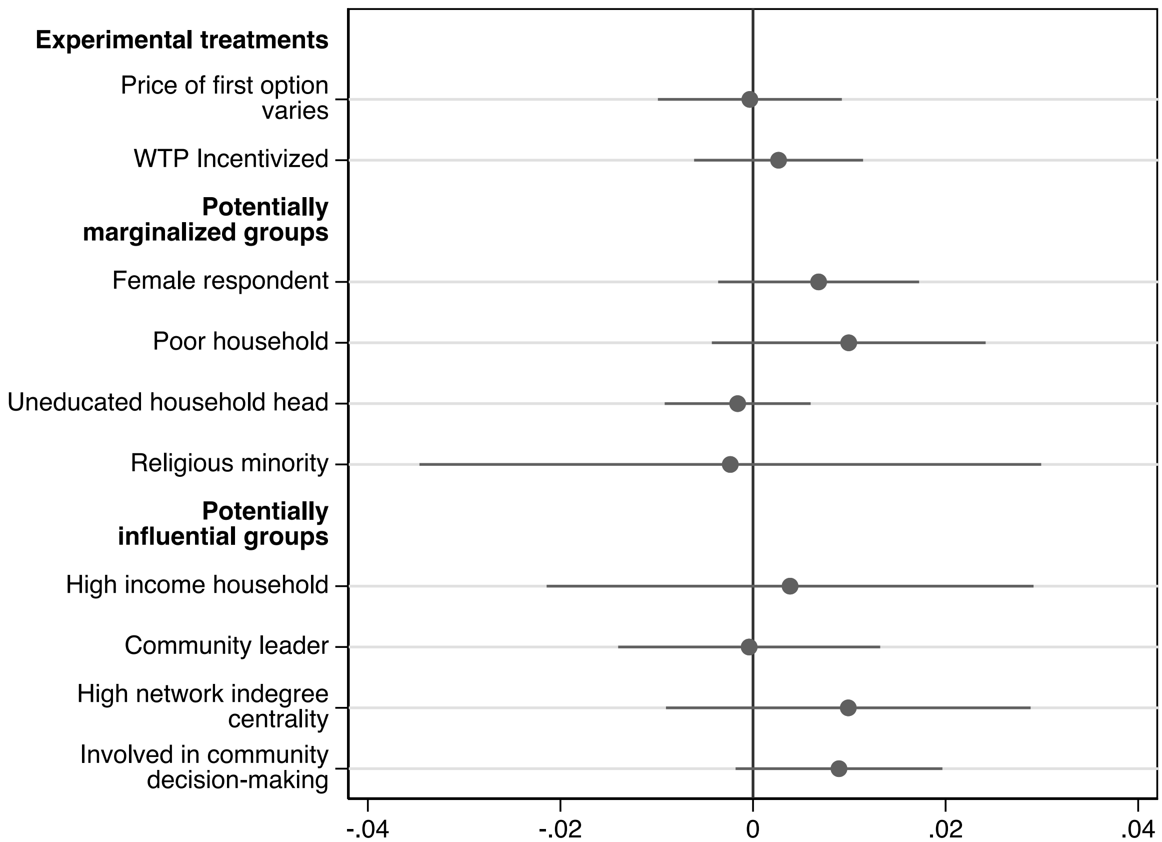
D5 Data quality

In verifying the validity of the data collected through the elicitation procedure, we observed that around 5% of households in the full sample reported preferences that imply intransitivity at zero price difference. A larger fraction (11%) reported unambiguously intransitive preferences when we incorporate information about relative prices, as defined in the main paper. As it is unlikely that individual preferences are truly intransitive, it is reasonable to be concerned that these observations are not necessarily reliable.

As we discuss in Section 6.2 of the main paper, the rate of intransitivity varies strongly across enumerators. Indeed, 60% of the cases of unambiguous intransitivity in the full sample correspond to data collected by two enumerators who did not participate in the full training process, having been hired at a later stage to replace enumerators who left the project. For these two enumerators, 79% and 87% of the data they collected corresponds to unambiguously intransitive preference data. No other enumerator collected data with rates of intransitivity higher than 17%; this enumerator also missed the training process. We interpret this as providing an unintentional check on the quality of our training procedures. In the descriptive analysis in this paper, we drop all data collected by these two enumerators (621 observations), and we discuss how we anticipate dealing with these cases in the experimental analysis in Section 10.1.

The rate of intransitivity does not however seem to vary strongly by household characteristics or in response to experimental manipulations. Appendix Figure D2 shows that there are no systematic differences in the rates of intransitivity with respect to either experimental manipulations of the design or household characteristics.

Figure D2: Heterogeneity in rates of intransitivity



Notes: Graph shows regression-estimated difference in rate of intransitive responses and 90% confidence intervals from a regression on a binary measure of the listed experimental treatment or household characteristic and enumerator and union fixed effects. Household characteristics are either predetermined or measured at baseline. Weights applied. Standard errors clustered by treatment unit.

The resulting full household sample consists of 7,052 households in 165 communities. This comprises 6,287 households sampled at random from the administrative lists and 765 households from the oversampled populations. The large majority of households in the random sample (5,759 households) were interviewed both at baseline and at follow-up.

E Tubewell feasibility

We initially recruited a total of 171 communities to the study. With respect to the first wave of safe drinking water projects, we assigned 129 to the treatment arms and 42 to the control group. We exclude 6 communities from the current study from the group assigned to treatment during the first wave. In these communities, we attempted to install deep tubewells during the first wave, but the installation attempts failed, because of hydrogeological constraints. Asking these communities about construction of a hypothetical future tubewell would have not been a meaningful exercise, since there was a very high likelihood that other attempts at installation would have failed. We therefore did not conduct the elicitation procedure with the survey respondents from these communities.

Higher arsenic contamination is associated with a higher likelihood of attempting installation (Cocciolo et al., 2019b). However, conditional on attempting installation, tubewell feasibility is a product of hydrogeological conditions only, which are unobservable from ground level. Unsurprisingly, therefore, Appendix Tables E1 and E2 show that, conditional on attempting installation, installation success is not correlated with socio-economic characteristics or baseline water quality and practices.

Table E1: Randomness of installation failures: Water-related variables

	Success	Failed	Diff
Arsenic contamination (WHO) (HH test)	0.77	0.91	-0.14**
	(0.06)	(0.08)	(0.06)
Arsenic contamination (BD) (HH test)	0.44	0.49	-0.04
	(0.11)	(0.13)	(0.08)
Bacteria contamination (HH test)	0.57	0.56	0.02
	(0.04)	(0.06)	(0.04)
Arsenic contamination (WHO) (WS test)	0.81	0.89	-0.09
	(0.06)	(0.08)	(0.06)
Arsenic contamination (BD) (WS test)	0.53	0.58	-0.05
	(0.11)	(0.14)	(0.09)
Bacteria contamination (WS test)	0.28	0.31	-0.04
	(0.06)	(0.07)	(0.04)
Storage dummy (observed)	0.65	0.64	0.02
	(0.05)	(0.06)	(0.03)
Water is treated before drinking (primary WS)	0.14	0.15	-0.00
	(0.04)	(0.05)	(0.03)
Time needed to collect water (mins)	2.63	2.74	-0.10
	(0.05)	(0.13)	(0.12)
Water collected per day (litres)	48.71	48.99	-0.28
	(2.40)	(5.01)	(4.40)
p value on joint F-test		0.442	
N		177	

Notes: Columns (1) and (2) report means and standard errors for, respectively, successful and failed installations. Means and standard errors, clustered at community level, are obtained from a regression with union FE and no constant. Column (3) tests for equality in means. Regressions are run on the sample of tubewells for which at least one installation was attempted. * p < 0:1, ** p < 0:05, *** p < 0:01.

Table E2: Randomness of installation failures: Socio-economic variables

	Success	Failed	Diff
Household size	3.85	3.96	-0.11
	(0.11)	(0.14)	(0.08)
Poverty score - 2 USD	0.74	0.72	0.01
	(0.02)	(0.02)	(0.02)
Muslim household	0.89	0.82	0.07*
	(0.08)	(0.09)	(0.04)
Network nominations	1.02	0.84	0.18**
	(0.12)	(0.14)	(0.09)
Network size	2.20	1.96	0.24
	(0.03)	(0.17)	(0.17)
Leader household	0.07	0.05	0.03**
	(0.02)	(0.03)	(0.01)
The household head has no education	0.34	0.35	-0.00
	(0.04)	(0.06)	(0.04)
Not educated HH members (%)	0.33	0.31	0.02
	(0.03)	(0.04)	(0.03)
Literacy rate in the household	0.61	0.64	-0.03
	(0.03)	(0.04)	(0.02)
The household owns livestock	0.67	0.65	0.02
	(0.05)	(0.06)	(0.02)
The household owns land for cultivation	0.35	0.34	0.01
	(0.05)	(0.06)	(0.04)
Land owned by the household (acres)	1.96	2.04	-0.08
	(0.62)	(0.62)	(0.07)
HH has some toilet facility	0.86	0.84	0.02
	(0.03)	(0.04)	(0.02)
Number of rooms to sleep	1.74	1.74	0.00
	(0.07)	(0.08)	(0.05)
The floor is made of earth or sand	0.85	0.83	0.02
	(0.02)	(0.03)	(0.03)
The roof is made of metal	1.00	1.00	0.00
	(0.00)	(0.01)	(0.01)
Mobile phone ownership	0.67	0.69	-0.02
	(0.03)	(0.04)	(0.04)
Ownership of a motorized vehicle	0.05	0.06	-0.01
	(0.02)	(0.02)	(0.02)
Wealthy household (assets index, top 5%)	0.20	0.24	-0.04
	(0.03)	(0.04)	(0.03)
High trust towards community	0.26	0.29	-0.03
	(0.02)	(0.06)	(0.06)
Know association	0.15	0.13	0.02
	(0.02)	(0.03)	(0.02)
p value on joint F-test		0.633	
N		177	

Notes: Columns (1) and (2) report means and standard errors for, respectively, successful and failed installations. Means and standard errors, clustered at community level, are obtained from a regression with union FE and no constant. Column (3) tests for equality in means. Regressions are run on the sample of tubewells for which at least one installation was attempted. * p < 0:1, ** p < 0:05, *** p < 0:01.

F Population weights

F1 Sampling weights

The sample of households used in this study consists of a random sample of households drawn from household lists, along with some households drawn from oversampled populations in some special categories of particular interest to the broader study e.g. leader households, network focal points, etc. We discuss the sampling procedure further in Appendix D.

The target number of randomly sampled households was 40 per treatment unit. However, because of the process to redefine treatment units, the baseline sample was not perfectly distributed across treatment units. At follow-up, we interviewed a minimum of 40 randomly sampled households per community, but some communities still had more than 40 households in the sample. We use sampling weights throughout to ensure that each treatment unit counts equally in each summary statistics. Specifically, in a sample with only randomly drawn households, we weight each household by $1/N_{sample}$, where N_{sample} is the number of households in the sample, so that the weights on all households within each community sum to 1.

In addition, the follow-up sample includes households from special categories that we oversampled. The follow-up sampling weights also account for the rate of incidence of these households in each community. We oversample 100% of the households in these special populations, so that the weight placed on each of these households is $1/N_{community}$, where $N_{community}$ is the number of households in the community. We then adjust the weights on all the randomly-sampled households within each community so that the sampling weights on all households within a community continue to sum to 1.

F2 Balancing weights

In Section 6.2 and Appendix E we discuss why we exclude 6 communities where attempts to install deep tubewells during the first wave of safe drinking water projects failed because of hydrogeological constraints. Tubewell feasibility is spatially correlated. As a result, excluding communities where tubewell construction was not feasible from the treatment arm could induce imbalance between the treatment and control group. This section describes the construction of the community-level balancing weights we use to address the exclusion of these communities from the treated arm.

We observe the feasibility of tubewell installation only for communities where an installation attempt was made. We therefore do not observe tubewell feasibility in either the control group or in communities which were assigned to treatment but in which we did not attempt installation because the community did not fulfil all the study requirements. The balancing weights we construct in the communities in which we did not attempt installation correspond to the probability of successfully installing at least one water source, conditional on attempting installation.

To be specific, we calculate the probability of successful installation of a tubewell conditional on attempting installation in each union, using data from all attempts to install wells in the study. We then calculate the probability of at least one successful installation in all study communities in which we did not attempt installation, accounting for whether we would have assigned one or two tubewells to any given community.

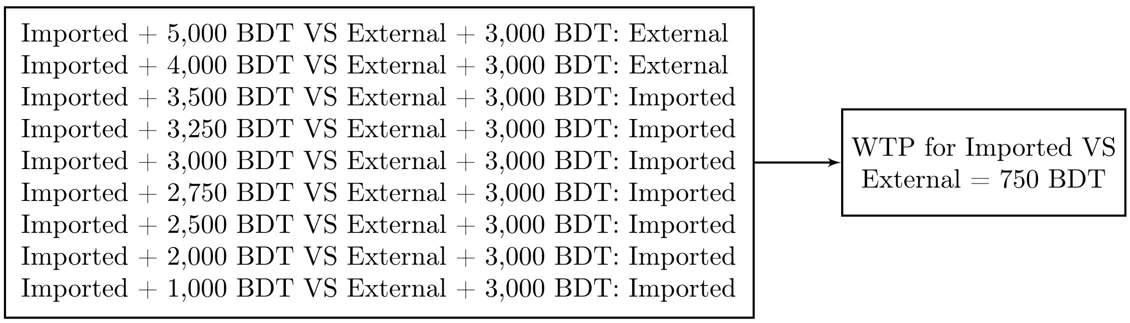
In the final sample, we weight communities in which we installed at least one well with a weight equal to one. The weight is implicitly zero is communities in which we attempted to install the maximum possible number of tubewells and all failed. In all other communities, the weights correspond to the community-specific probability of successful installation if we had made an attempt. In one treatment unit only, we made one attempt out of two possible attempts, which failed. We assign this treatment unit the probability of the second attempt being successful, had we attempted it.

Most cases of tubewell infeasibility occurred in one union. Our balancing weights therefore reduce the weight assigned to this union in the analysis. In a robustness test, we prespecify an alternative approach to dealing with this issue, which is to exclude this union from the analysis.

G Willingness to Pay calculations

We used fixed values of the community contribution in the elicitation procedure. We therefore defined the Willingness to Pay (WTP) measure using the midpoint between intervals. Appendix Figure G1 elucidates the algorithm with one example.

Figure G1: WTP calculation - Example



H Beliefs elicitation

After the elicitation procedure, we elicit beliefs on pivotality (Appendix B7), tubewell location (Appendix B8) and household contribution (Appendix B9). We use the same scenario for this set of questions, defined as one pairwise comparison between two decision-making process at one given subsidy level difference.

The decision-making process pair used in the beliefs elicitation is randomly selected at household level. The contribution requirements are endogenously defined based on the respondent's answers during the elicitation procedure. For each pairwise comparison between two decision-making processes, we identify the highest contribution at which the respondent chooses the first decision-making process. We use this contribution condition for the set of beliefs elicitation questions or, in case the respondent never prefers the first decision-making process over the second one, the lowest contribution requirement (1,000 BDT) allowed in the elicitation procedure.

We elicit beliefs on tubewell location and household contribution for the first decision-making process at the endogenously defined contribution requirement, and for the second decision-making process at 3,000 BDT only in case the respondent reports that her expected location/household contribution would be different under this second decision-making process and contribution requirement combination.

H1 Beliefs on pivotality and closeness

Appendix Figure H1 describes how responses to the questions about pivotality and closeness

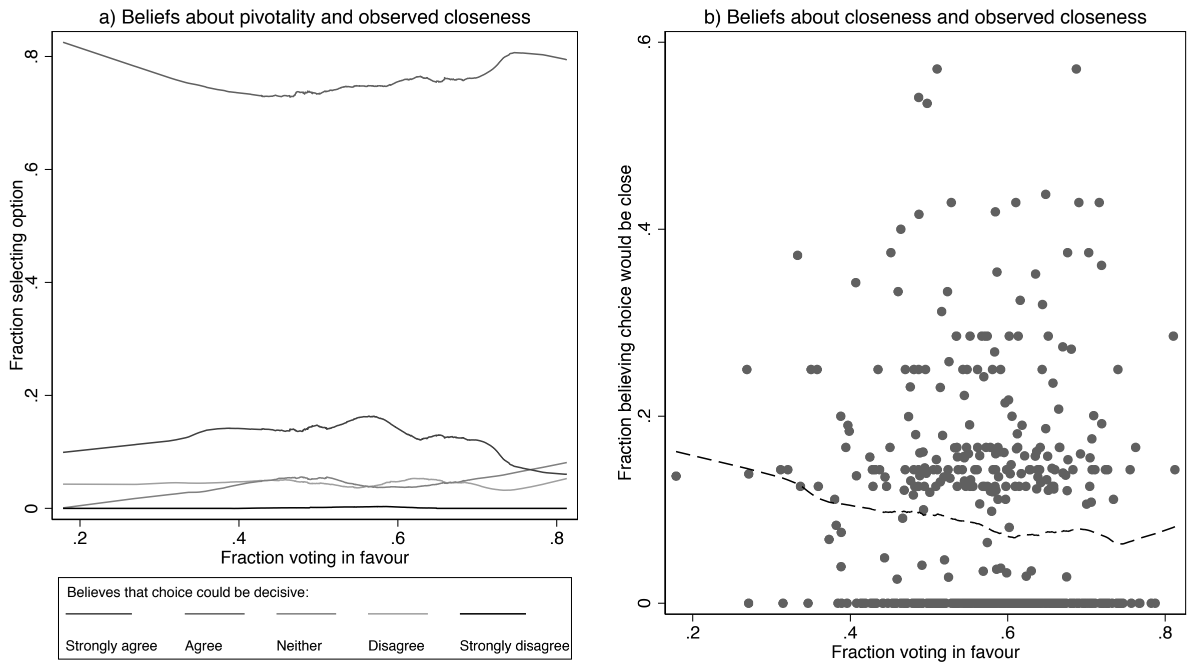
Panel a) shows the distribution of community average responses to the question of whether the households agreed with the statement that their choice "could be decisive" in determining the way the project was implemented in their community. The x axis shows the fraction of households in a give community voting in favour of one option. Communities where this fraction is around $1/2$ are communities in which the observed choice is unambiguously close. The y axis shows the fraction of households selecting each of several options in response to the question.

The large majority of households either agreed or strongly agreed that their choice could be decisive. Relatively few households disagreed with the statement, were indifferent, or did not know. The fraction of households reporting that they "strongly agree"—rather than only "agree"—with the statement increases with the true observed closeness of the decision.

Panel b) shows the distribution of community-average responses to the question of whether a household believed a choice would be close. The x axis is defined as in panel a); the y axis shows the community-average of whether households thought a particular choice would be close. In contrast to the reported perceptions about pivotality, relatively few households reported that they believed that the choice would be close. The distribution does however include treatment units where almost 60% of households believed that the decision would be close.[64]

[64] The question gave households the opportunity to report which of two options would be selected. However, we use only the variation in whether or not households reported the choice would be close because the other options did not seem to have been utilized correctly. An overwhelming majority of households selected "the first option" rather than "the second option", regardless of which model was offered first or second.

Figure H1: Beliefs about pivotality and closeness



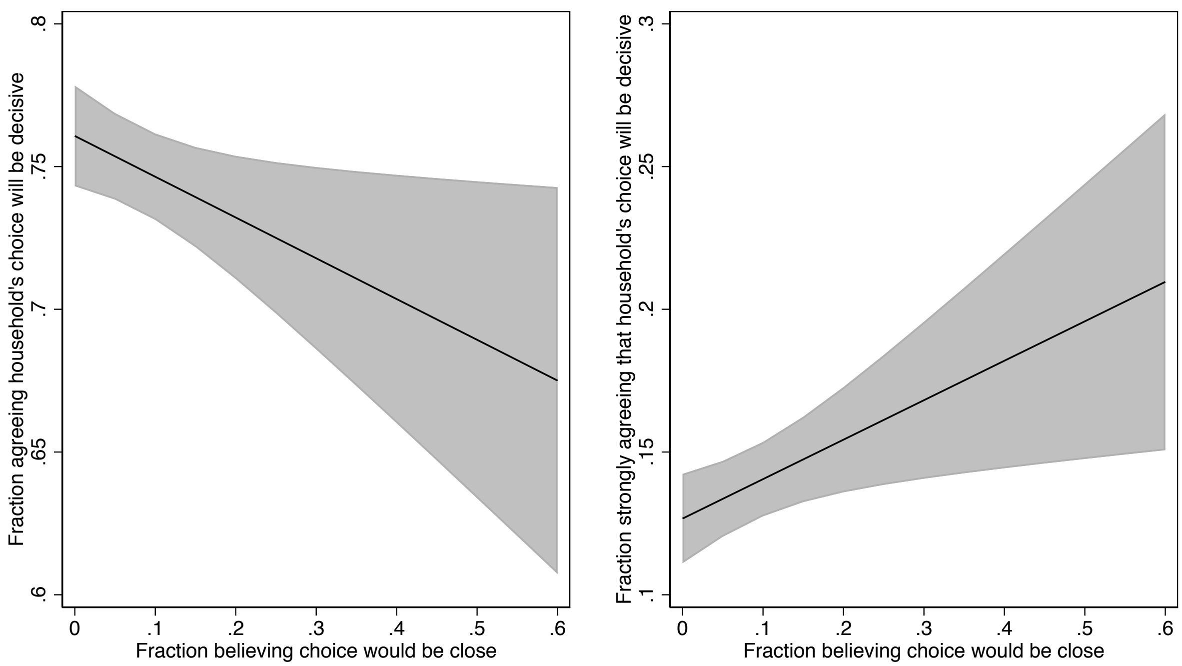
Notes: Graph summarize community-average raw data on beliefs about pivotality and closeness. Lines show non-parametrically estimated relationships; dots shown represent community-level averages for a specific choice.

Appendix Figure H2 shows the estimated relationship between the two measures of pivotality: decisiveness and closeness. We show these relationships at the treatment unit level, because we asked each household only one of the two measures of beliefs about pivotality. We randomly extracted which households to ask about decisiveness and which to ask about closeness.

Panel a) shows that the fraction of households "agreeing" with the statement about decisiveness *declines* with the fraction believing the choice would be close. Panel b) shows that the fraction of households "strongly agreeing" with the statement about decisiveness *increase* with the fraction believing the choice would be close. The rates of any other response do not change with beliefs about closeness.

The R^2 and raw correlation coefficients are however low, suggesting that most of the variation is uncorrelated. The relationship estimated is at the treatment unit level, meaning that the measures come from different households, which may partly explain the noise.

Figure H2: Correlation between beliefs about pivotality and closeness

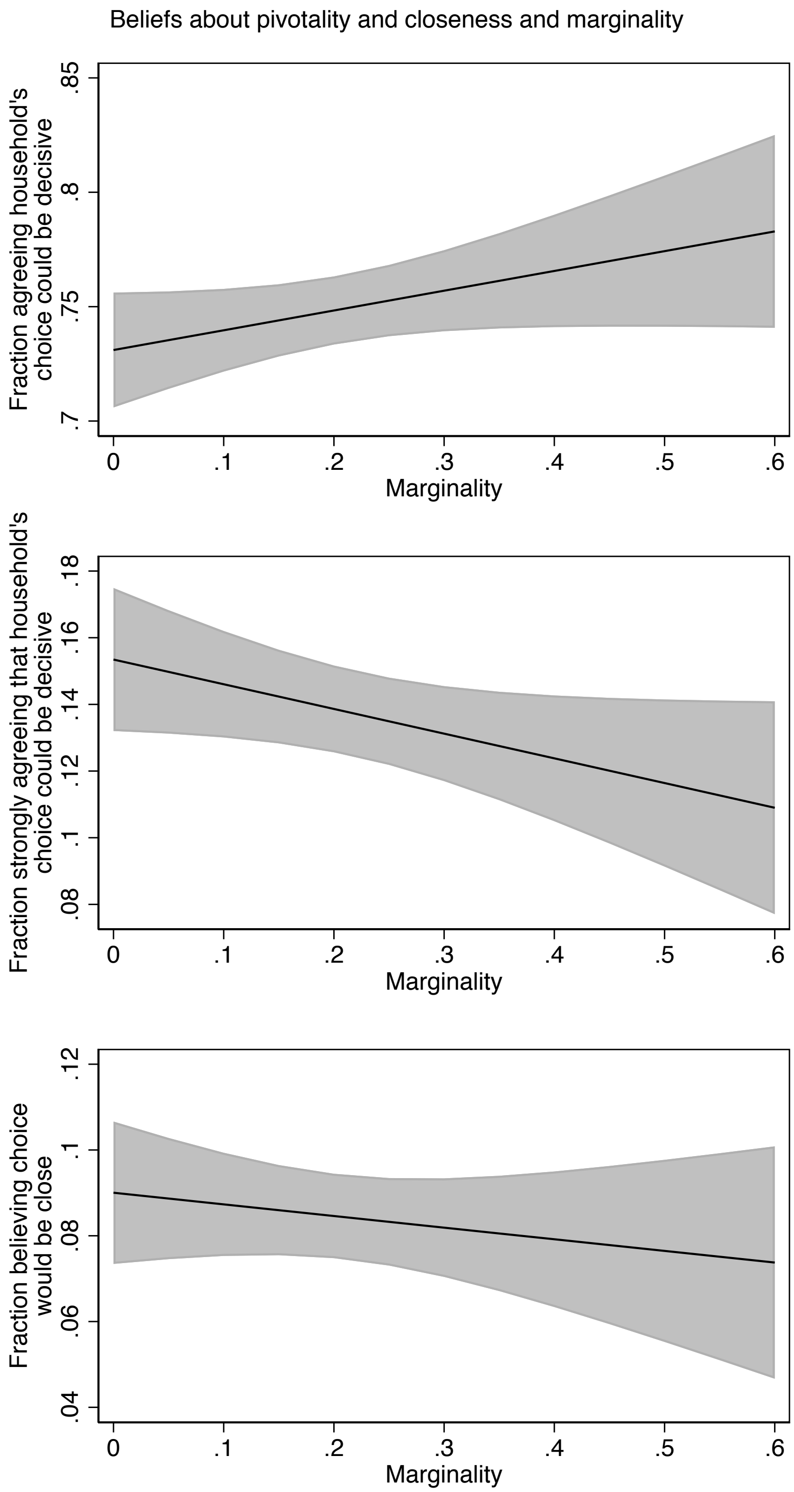


Notes: Graph plots the regression-estimated marginal linear effect of variable listed on the x axis on variable listed on the y axis from bivariate regressions. 90% confidence intervals shown. Standard error clustered by treatment unit.

Appendix Figure H3 shows the relationship between the measures of beliefs about pivotality and closeness and the true marginality of the decision taken, constructed as in Matsusaka (1993) so that marginality of zero corresponds to a perfectly close election and marginality of one would correspond to 100% consensus among voters.

Beliefs about pivotality correlate more clearly with marginality than do beliefs about closeness. However, in both cases the relationship is noisier than the relationship shown in Appendix Figure H2. This may be because the manifested marginality index is itself derived from a sample of households in the community and is therefore a noisy measure of the true marginality index.

Figure H3: Correlation between beliefs and marginality



Notes: Graph plots the regression-estimated marginal linear effect of variable listed on the x axis on variable listed on the y axis from bivariate regressions. 90% confidence intervals shown. Standard errors clustered by treatment unit.

H2 Beliefs about expected private benefit

To proxy for expected private benefit, we elicited beliefs about i) how far a water source would be constructed from a household under a given decision-making process; and ii) how much a household expected to contribute individually towards a well in a given location under a given decision-making process.

Our original project design elicited beliefs about expected locations and expected contribution for a randomly selected pair of decision-making process. As with all choices we presented to the community, one process was offered at the baseline subsidy level, corresponding to a community contribution of 3000 BDT, and one process was offered at a varying subsidy level. We asked households their beliefs about the model with the varying subsidy level at the marginal subsidy level (the highest subsidy level at which they accepted the model with the varying subsidy level). Our study design was intended to create: 1) a dataset of beliefs about contributions and locations for a randomly-extracted model at the baseline subsidy level; and 2) a dataset for each household of a complete set of beliefs on choices, expected contributions and expected locations for one pairwise choice.

Unfortunately, we were only partially successful in achieving these goals. The location elicitation procedure was somewhat demanding for both enumerators and respondents. The procedure consisted of the following steps: 1) the enumerator showed the household a laminated map of the community; 2) the household selected the expected site; 3) the enumerator opened a map interface on the tablet with offline functionality, which would automatically zoom to the centroid of the community in which the interview was taking place; 4) the enumerator selected the chosen location from the map and saving the answer. Because the process was relatively time-consuming, enumerators requested that we give them the option to record that households had the same beliefs about location for the second process as for the first. Perhaps inevitably, enumerators used this option in almost 98% of observations. As a result, we only have valid location data for the first process each household was asked about, which was also the process with the varying contribution requirement. In contrast, 37% of households reported different expectations about contributions for different processes and subsidy levels, so we believe that our contribution data is reliable with respect to both processes.

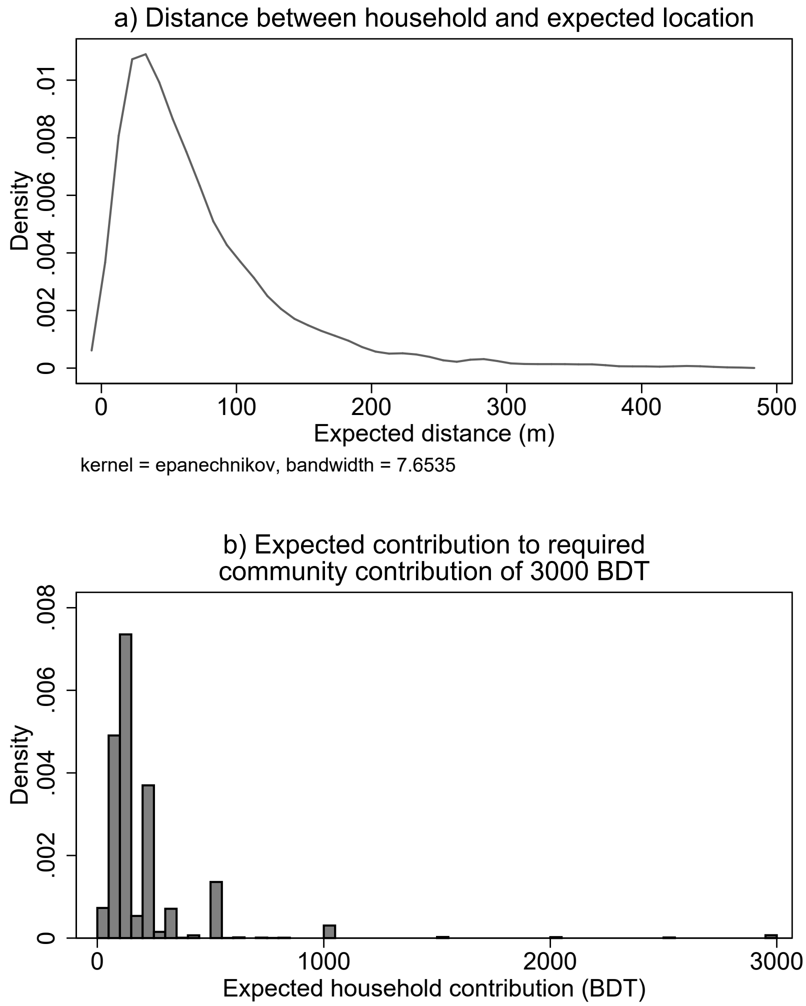
As a consequence, we have reliable beliefs data only for: 1) beliefs about location for an randomly extracted model at a subsidy level that varies depending on household preferences; and 2) contribution beliefs for two randomly-extracted processes, one at the baseline subsidy level, and one at a subsidy level that varies depending on household preferences. When we report beliefs about location, a potential concern is whether households report different locations at different subsidy levels. In our data, we cannot distinguish between differences in beliefs about location at different subsidy levels from differences in beliefs that correlate with preferences. We report results on beliefs about location with this caveat.

Otherwise, we are confident in the quality of our location measures. When first shown the map, enumerators recorded that 86% of survey respondents had a good or very good understanding of the map, 12% a limited understanding, and only 2% could not read the map. We were able to record the expected location for 95% of respondents. Among those for whom we could not record a location, only 1% could not understand the map. The remaining 4% did not know what to expect.

Appendix Figure H4 shows the distribution of beliefs about location, calculated using the distance between the household (measured using GPS) and the chosen location (GPS coordinates recorded by the enumerator by selecting the location from an interactive map interface). The right-hand tail may well correspond to outlier errors in the recorded household geocoordinates, so we mostly analyze these data using a log transformation.

Appendix Figure H4 also shows the distribution of beliefs about household contributions when the expected contribution is 3000 BDT. The vast majority of households report expecting to contribute at least a small amount, but the large majority of households (86%) report expecting to contribute no more than 200 BDT. A few (less than 1%) report expecting to contribute the full community contribution as an individual household.

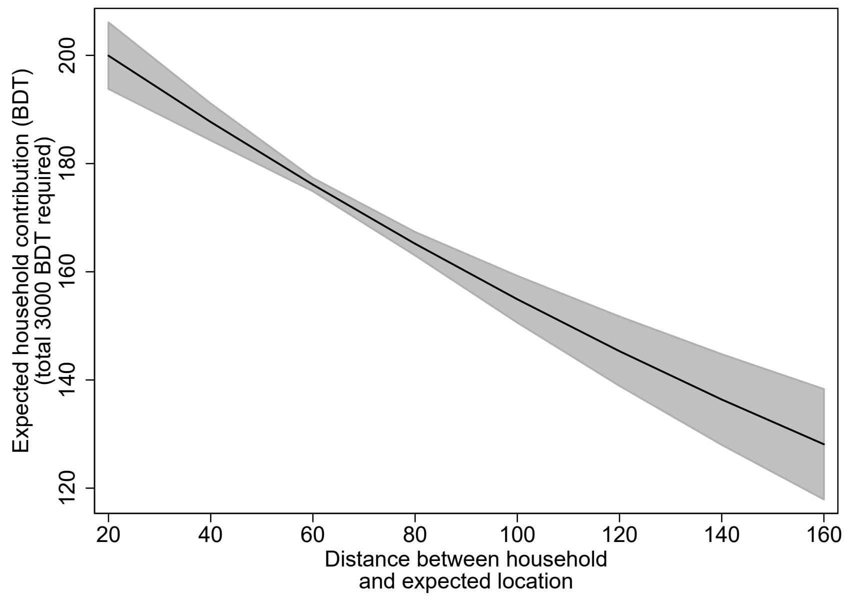
Figure H4: Beliefs about expected private benefit



Notes: Panel a) shows non-parametrically estimated density function of distances between households and expected locations, excluding outlier values with distances larger than 500m. Panel b) shows histogram of expected contributions.

Appendix Figure H5 shows that expectations about location are correlated with expected contributions: expected contributions decline by about half when we compare a household that expects a well to be built close to their home compared to one who expects a well to be built about two minutes walk from their home.

Figure H5: Correlations between beliefs about locations and contributions



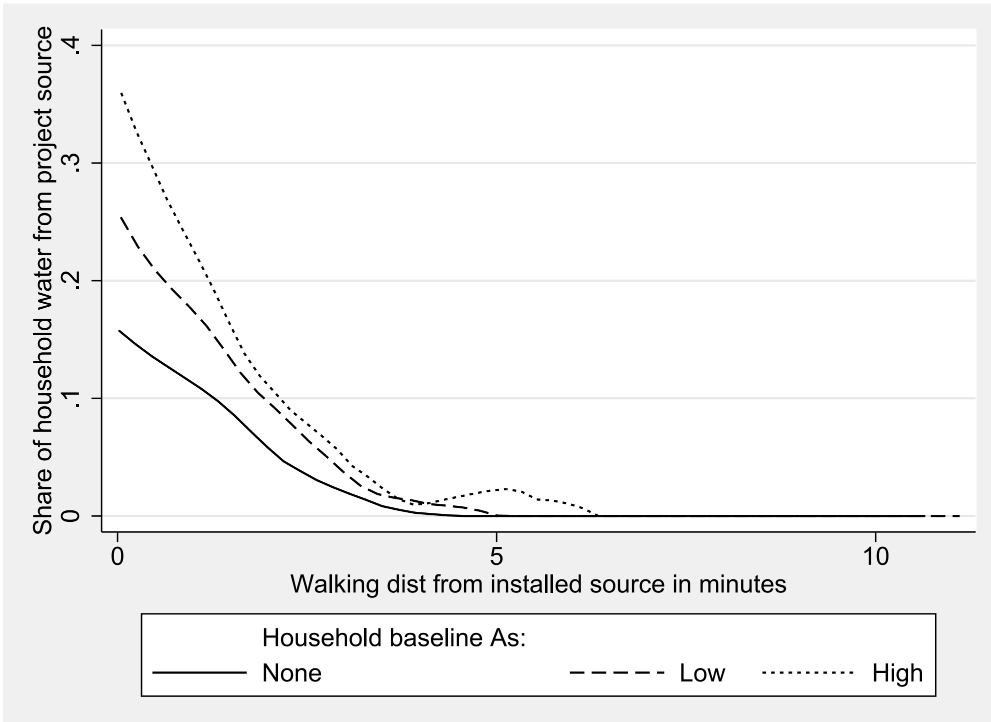
Notes: Graph plots the within-village regression-estimated marginal effect of expected household distance to well location and expected contribution, from a regression with a quadratic specification, treatment unit fixed effects and subsidy level dummies. Weights applied. 90% confidence intervals shown. Standard errors clustered by treatment unit.

H3 Beliefs about expected social benefit

We use the information about expected locations to construct a measure of expected social benefit. As a proxy for social benefit we construct a measure of predicted use at expected locations. To ensure that the measure we construct is comparable across treatment and control, we normalize the measure of predicted use by the distribution of the predicted use variable across a grid approximating the universe of possible well locations within a community.

Our predicted use measure uses information about actual take-up rates of wells that we installed during the first wave of safe drinking water projects. Appendix Figure H6 illustrates how take-up varies with baseline arsenic contamination and distance from a constructed well in communities where we successfully installed wells.

Figure H6: Use of wells installed during first wave with distance from installed source: by arsenic status



We then predict take-up at each expected location. At each expected location, we estimate the share of the community's water that could be provided by these sources, assuming average take-up rates. Specifically, we predict for each household in our follow-up dataset the share of their water they would obtain from a source constructed at a given location, using the distance between the household and the predicted location and the arsenic contamination status of the household's drinking water at follow-up. We sum across all the households in the sample, weighting according to sampling frequency, to obtain a measure of predicted take-up for the community as a whole. Appendix Figure H7, panel a), plots the distribution of predicted take-up across expected locations.

Communities treated under the first wave of safe drinking water might have lower average values of predicted take-up, if wells were already built in the areas with greatest demand. For this reason, we normalize the value of our social benefit measure with respect to the distribution of values of predicted take-up across all feasible locations in the community.

To establish the set of feasible locations in the community, we construct a 20m by 20m grid in each community, spanning the full set of geographical points (households, water sources, local landmarks) that we recorded as being located within each community. We exclude any point from the grid where there is no recorded point within 20m of the source. This excludes areas where there are no households, water sources or local landmarks. Typically, these areas are either ponds, farmland or forested areas. Appendix Figure H8 illustrates one example.

We then calculate expected predicted take-up at each of the gridpoints, using the same approach that we use to calculate expected predicted take-up for any given location. We use the distribution of values of predicted take-up across all gridpoints within a community to normalize the expected take-up at any given location, using two approaches.

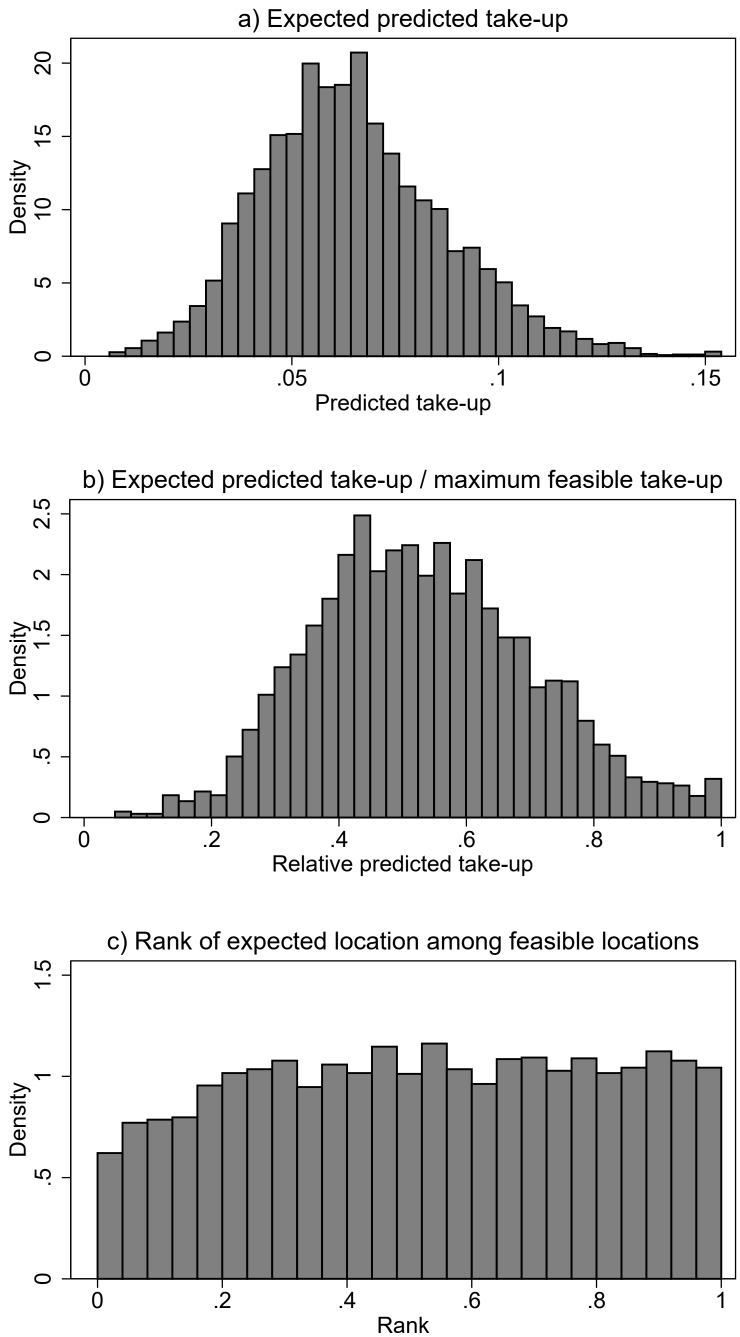
First, we divide expected predicted take-up by the maximum value of expected predicted take-up at any gridcell in the community. The resultant measure captures the percentage of maximum feasible take-up achieved by a given location. A value of 1 means that the expected location is the location within the community that maximizes predicted take-up; a value near 0 means that the expected location achieves only a very small fraction of the maximum feasible predicted take-up. Appendix Figure H7, panel b) illustrates the distribution of this measure.

Second, we compare expected predicted take-up to the distribution of values of expected predicted take-up at all gridcells in the community, to construct a percentile rank measure of location quality. A value of 1 means that 100% of the gridcell locations yield lower predicted take-up that the expected location; a value near 0 means that almost all of the gridcell locations yield higher predicted take-up. Appendix Figure H7, panel c) illustrates the distribution of this measure.

We interpret our measures of predicted use as revealed preference measures of the potential social benefit provided by a source at a given location. Our predicted use measure implicitly places more weight on households exposed to arsenic contamination, because these households adopt the new wells at higher rates than do households who are not exposed to arsenic contamination.

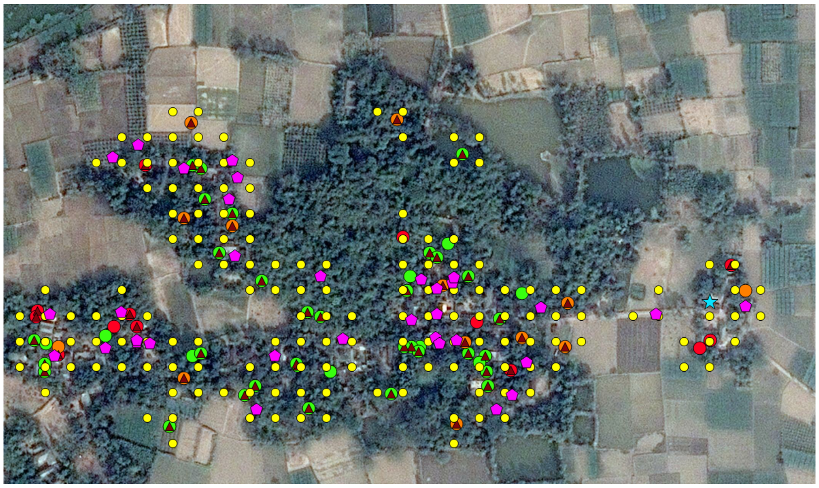
Appendix Figures H9 and H10 show how expected contributions correlate with expected social benefit. Again, the relationship is ambiguous. Households who think the well will be located in a location that serves many households may expect many households to contribute, meaning that they need to contribute less personally. On the other hand, altruistic households may be willing to contribute more to locations with higher expected social benefit. Appendix Figures H9 and H10 show that the relationship is indeed much weaker than the relationship between expected contributions and expected locations.

Figure H7: Estimated social benefits of expected well location



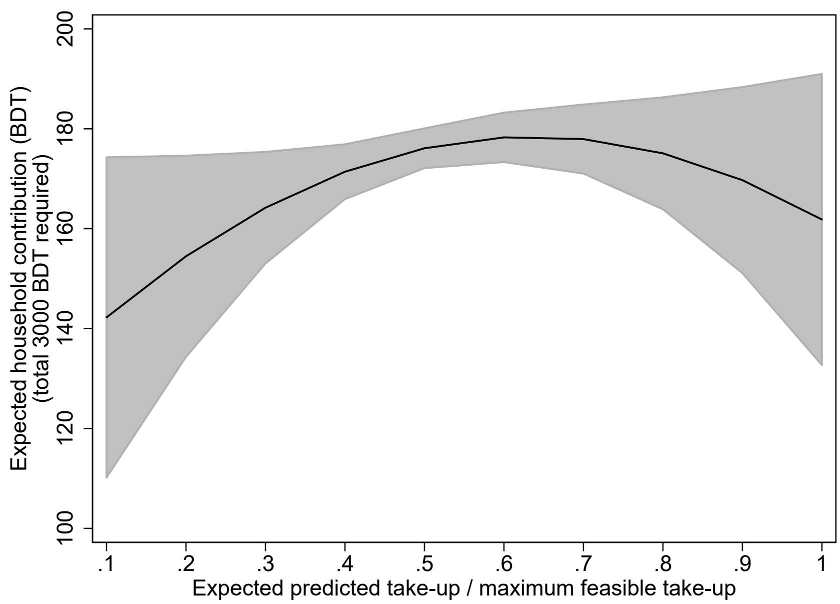
Notes: Panel a) shows distribution of predicted take-up across all expected well locations. Panel b) shows distribution of predicted take-up after normalizing with respect to the maximum predicted take-up across a 20m grid of locations within the community. Panel c) shows the distribution of the percentile rank of the expected well location among all feasible well locations in the treatment unit.

Figure H8: Example of grid of feasible locations within a community



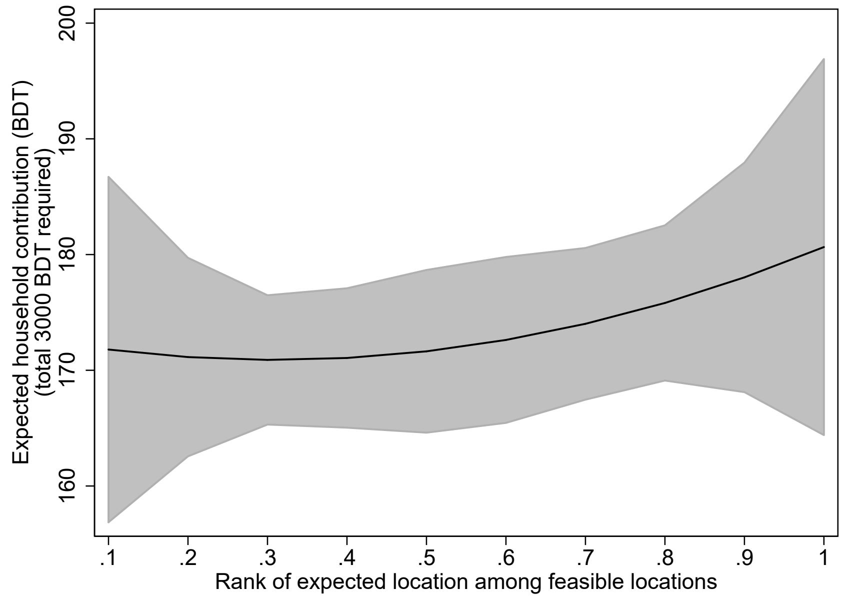
Example of grid of feasible locations. Grid is a 20m x 20m grid with limits defined by all local locations recorded during the study, excluding points that are more than 20m from any location.

Figure H9: Correlation between relative predicted expected take-up and expected household contributions



Notes: Graph plots the within-village regression-estimated marginal effect of the relative number of effective users associated to the expected well location and expected contribution, from a regression with a quadratic specification, treatment unit fixed effects and subsidy level dummies. Weights applied. 90% confidence intervals shown. Standard errors clustered by treatment unit.

Figure H10: Correlations between rank of expected location and expected household contributions



Notes: Graph plots the within-village regression-estimated marginal effect of the rank of the expected well location among all feasible well locations in the treatment unit and expected contribution, from a regression with a quadratic specification, treatment unit fixed effects and subsidy level dummies. Weights applied. 90% confidence intervals shown. Standard errors clustered by treatment unit.

I Balance checks

Appendix Tables I1 and I2 show that the treated and control populations, as defined with respect to the first wave of safe drinking water projects, are equivalent at baseline. For each characteristic shown in the tables, we estimate the following regression:

$$y_{hc} = \alpha + \beta T_c + \boldsymbol{Z_c \gamma} + \epsilon_{hc} \quad (4)$$

where y_{hc} is a baseline characteristic in household h in community c; T_c is an indicator for whether community c received treatment under the first wave of safe drinking water projects; $\boldsymbol{Z_c \gamma}$ is a vector of control variables which reflects stratification in the original randomization. We include fixed effects for each lottery at which treatment was assigned. Following Lin (2013), Imbens and Rubin (2015) and Gibbons et al. (forthcoming), we demean the lottery fixed effects and include the interaction term between the lottery controls and the treatment dummies, meaning that β consistently estimates the average difference between treated and control villages. We weight each observation to account for differences in sampling frequencies across populations and to compensate for potential imbalance induced by excluding communities in which tubewells were not feasible, as discussed in Appendices E and F.

We conduct inference using randomization-based inference, by reassigning treatment within lotteries 500 times. We then compare the coefficient β estimated under the observed treatment assignment with the distribution of $\hat{\beta}$ estimated across the 500 simulations.

The variables we evaluate in Appendix Tables I1 and I2 are not independent. Indeed, several of the variables in Appendix Table I1 are simply different transformations of the same variables or are highly correlated. To evaluate joint equality on the tests we carry out, we also carry out two tests of joint significance for the groups of variables shown. First, we regress a dummy for treatment status on all the characteristics shown in each of the two tables and lottery fixed effects, and calculate the distribution of the F-statistic on joint significance of all variables (excluding the lottery fixed effects) under the 500 replications. We then calculate the F-statistic under the observed treatment assignment and compare it to this distribution. Second, we collapse the data to community-level means and calculate the distribution of the Hotelling's T-Squared statistic under the 500 replications. We then calculate Hotelling's T-Squared statistic for the observed treatment assignment and compare it to this distribution. Both approaches yield a p value which evaluates the likelihood of observing the full set of differences across characteristics under the null hypothesis of random assignment to treatment.

The joint tests confirm that differences between treated and control groups at baseline occur only due to chance.

Table I1: Balance: Water-related characteristics

	Control	Treated	Obs
Arsenic contamination (WHO) (HH test)	0.56	.63*	6300
	(0.03)	(0.02)	
Bacteria contamination (HH test)	0.67	.61**	6281
	(0.02)	(0.01)	
Arsenic contamination (WHO) (WS test)	0.63	.7*	6302
	(0.03)	(0.02)	
Arsenic contamination (BD) (WS test)	0.24	.32**	6300
	(0.02)	(0.02)	
Bacteria contamination (WS test)	0.58	0.55	6294
	(0.02)	(0.01)	
Storage dummy (observed)	0.71	0.72	6300
	(0.02)	(0.01)	
Water is treated before drinking (primary WS)	0.081	0.090	6293
	(0.01)	(0.01)	
Time needed to collect water (mins)	2.2	2.2	5661
	(0.07)	(0.04)	
Water collected per day (litres)	2.2	2.2	6291
	(1.99)	(1.42)	
Pvalue of F-test for joint significance		0.246	5585
Pvalue of Hotelling's T-Squared test		0.822	

Notes: The table reports means and standard errors (in parentheses) of baseline characteristics in treatment and control groups. Reported standard errors are clustered by treatment unit. Significance levels indicated are obtained from randomization based inference, reassigning treament status at the community level with 500 repetitions, applying sampling weights. p values from joint F test and Hotelling's T-squared test joint significance of differences on all listed variables between treatment and control groups. * p < 0:1, ** p < 0:05, *** p < 0:01.

Table I2: Balance: Socio-economic characteristics

	Control	Treated	Obs
Household size	3.9	3.9	6303
	(0.03)	(0.03)	
Poverty score - 2 USD	0.76	0.74	6268
	(0.01)	(0.01)	
Muslim household	0.96	0.96	6297
	(0.02)	(0.01)	
Network - indegree centrality	1.1	1.0	6303
	(0.05)	(0.03)	
Network - outdegree centrality	2.8	2.9	6303
	(0.06)	(0.03)	
Leader household	0.099	0.088	6303
	(0.01)	(0.00)	
The household head has no education	0.41	0.43	6275
	(0.01)	(0.01)	
Not educated HH members (%)	0.37	0.38	6303
	(0.01)	(0.01)	
Literacy rate in the household	0.53	0.53	6292
	(0.01)	(0.01)	
The household owns livestock	0.74	.77*	6302
	(0.02)	(0.01)	
The household owns land for cultivation	0.53	0.53	6295
	(0.02)	(0.01)	
HH has some toilet facility	0.84	0.84	6303
	(0.02)	(0.01)	
Number of rooms to sleep	1.9	1.9	6299
	(0.03)	(0.02)	
The floor is made of earth or sand	0.84	0.84	6303
	(0.01)	(0.01)	
The roof is made of metal	0.95	0.97	6302
	(0.01)	(0.00)	
Mobile phone ownership	0.58	.64**	6303
	(0.02)	(0.01)	
Ownership of a motorized vehicle	0.055	0.068	6299
	(0.01)	(0.00)	
Wealthy households (top-quintile HH assets index)	0.19	0.21	5607
	(0.02)	(0.01)	
High trust towards community	0.48	0.48	6300
	(0.02)	(0.01)	
Know association	0.28	0.30	6227
	(0.02)	(0.01)	
Pvalue of F-test for joint significance		0.790	5452
Pvalue of Hotelling's T-Squared test		0.742	

Notes: The table reports means and standard errors (in parentheses) of baseline characteristics in treatment and control groups. Reported standard errors are clustered by treatment unit. Significance levels indicated are obtained from randomization based inference, reassigning treament status at the community level with 500 repetitions, applying sampling weights. p values from joint F test and Hotelling's T-squared test joint significance of differences on all listed variables between treatment and control groups. * p < 0:1, ** p < 0:05, *** p < 0:01.

In a robustness check (Section 10.4), we will use an alternative approach to dealing with the excluded communities in which tubewells were not feasible, dropping the union in which 5 out 6 exclusions were made and omitting the balancing weights. For comparison, Appendix Tables I3 and I4 show balance tables corresponding to these specifications.

Table I3: Balance: Water-related characteristics - Excluding one union

	Control	Treated	Obs
Arsenic contamination (WHO) (HH test)	0.54	.61*	5289
	(0.03)	(0.02)	
Bacteria contamination (HH test)	0.66	.6*	5274
	(0.03)	(0.01)	
Arsenic contamination (WHO) (WS test)	0.61	.69*	5292
	(0.03)	(0.02)	
Arsenic contamination (BD) (WS test)	0.24	.32**	5290
	(0.02)	(0.02)	
Bacteria contamination (WS test)	0.59	0.55	5285
	(0.02)	(0.01)	
Storage dummy (observed)	0.70	0.71	5289
	(0.02)	(0.01)	
Water is treated before drinking (primary WS)	0.081	0.092	5285
	(0.01)	(0.01)	
Time needed to collect water (mins)	2.2	2.2	4757
	(0.08)	(0.04)	
Water collected per day (litres)	2.2	2.2	5285
	(2.24)	(1.55)	
Pvalue of F-test for joint significance		0.344	4689
Pvalue of Hotelling's T-Squared test		0.768	

Notes: The table reports means and standard errors (in parentheses) of baseline characteristics in treatment and control groups. Reported standard errors are clustered by treatment unit. Significance levels indicated are obtained from randomization based inference, reassigning treament status at the community level with 500 repetitions, applying sampling weights. p values from joint F test and Hotelling's T-squared test joint significance of differences on all listed variables between treatment and control groups. * p < 0:1, ** p < 0:05, *** p < 0:01.

Table I4: Balance: Socio-economic characteristics - Excluding one union

	Control	Treated	Obs
Household size	3.9	3.9	5292
	(0.03)	(0.03)	
Poverty score - 2 USD	0.76	0.74	5261
	(0.01)	(0.01)	
Muslim household	0.97	0.97	5287
	(0.02)	(0.01)	
Network - indegree centrality	1.1	1.1	5292
	(0.06)	(0.03)	
Network - outdegree centrality	2.8	2.9	5292
	(0.06)	(0.04)	
Leader household	0.098	0.088	5292
	(0.01)	(0.00)	
The household head has no education	0.42	0.43	5268
	(0.02)	(0.01)	
Not educated HH members (%)	0.38	0.39	5292
	(0.01)	(0.01)	
Literacy rate in the household	0.53	0.53	5281
	(0.01)	(0.01)	
The household owns livestock	0.72	.77**	5291
	(0.02)	(0.01)	
The household owns land for cultivation	0.53	0.53	5284
	(0.02)	(0.01)	
HH has some toilet facility	0.83	0.83	5292
	(0.02)	(0.01)	
Number of rooms to sleep	1.9	1.8	5290
	(0.03)	(0.02)	
The floor is made of earth or sand	0.83	0.85	5292
	(0.02)	(0.01)	
The roof is made of metal	0.95	0.97	5292
	(0.01)	(0.01)	
Mobile phone ownership	0.60	.66**	5292
	(0.03)	(0.01)	
Ownership of a motorized vehicle	0.057	0.068	5288
	(0.01)	(0.00)	
Wealthy households (top-quintile HH assets index)	0.19	0.21	4665
	(0.02)	(0.01)	
High trust towards community	0.49	0.48	5289
	(0.02)	(0.01)	
Know association	0.28	0.29	5216
	(0.02)	(0.01)	
Pvalue of F-test for joint significance		0.738	4518
Pvalue of Hotelling's T-Squared test		0.968	

Notes: The table reports means and standard errors (in parentheses) of baseline characteristics in treatment and control groups. Reported standard errors are clustered by treatment unit. Significance levels indicated are obtained from randomization based inference, reassigning treament status at the community level with 500 repetitions, applying sampling weights. p values from joint F test and Hotelling's T-squared test joint significance of differences on all listed variables between treatment and control groups. * p < 0:1, ** p < 0:05, *** p < 0:01.

J Power calculations

J1 Ex-ante power calculations

In this section, we describe our original power calculations, conducted at the time of study design. Our most conservative power calculations exploited only the variation at community level. These power calculations implied minimum detectable effects of 0.50 standard deviations for the baseline comparison between control and treated communities, with standard significance level at 5% and power 80%. With our preferred specification at participant level, the minimum detectable effect reduces to 0.28-0.36 standard deviations, assuming intra-cluster correlations between 0.3 and 0.5 (Hemming et al., 2011).

Note that in the absence of data on preferences over institutions, we used data from a previous lab-in-the-field experiment described in Cocciolo (2019), carried out between December 2016 and May 2017 in the same communities and with the same households to conduct power calculations. Cocciolo (2019) implements a lab-in-the-field experiment to elicit WTP for participatory decision-making and evaluates how it is altered with exposure to democratic institutions through the same (first wave) safe drinking water projects. The intra-cluster correlation range we used for these ex-ante power calculations was based on the distribution of the WTP for participatory decision-making measured during this lab-in-the-field experiment.

J2 Updated power calculations using real data

For this pre-analysis plan, we have updated our power calculations making use of the real data, blind to treatment status with respect to the first wave of safe drinking water projects, as with all all analysis in this document. As discussed in the text, we update our analytical power calculations using the observed intra-cluster correlations. We also carry out simulation-based power calculations.

In our power calculation simulations, we permute treatment status across our communities, respecting the stratified approach of assigning treatment to communities at lotteries. For each of 500 replications, we simulate a plausible effect size by using the shift in the WTP distribution observed in Cocciolo (2019) (Appendix Table J1), who estimates the effect of the same first wave of safe drinking water projects on the willingness to pay for participating in group decision-making in a lab-in-the-field experiment conducted in the same communities and with the same households. Specifically, we categorize households in 5 categories of WTP. For each simulation, we randomly assign households in each category to change their WTP in proportions obtained from the treatment effects in Cocciolo (2019).

Table J1: Price list

WTP category	Shift (pp)
Minimum WTP	-0.51
Negative WTP	-3.51
0 WTP	-0.15
Positive WTP	-3.28
Maximum WTP	+8.84

Using the simulated data, we calculate both the K-S statistics and the coefficient on the treatment dummy from a regression with union FE, standard errors clustered at community level and balancing weights as in the main specification (Section 10.1). We obtain the standard deviations of the regression coefficients of interest from their empirical distributions and calculate the minimum detectable effects (MDEs) by multiplying the standard deviations by 2.8.

We use the empirical distribution of the K-S statistics under the null and under the expected effect size in order to obtain the relevant thresholds for 95% significance level and for 80% power. The MDEs for the K-S test are calculated by summing these two thresholds. Specifically, we examine the distribution of the K-S statistic under the null to identify how large the estimated effect would need to be to allow us to reject the null with 95% confidence. Then, we examine the distribution of the K-S statistic under the simulated treatment effects to identify how large the true effect (we use the median of the KS statistics estimated under the simulated treatment effect) would need to be for 80% of the estimated treatment effects to be larger than the threshold identified to reject the null with 95% confidence.

K Lasso algorithm

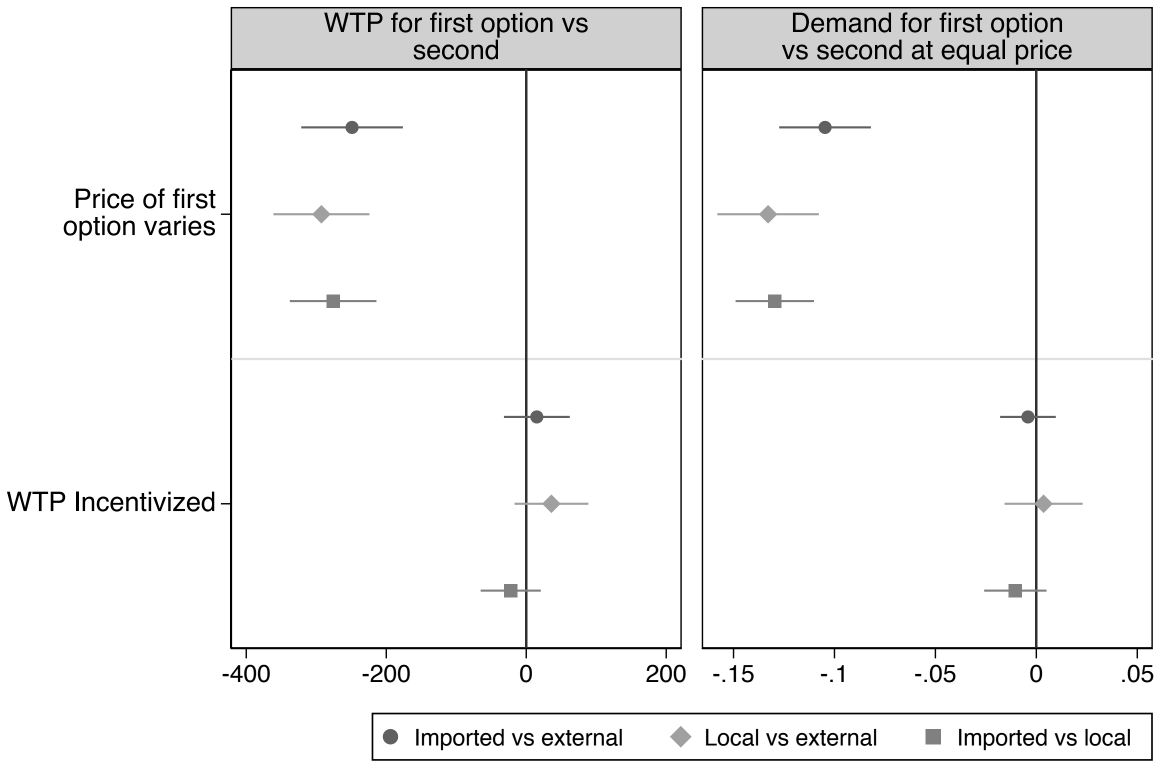
In one of our robustness checks, we will estimate the main effects using control variables, using the Lasso algorithm to select the optimal set of controls (Section 10.4). We limit the list of controls we will use to variables that are either measured at baseline for all households (leadership status and network indegree centrality) or are unlikely to be affected by exposure to the safe drinking water program.

The list of controls included in the Lasso algorithm will be the following:

- Respondent gender
- Household size
- Poverty score - 2 USD
- Muslim household
- Network - indegree centrality
- Leader household
- The household head has no education
- Not educated HH members (%)
- Literacy rate in the household
- The household owns livestock
- The household owns land for cultivation
- HH has some toilet facility
- Number of rooms to sleep
- The floor is made of earth or sand
- The roof is made of metal
- Mobile phone ownership
- Ownership of a motorized vehicle
- Wealthy households (top-quintile HH assets index)

L Additional Figures and Tables

Figure L1: Effect of experimental treatments on reported preferences



Notes: Graph shows regression-estimated difference in outcome variables and 90% confidence intervals from a regression on the treatment dummy and union fixed effects. Weights applied. Standard errors clustered by community.

Figure L2: Heterogeneous responses by fixed and varying price

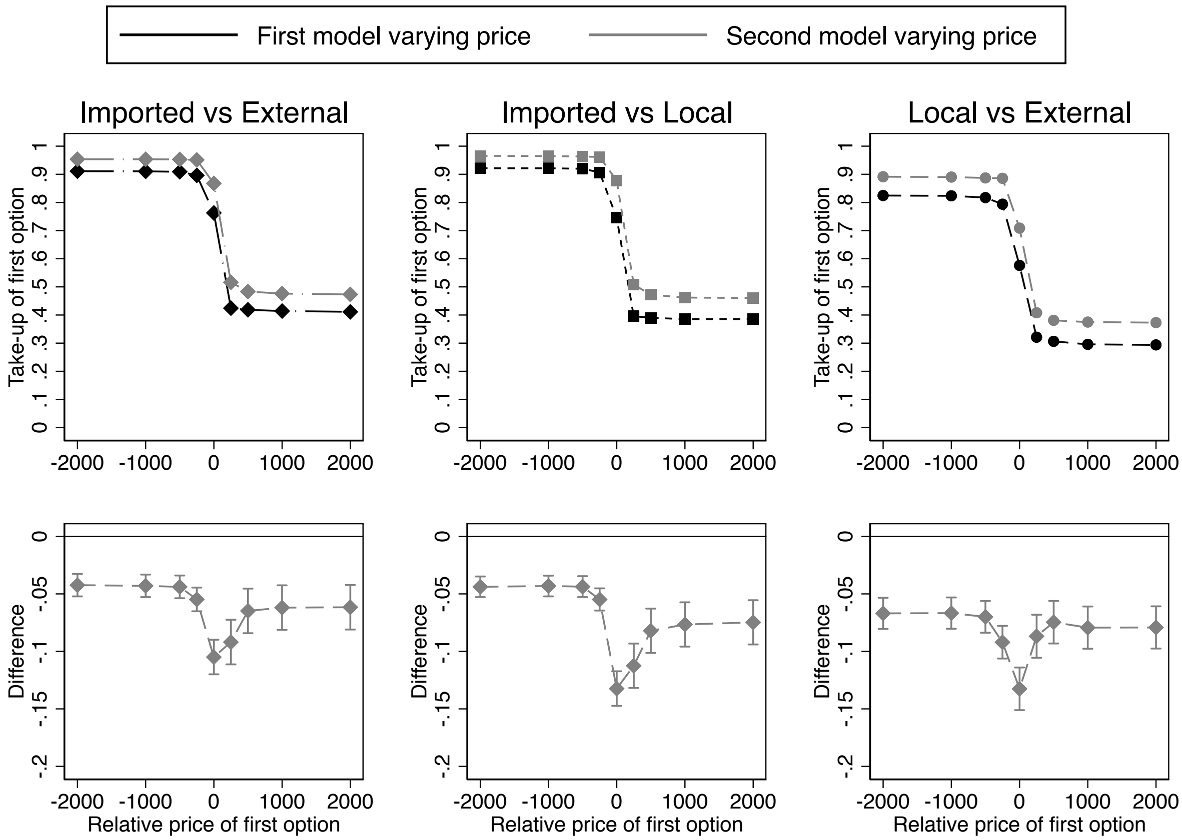


Figure L3: Heterogeneous responses by incentivization

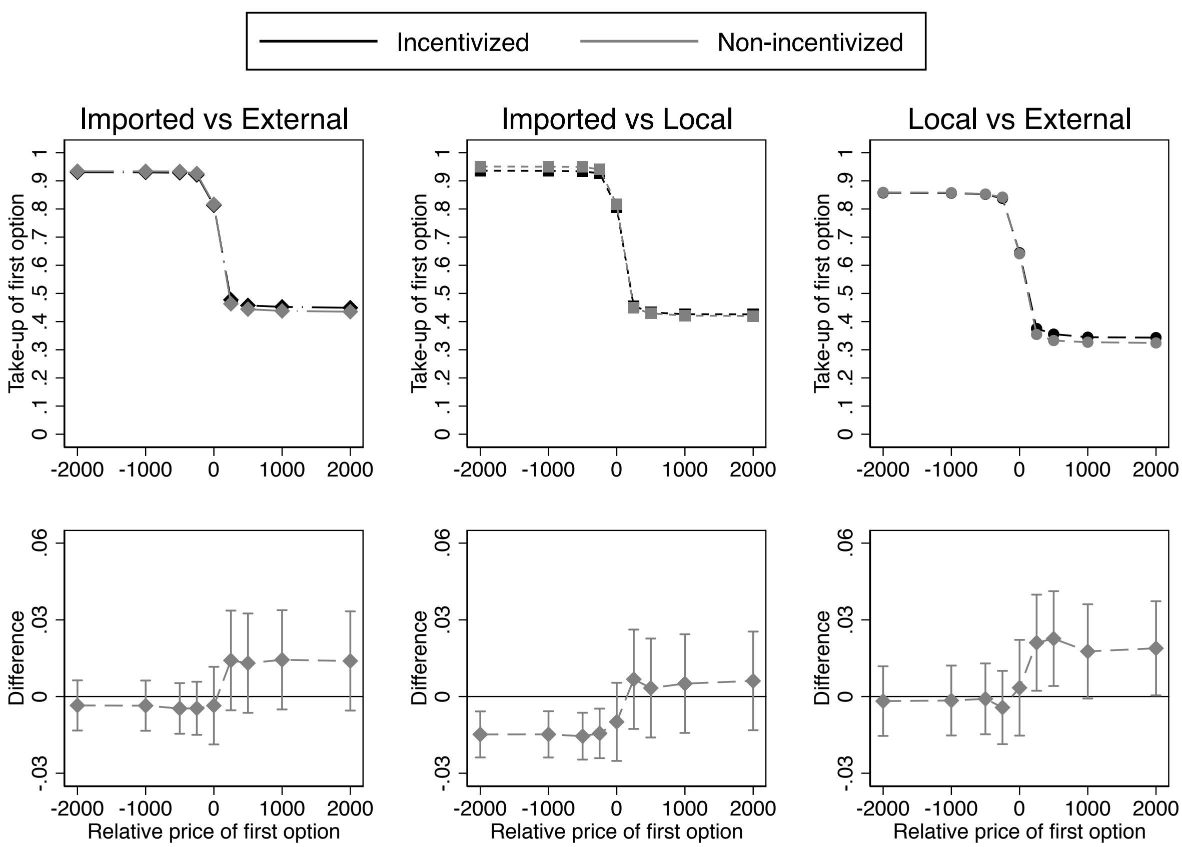


Figure L4: Heterogeneous responses by gender

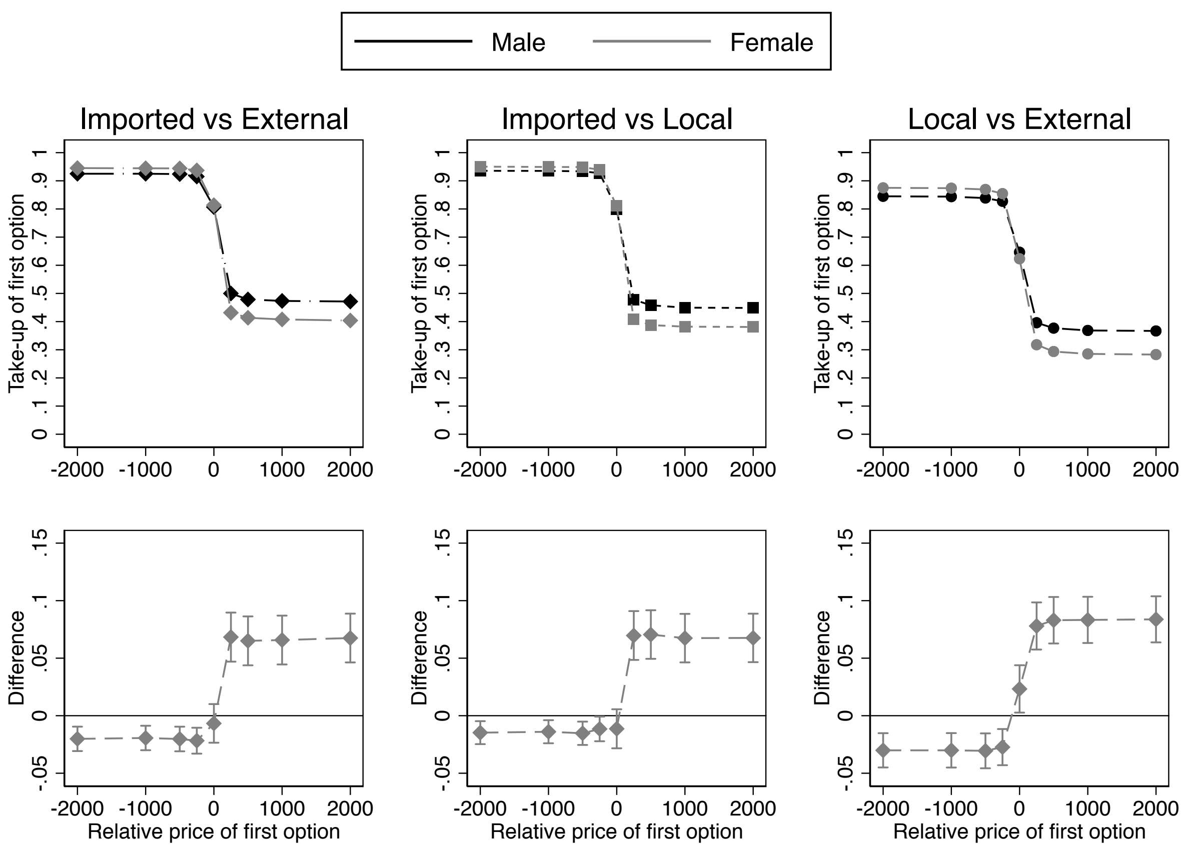


Figure L5: Heterogeneous responses by poverty status

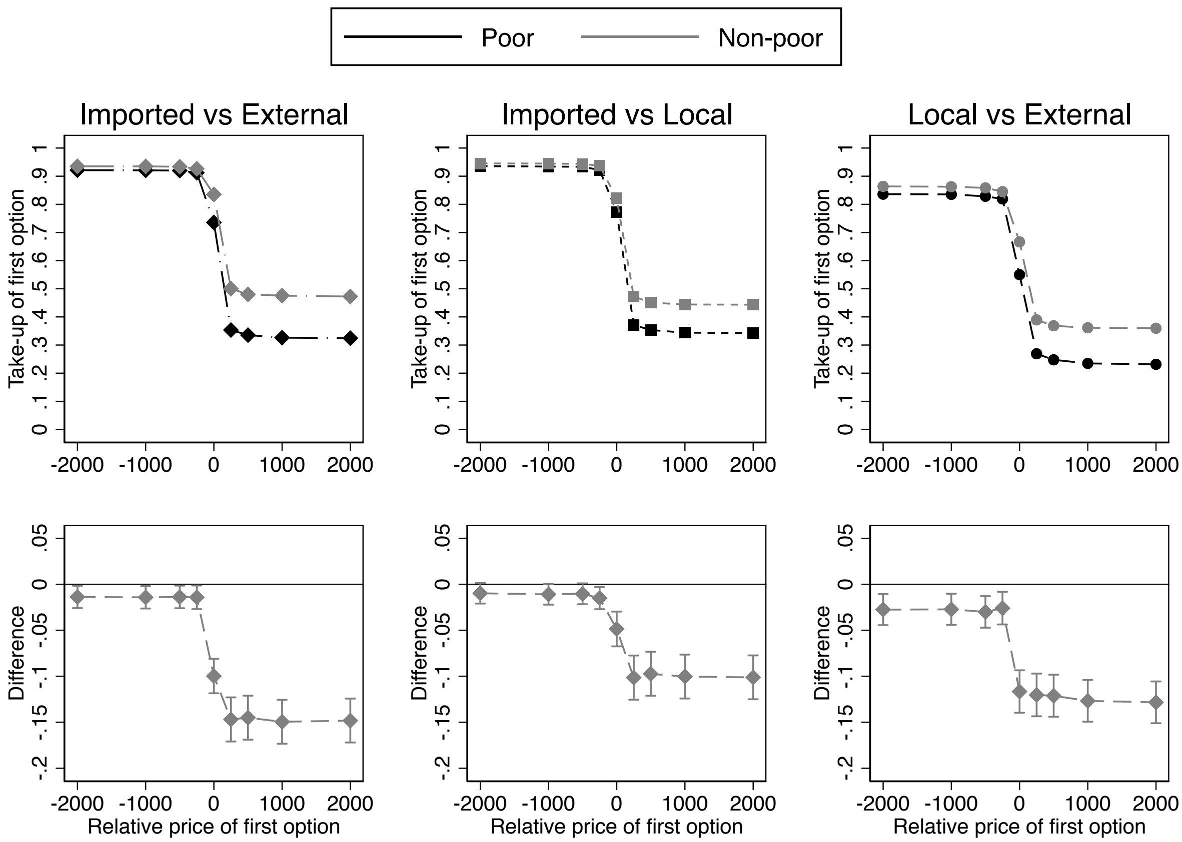


Figure L6: Heterogeneous responses by education level

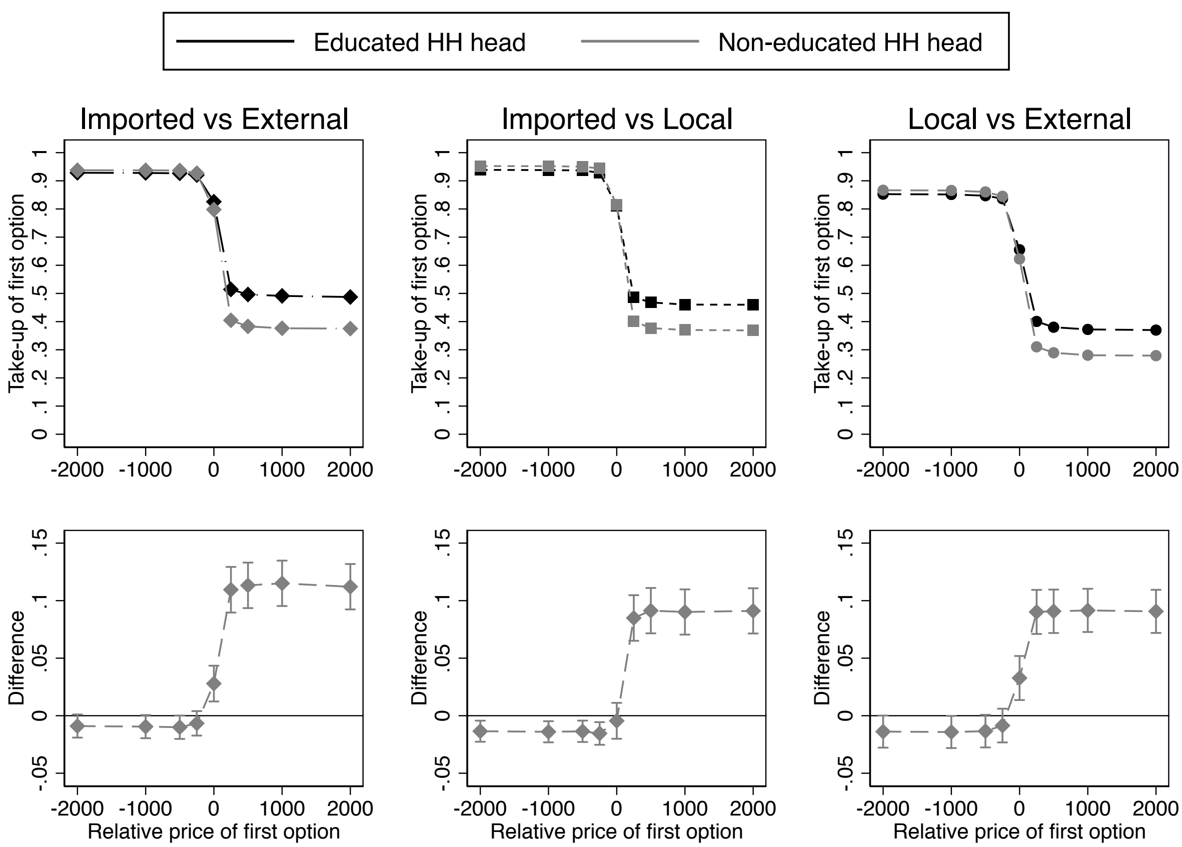


Figure L7: Heterogeneous responses by religious minority status

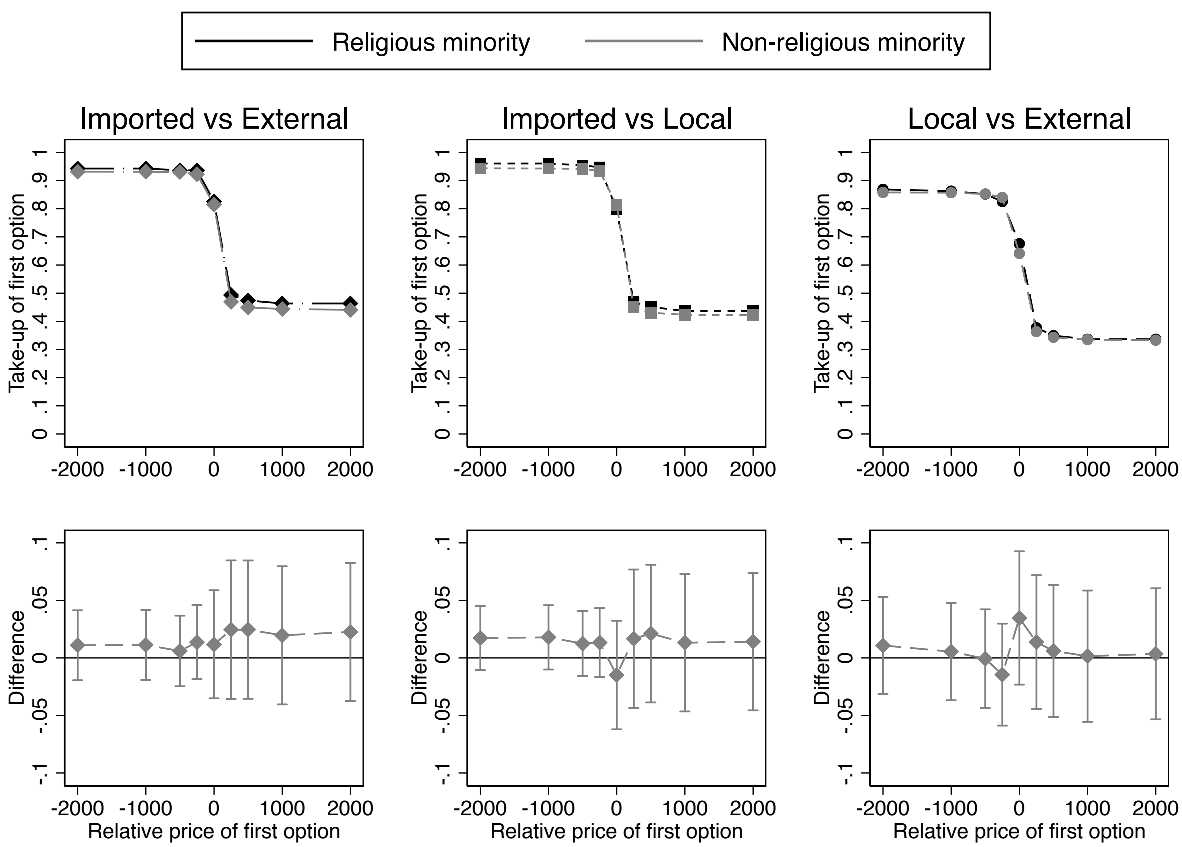


Figure L8: Heterogeneous responses by high income status

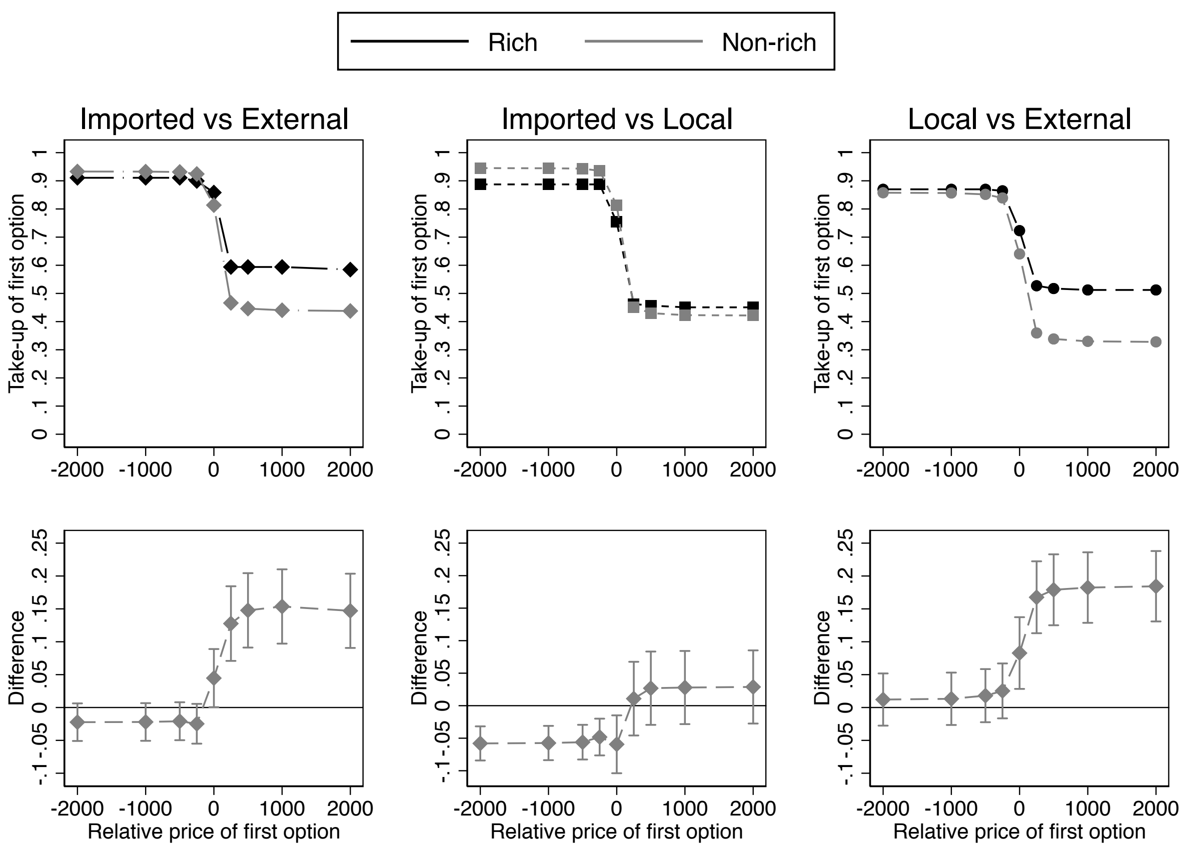


Figure L9: Heterogeneous responses by leader status

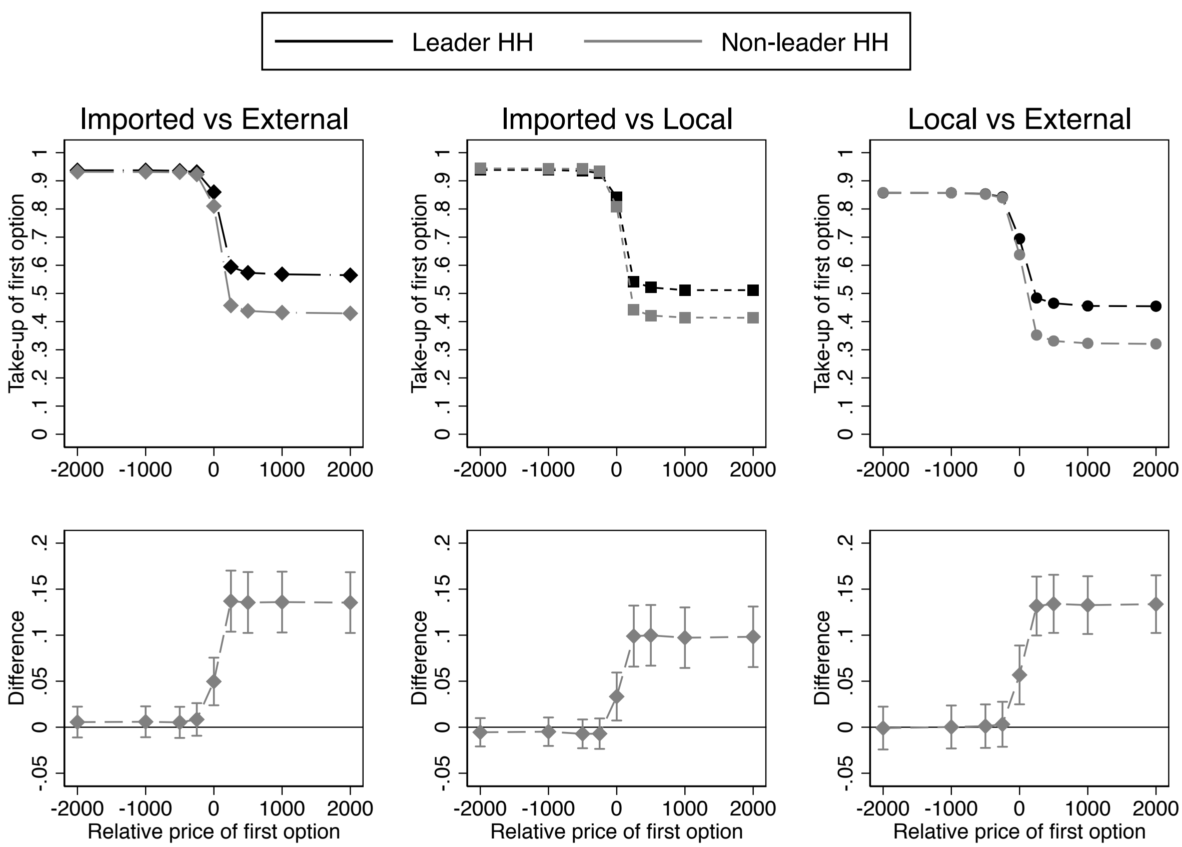


Figure L10: Heterogeneous responses by network indegree centrality

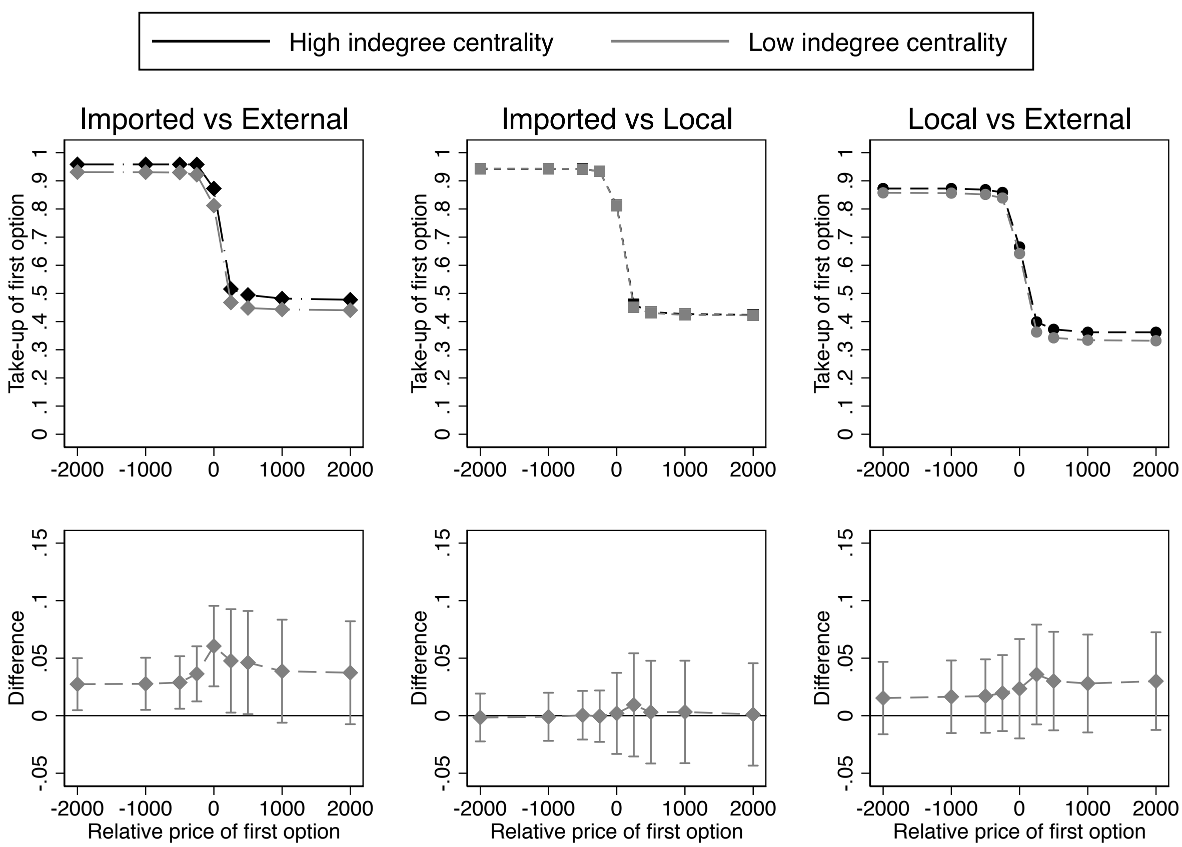


Figure L11: Heterogeneous responses by baseline involvement in community decision-making

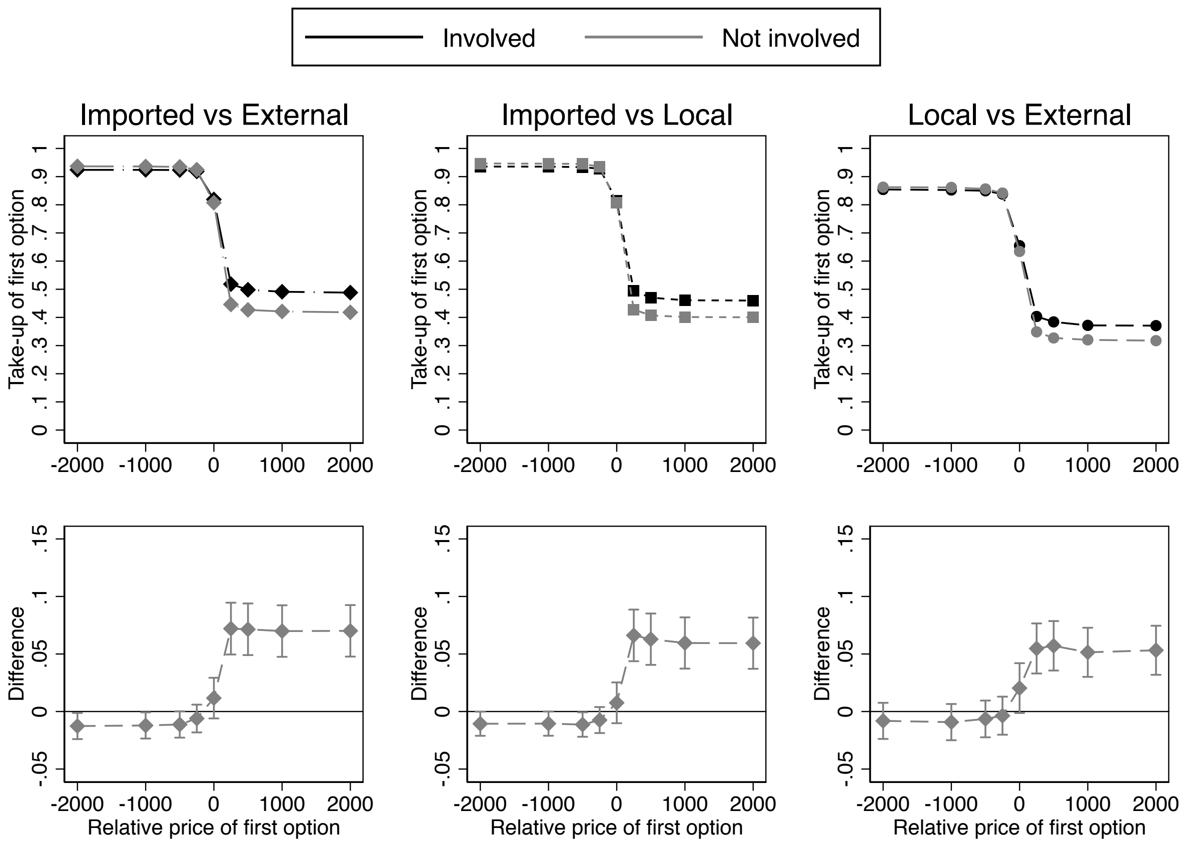


Table L1: Validating extreme WTPs

Dependent variable: Extreme WTP	Imported vs External		Local vs External		Imported vs Local	
	(1)	(2)	(3)	(4)	(5)	(6)
Instruction time (mins)	0.0017	0.0045***	0.0043***	0.0064***	0.0029**	0.0053***
	(0.0013)	(0.0013)	(0.0014)	(0.0014)	(0.0014)	(0.0013)
Enumerators experience (months)	0.0023	-0.0055	0.0203***	0.0119**	-0.0021	-0.0105*
	(0.0062)	(0.0054)	(0.0059)	(0.0048)	(0.0063)	(0.0056)
Time for WTP elicitation (mins)	0.0008	0.0014***	-0.0001	0.0044***	-0.0071***	0.0008
	(0.0005)	(0.0005)	(0.0014)	(0.0010)	(0.0015)	(0.0014)
Enumerator FE		✓		✓		✓
N	6919	6919	6918	6918	6920	6920

Notes: Standard errors clustered at community level.

Table L2: Validating intransitive preferences

	Intransitive preferences	
	(1)	(2)
Instruction time (mins)	-0.0019***	-0.0007
	(0.0005)	(0.0005)
Enumerators experience (months)	-0.0045**	-0.0068***
	(0.0022)	(0.0026)
Time for WTP elicitation (mins)	0.0003	0.0004*
	(0.0002)	(0.0002)
Enumerator FE		✓
N	6919	6919

Notes: Standard errors clustered at community level.

Table L3: Correlations between households characteristics

	Female respondent	Uneducated HH head	Poor HH	Religious minority	High income HH	Leader HH	Network HH	Involved in community decisions
Poverty index	0.098	0.18	0.34	0.023	-0.19	-0.20	-0.062	-0.22
The household owns any land	-0.043	-0.077	-0.20	-0.015	0.047	0.069	0.0076	0.085
The floor is made of earth or sand	0.025	0.23	0.22	-0.0025	-0.24	-0.20	-0.043	-0.24
Mobile phone ownership	-0.032	-0.14	-0.13	-0.0013	0.033	0.038	0.0065	0.068
Ownership of a motorized vehicle	-0.046	-0.15	-0.14	-0.0040	0.23	0.16	0.027	0.13
Wealthy HH (top-5% assets index)	-0.050	-0.12	-0.11	-0.0021	0.31	0.22	0.071	0.14
Arsenic contaminated HH (WHO)	0.014	0.026	-0.0025	-0.015	-0.020	0.011	0.018	-0.037
Bacteria contaminated HH	-0.0020	0.0087	0.046	0.0024	-0.043	-0.0039	-0.017	-0.033

Notes: Weights applied.

Table L4: Correlations between households characteristics

	Female respondent	Uneducated HH head	Poor HH	Religious minority	High income HH	Leader HH	Network HH	Involved in community decisions
Female respondent	1.0							
Uneducated HH head	0.021	1.0						
Poor HH	0.024	0.18	1.0					
Religious minority	0.0022	-0.031	0.025	1.0				
High income HH	-0.056	-0.086	-0.090	-0.027	1.0			
Leader HH	-0.042	-0.13	-0.091	-0.016	0.12	1.0		
Network HH	0.0034	-0.026	-0.046	-0.015	0.015	0.14	1.0	
Involved in community decisions	-0.044	-0.13	-0.13	-0.012	0.100	0.22	0.019	1.0

Notes: Weights applied.