Journal of Development Economics

Informal Risk Sharing to Mitigate Local Environmental Risks

--Manuscript Draft--

Manuscript Number:	DEVEC-D-22-01313R1
Article Type:	Registered Report Stage 1: Proposal
Section/Category:	Insurance, Agriculture, Land, Credit, poverty, Africa - Micro, households, gender
Keywords:	Informal risk sharing; Commitment; peer-monitoring; social network
Corresponding Author:	Prabhat Barnwal East Lansing, MI UNITED STATES
First Author:	Prabhat Barnwal
Order of Authors:	Prabhat Barnwal
	Alexander van Geen
	Yiqian Wang
Abstract:	Informal risk sharing between households can help smooth consumption during shocks. We study how ex-ante commitments to mutually insure each other's consumption shape risk-sharing in the context of an environmental disaster – arsenic poisoning of groundwater wells in Bangladesh. Arsenic contamination of wells occurs naturally, varies at short distances, and is hard to predict without testing. However, households can potentially switch to a nearby safe well. In a field experiment in 135 villages, we evaluate the impact of facilitating ex-ante commitments between household pairs to share safe water. These commitments are formed before households get to know their well's arsenic status. We implemented this intervention right before the roll-out of a nationwide program to test wells. In addition, a two-part intervention informs households about future peer-monitoring of these commitments before testing and implements it after testing. This design allows us to study how risk-sharing networks evolve under different enforcement environments.
Response to Reviewers:	

Powered by Editorial Manager® and ProduXion Manager® from Aries Systems Corporation

Department of Economics Michigan State University
East Lansing, MI 48824
Email: prabhat@msu.edu

April 21, 2023

The Editor
Journal of Development Economics

Dear Prof. Foster/ Prof. Karlan,

We thank you for your thoughtful comments and for giving us the opportunity to revise this manuscript.

We apologize for the delay in revision. I had to take some weeks off on parental leave which significantly delayed it on our end.

Please find enclosed our revised manuscript titled "Informal Risk Sharing to Mitigate Local Environmental Risks" for submission to JDE's pre-results review track. We have also attached our response to your and the referees' comments in one pdf document.

Thank you.

Sincerely,
Prabhat Barnwal

Response to the Editor and Referees

Informal Risk Sharing to Mitigate Local Environmental Risks (Pre-Results Review)

Prabhat Barnwal* and Alexander van Geen† and Yiqian Wang‡

April 22, 2023

Dear Prof. Foster, Thank you for providing us with useful comments and the opportunity to revise our manuscript. We reproduce your and referees' comments in italics and provide a point-wise response in this document and two attachments. We took the liberty to break down longer comments in parts to improve tractability and clarity.

Thank you for submitting your manuscript to the Journal of Development Economics as a Phase 1 Registered Report. I have received two reports and both referees recommend a revision. Based on these reports and my own reassessment I am requesting a revision. I think the points can be addressed quickly and I don't anticipate needing to send the Phase 1 report to referees again so please get this back in as soon as possible so we have a basis on which to transition to Phase 2. I think the referees make some good points.

1. First, there is a concern about attrition. You have some evidence to date on that. It may be attrition will be lower with an in-person survey but the registered report should document how you would evaluate attrition in the end line and how this would be incorporated into the analysis.

Response We have added a subsection on attrition in the revised draft following your comment above and R2's comment #3. The following para provides a summary.

Section 4 in the revised draft describes our strategy to address concerns about attrition. In sum, we will first test whether attrited households differ from non-attrited households by comparing household characteristics collected at the baseline. Second, we will test for treatment group-specific differential attrition. We will apply the Lee bounds to correct for the attrition bias if there is a significant level of treatment group-specific attrition. This section also discusses plausible sources of attrition.

*Department of Economics, Michigan State University. 486 W. Circle Drive 110 Marshall-Adams Hall, East Lansing, MI 48824-1038 (email: prabhat@msu.edu.)

†Lamont-Doherty Earth Observatory, Columbia University, Palisades, NY 10964 (e-mail: avangeen@ldeo.columbia.edu.)

‡Department of Economics, Michigan State University. 486 W. Circle Drive 110 Marshall-Adams Hall, East Lansing, MI 48824-1038 (email: wangy160@msu.edu.)

2. Second, there is some concern about
(a) how to think about the effects of the different treatments on the set of people with whom one has contracts. How do you assess whether the set of people with whom one shares coupons varies with peer monitoring? And if so, is there any way to establish whether the peer effect is significant conditional on endogenous uptake?
(b) There is also a concern expressed by the first referee about what happens when the peer has a selfish interest related to enforcement.

Response. (a)

Our design allows us to test whether potential peer monitoring would affect the ex-ante risk-sharing choices people make.

We consider this in Hypothesis 6 in the original draft (H6, page 22) – how the formation of these risk-sharing contracts under potential peer-monitoring could be different. Specifically, in H6, we propose to test whether the count of WSCs exchanged changes when households expect peer monitoring (treatment arms T2+T3), compared to the counterfactual of no expected peer monitoring (T1). We use both aggregate- and dyad-level specifications, as shown in Equations 7 and 8 respectively. The dyad-level specification allows us to include between-households factors – such as social network, economic, and geographic distance between two households – that may affect WSC contracts between any given pair of households in the same village. Furthermore, to test for heterogeneity, we describe a specification containing interactions of each between-household characteristic with the village-level treatment dummies. This approach allows us to test for assortative matching between households in general as well as any increase in assortative matching that may arise due to potential peer monitoring.

We anticipated some degree of differential selection in contracts made under expected peer monitoring when compared to treatment arm T1. That is the reason we added treatment arm T2 in addition to T3. Since households in T2 and T3 both similarly expected peer monitoring but only T3 households observed actual peer monitoring, we can see the difference in water quality outcomes at the endline in these two arms conditional on the endogenous take-up. Specifically, this design allows us to address concerns about endogeneity in the set of people one makes contracts with when potential peer-monitoring is common knowledge.

A related point is highlighted by R2 (in the Suggestions section) that we should also compare outcomes (i.e., water quality) in T1 vs. T2 because it is important to understand if any reduction in quantity/quality of contracts due to expected peer-monitoring leads to worse outcomes. We will report the difference between estimated coefficients on T1 and T2 (Equation 3) to address this point.

(b) We provided a detailed response to R1's comment on strategic considerations in peer monitoring on page 8 of this document.

R1 has provided insightful feedback on strategic considerations in monitoring when monitors face different incentives leading to a conflict of interest (CoI). As we discuss in our detailed response to R1, two out of eight combinations in triples (household A, B and C, as shown by R1 in his/her report) may face this concern.

We have added an empirical approach to understand this further in footnote 10 on page 19. At the same time, we argue that these strategic considerations are unlikely to drive the outcomes for two reasons. First, these two cases are the only two triples that contain a CoI relation among all eight types of triples. Second, a household could be simultaneously monitored by a positive CoI monitor and a negative CoI monitor, then the contradicting forces alleviated or even canceled. Rigorously studying this question is hard in our setting as the monitor is not strictly randomly assigned. Also, the network structure adds an extra layer of barriers to identification.

3. (a) Finally, the first referee asks for a more clear articulation of the theory of change. (b) Is there a view that coupons could be implemented more broadly if successful? Or is this is seen as a test of market failure or of the value of ***formalizing informal contracts*** *in a way that would help in the formation of other mechanisms to reduce arsenic exposure? (c) One perspective would be that the coupons are less of a contract than a nudge. They will be unnecessary between households that are already connected, say through kinship, and unworkable in households that have little contact (e.g., different economic strata). So how does one measure and identify groups who are marginal in this sense? Are you powered to detect such groups?*

Response. We thank you and R1 for highlighting these points.

(a) Following your and R1's comment, we have added text to the manuscript explaining the theory of change. See our response to R1, followed by the text added to the paper explaining ThoC in Section 2.3 on page 9. The main point is that coupons (WSCs) primarily help form ex-ante commitments between households and thus potentially address the limited commitment aspect in risk-sharing.

(b) In the business-as-usual scenario, we expect that households are less likely to explicitly discuss commitments for water sharing with others **before** wells are tested. That ultimately leads to market failure in terms of incomplete risk-sharing, i.e., households who turned out to be drinking from unsafe wells may not be able to switch to safer wells, despite having safer wells nearby. Coupons provide a mechanism for actualizing ex-ante commitments.

We see our core motivation in this paper as consistent with both points you have mentioned. Primarily, coupons provide a test for market failure arising from limited commitment. Coupons enhance ex-ante commitment which has been highlighted as one of the factors behind incomplete risk-sharing in the literature. If coupon exchange does end up increasing access to low arsenic water, it would imply that the business-as-usual switching/sharing arrangement of wells is not efficient because of limited commitment. Our revised draft discusses this point in more detail.

Secondarily, if effective, coupons could be one potential way the returns to arsenic testing can be maximized in terms of improving access to safe water. That said, implementing coupons broadly and at scale, if successful, would be admittedly a relatively complicated application in this setting for the government to be involved with. However, since implementing coupons is relatively cheap, NGOs and local village bodies might find some variant of coupon distribution that is implementable.

(c) Coupons can be also seen as a nudge to get people to make a commitment to each other ex-ante, that does not happen otherwise. We obviously cannot separate the nudge aspect from the contract aspect in our design. We share the perspective that coupons would not work when households are unlikely to go along and would not be necessary when households are already connected, say, through kinship. There may still, however, be a significant mass in the middle where coupons may help increase trust and increase the cost of deviation. One way to explore the value of coupons for these marginal groups is to use social network data and coupon exchange information to assess how coupons help overcome some barriers (but not all). For instance, we can measure coupon exchanges between households that are not connected in the social network. We have proposed to analyze matching on household characteristics in the dyad-level data (as in the specification to test H6 in the manuscript). We articulate these points in our discussion on the theory of change in the revised draft.

In the process of revising this draft and responding to referees, we made minor edit changes and fixed some typos. See the summary of the main points below.

1. We will show results by baseline well ownership in H1 and H3. R1 highlights the issue of potential impact on households who do not own a well and we agree that such spillover effects could be important in this context. We have edited the text when we discuss the sample in Hypothesis 1.
2. Related to R2 and your concerns about attrition, we have added a subsection on attrition, following empirical specifications. At the end of the subsection on attrition (p. 23), we have also described our strategy to deal with missing values in control variables, that may otherwise reduce the sample used in the analysis.
3. We recognize the possibility that, especially when a less-than-50ppb well is not nearby, households may switch from higher arsenic well (e.g., 500 ppb) to a lower arsenic well still above 50ppb (e.g., 100ppb). In the previous draft, we only considered arsenic greater than 50ppb as the cutoff for determining whether a well is unsafe since this is the threshold the Bangladesh government uses for painting wells green or red after testing. In general, households may like to reduce their exposure to arsenic by switching to the lowest arsenic they can, irrespective of 50ppb cutoff (Madajewicz et al. 2007). We also provided arsenic test result cards showing the actual level of arsenic in their well water to households. So we have also included primary well arsenic level as one outcome variable. This approach helps us in preserving the sample since conditioning on baseline well arsenic at greater than 50ppb causes attrition of wells that could not be tested in the field (R2's concern about attrition).
4. Well-testing was carried out after randomization and WSC exchange intervention. The available test data shows some differences in the proportion of safe wells available across treatment groups. We, therefore, have included a control for the baseline village-level proportion of safe wells in our specifications.
5. We dropped one switching and sharing outcome variable from H1 and H3 that are similar to another outcome variable. Specifically, the intent of the question *whether household i owning an unsafe well switched to a safe well (sample restricted to households owning high-arsenic wells)* is similar to *the status relative to the* $50\mu g/L$ *standard of the primary well households use for drinking water*. We continue to keep switching and sharing to safer well for dyad regressions. However, since households can move to a lower arsenic well (but still stay above 50ppb), we will use a broader definition for switching that includes household switching to any lower arsenic well, not only to wells below 50ppb.

Response to Reviewer 1
Comments on "Informal Risk Sharing to Mitigate Local Environmental Risks" (Pre-Results Review)

Thank you very much for reading the paper and sharing your comments. We reproduce your comments in italics below, followed by our point-wise response.

This paper aims to understand ex-ante water risk-sharing arrangements that arise when well-owners learn about the level of arsenic contamination of their wells. Given the literature on incomplete risk sharing, mostly concerning income risk, the authors design an intervention that is supposed to encourage risk sharing. Before well-owners learn about the arsenic level in their wells, they are given printed Water Sharing Coupons (WSC) to be distributed amongst their networks. A WSC states that the household owning a safe well will allow the household with an unsafe well to collect water from the safe well. In the field experiment, there are three groups of villages receiving WSCs. Households in the first group only receive WSCs. Households in the other two groups are told in advance that the WSCs will be monitored, that is, a neighbor is told about the WSC between two households, and this should lead to a higher probability that the water-sharing arrangement is enforced. But monitoring will only be implemented in one of these two groups of villages where monitoring is announced. This way the authors can study if the distribution of WSC is affected by expectations of monitoring.

1. I find the paper and context very interesting. Carefully documenting what happens to the control group, without WSCs seems important to assess the degree to which it can achieve full risk sharing. What types of water-sharing arrangements arise organically? How well informed are households about the well status of their neighbors?
What happens to households that did not have a well and were accessing water from a neighbor with a safe well? Do they still have access to water?
And what about owners of unsafe wells? Are they more likely to drill another well in the property? How protected are wells from unauthorized use? Are wells enclosed / protected within the house walls, or are there in the open? Would owners of safe wells make investments to protect them?

Response: Thank you. We agree with you that it is important to document business-as-usual ways people gain access to safer wells in the control group. We discuss this in response to your comment 2 below and have added text to the revised draft. In short, based on the prior research, we expect significant water sharing to evolve organically even without our intervention. We will add a discussion on water-sharing arrangements in the control group with the relevant descriptive statistics in the final paper.

Regarding your point about information on neighbor's well status, our prior is that in general households are likely to be sufficiently informed about their nearby neighbors' wells. Wells are painted in red and green which makes it harder for a household to hide the test result, at least during months that the paint applied to a rusty cast-iron pump will remain visible. A significant proportion of wells are located outside of the main structure of the house, making it even easier for the neighbors to see the well arsenic status. This information would not be available to neighbors only when a nearby well can not be easily observed from the outside and the well-owner strategically hides the information from their neighbors. In the endline survey, we ask households about their knowledge of their neighbors' wells. We will provide descriptive stats on this in the final paper.

Your next point is about households who do not own a well. This is an important point. In general, it should be easier for non-well-owners to switch since they anyway incur the cost of traveling to a well (compared to well owners with a captive well in their house) on a regular basis, and so, the additional marginal cost of switching to a safer well is relatively low. But an indirect adverse impact on the non-well-owners' ability to access safe water is also possible if the safe well-owners start prioritizing the households who exchanged coupons with them. This will be interesting to see if the former outweighs the latter. We will report results separately for all households, well owners only, and non-well owners only, as we mention on p. 17 when we describe the sample.

Unsafe well owners are likely to be inclined to drill a new well, if the cost is not high and they expect the new well will be lower in arsenic. Our endline survey collects data on households' preference for drilling a well in the near future. We will report descriptive statistics on this in the final paper.

Your final point is about the unauthorized usage of private wells. If unauthorized use of safe wells is a concern, it is fair to expect that owner of a safe well will invest in protecting them. However, if drawing an additional few buckets of water from the well does not impose much cost on the well owner, it is unlikely that she will invest in hiding her well. Our endline survey does not cover investment in protecting wells from others in the village, so our discussion on this aspect will be limited.

2. My biggest concern is that I don't see how the distribution of WSC solves the market imperfections that lead to the presumed incomplete risk-sharing. The theory of change should be better explained.

Response: Thank you for this comment. We have added the following text to the manuscript on p.9.

Added text: "Households may start using water from nearby safe wells after they get to know that their own wells are high in arsenic. Complete risk-sharing in this context would mean that if at least one well is safe in the entire village and all other wells are unsafe, all households can take water from that well and access the safe water, ignoring any costs. A sub-group version of complete risk-sharing would mean that, within a group of n nearby households, if at least one household has a safe well, other $n-1$ households can access water from that well. However, as prior studies show, not all households using an unsafe well before testing are able or willing to switch to nearby low-arsenic wells after well tests are conducted.

In the business-as-usual scenario, a subset of households are able to switch from unsafe wells to safer wells. Usually, once wells are tested and as the status of wells becomes known, households may discuss with each other, and safe well-owners may agree on sharing water (ex-post). Another approach could be that households discuss the possibility of sharing water *before* well tests are conducted (ex-ante). However, there is a difference between making an ex-ante agreement and trying to reach an agreement ex-post. Ex-ante, both households are on roughly equal footing – each is equally likely to be lucky or unlucky regarding the arsenic shock, assuming arsenic strikes at random. So, it is in a sense an equal exchange – ignoring outside options – and thus more likely to be mutually beneficial. Contrast that with trying to strike an agreement ex-post, when there will be an inherent asymmetry, and it will be less obvious that the lucky party will be able to benefit from helping the unlucky party, so a sharing agreement may be harder to reach. In a sense, ex-ante both parties clearly have something to offer (probabilistically) while ex-post, it is not guaranteed that both parties have something to offer, so an agreement is less likely.[1]

The WSC (coupon) intervention aims to improve risk-sharing by facilitating explicit ex-ante (i.e., before well arsenic status is known) commitments between households. The explicit mutual insurance phrasing pre-written on WSCs could potentially increase the participation of households who otherwise may not prefer sharing or switching wells. WSCs provide a way for households to make commitments and also act as a nudge to encourage households to initiate discussions with others regarding such commitments. Households on the margin may want to enhance their chances of gaining access to safe water by making commitments with WSCs. In the end, WSCs may help expand the choice set of potentially accessible safe wells ex-post.[2]

Furthermore, WSCs themselves do not have any mechanism to enforce commitments. Lack of enforcement may affect the household participation at the WSC exchange as well as their compliance after well testing. So we added the treatment T2 to study this. Specifically, our two peer monitoring treatments increase the expected enforcement of these commitments and potentially increase the cost of deviation."

3. It is also unclear what incentives do monitors have to enforce WSC. Suppose a situation with second-order neighbors as depicted in panels A and B of Figure 1 below, where household A has WSCs with households B and D (given by the blue arrows). C is the second-order neighbor of household A and the monitor for the WSC between A and B (given by the dashed red arrows). Because C is a second-order neighbor, C also has an arrangement with D (given again by the blue arrow). Panel A: C and A have a safe well Panel B: C and A have an unsafe well In Panel A, we assume that C and A have a safe well while B and D have an unsafe well. In this case, C has no incentive to enforce the AB arrangement because C prefers that D gets water from A instead of C under the CD arrangement. In sum, AB arrangement will not be enforced.
< figure >
In Panel B, the situation is reversed. C has an unsafe well and wants to receive water from D who also has an arrangement with A. So C would prefer that A received water from B (than from D) and will thus enforce the AB arrangement. To conclude, it is not clear whether arrangements will be enforced. At the very least, the specifications in H3-H5 should include whether the monitor has a safe or unsafe well and its interactions with treatment.

Response Thank you for highlighting this interesting and important aspect that we overlooked. Your comment is about the **second-order** monitor having private benefits from enforcing or not enforcing the switching of the monitored household.

To illustrate, let us define the 'linking household' as the household that linked both the (second-order) monitor and the monitored household through WSCs, and these three households form a triple. As described below, high and low arsenic occurrence leads to eight

[1]Some households will still be successful in accessing low-arsenic water through ex-post agreements. For instance, while finding arsenic in one's own well is a one-time shock, the possibility of reciprocal transfers during other future shocks may also encourage households to engage in such ex-post agreements to some extent. Likewise, social and kinship ties between potential sharers and switchers may play an important role.

[2]WSCs, however, may not have any additional impact on households already sharing strong social- and kinship ties since they will share wells anyway. WSCs are also unlikely to help when there are other strong barriers to overcome (e.g., neighboring households not getting along well, no nearby well).

types of triple. And the monitor could receive private benefits related to water sharing in two out of eight scenarios.

In the first type, when only the linking household has the unsafe well, the monitor is incentivized to enforce the switching. This is because enforcing the switching between the monitored and linking households will excuse the monitor from sharing the safe well to the linking household. We will refer this type of triple as positive CoI (Conflict of Interest). In the second type, only the linking household in the triple has a safe well. If the monitor enforces the switching between the linking and monitored household, the monitor himself or herself could be excluded from using the linking household's safe well. Then the monitor is incentivized not to enforce the switching. We will refer this type of triple as negative CoI.

We argue that such strategic considerations (of monitors) are unlikely to be significant for two reasons. First, these two cases are the only two triples that contain a CoI relation among all eight types of triples. Second, a household could be simultaneously monitored by a positive CoI monitor and a negative CoI monitor, then the contradicting forces alleviated or even canceled. Rigorously studying this question is hard in our setting as the monitor is not strictly randomly assigned. Plus the complicated network structure adds an extra layer of barrier to identification. However, we have proposed one approach in this revised draft to study this, as described below.

To address the issue, we will first report the ratio of these two types among all the triples. Further, to assess the influence of CoI, we will construct a set of CoI indexes that counts the number of positive CoIs and negative CoIs for each monitored household. The PCol (NCoI) counts the number of distinct positive (negative) CoI triples that contains the household. Our data will allow us to estimate the correlation of switching outcomes with both COIs. We have added the relevant text in footnote 10 on p. 20.

Added text: Footnote 10 on p. 20 "Two scenarios of conflict of interest (CoI) deserve our attention here. Say, a 'linking household' is a household that is linked between a monitor and a monitored household via WSC exchange. In the first case, when only the linking household has the unsafe well, the monitor is incentivized to enforce the switching. This is because enforcing the switching between the monitored and linking households will excuse the monitor from sharing the safe well to the linking household (positve CoI). In the second case, only the linking household in the triple has a safe well. If the monitor enforces the switching between the linking and monitored household, the monitor himself or herself could be excluded from using the linking household's safe well. Then the monitor is incentivized not to enforce the switching (negative CoI). We argue that these two situations are unlikely to drive the outcomes for two reasons. First, these two cases are the only two triples that contain a CoI relation among all eight types of triples. Second, a household could be simultaneously monitored by a positive CoI monitor and a negative CoI monitor, then the contradicting forces alleviated or even canceled. Rigorously studying this question is hard in our setting as the monitor is not strictly randomly assigned. We can however take the following approach to assess the influence of CoIs. We will construct a set of CoI indexes that counts the number of positive CoIs and negative CoIs for each monitored household. The PCol (NCoI) counts the number of distinct positive (negative) CoI triples that contains the household. We will show estimates of the correlation of switching outcomes with both COIs."

4. In addition, because WSCs are given before the status of the wells is known, the ex-post distribution of water may be suboptimal. That is, owners of unsafe wells who ended up sharing WSC with mostly owners of unsafe wells will have less access to water compared to owners of unsafe wells who ended up sharing WSCs with owners of safe wells. Ex-post,

these inefficiencies could be corrected with other risk-sharing arrangements without WSCs.
Response We agree that WSCs mechanism may be sub-optimal for some households who may later find that their own well and the wells of all neighbors who they exchange WSCs with – both are unsafe. We provided a generous supply of WSCs to reduce constraints on the number of such ex-ante contracts one can make. But most importantly, our intervention does not tie households to only their WSC network in any way. Households are free to share and switch to other wells through other sharing arrangements, as you have correctly noted. We will be able to provide descriptive statistics on sharing-beyond-WSCs in the final paper. We have included the following text in the draft in response to your comment.

Added text to p.5 of the report: Since WSCs are exchanged before the status of the wells becomes known to households, the ex-post access to safe wells may be suboptimal for some households. That is, owners of unsafe wells who ended up only sharing WSCs with owners of unsafe wells will have less access to safe water ex-post compared to owners of unsafe wells who ended up sharing WSCs with at least one safe-well owner. In this experiment, we try to mitigate this concern by providing a relatively large number of WSCs to households (i.e., 10 WSCs per household). The WSC exchange data confirms that the number of WSCs provided is hardly binding for most households – households on average exchange less than 6 WSCs and a relatively small proportion of households exhaust all their WSCs. More importantly, exchanging WSCs does not tie households to only their WSC network. Households are free to share and switch to other wells through other business-as-usual arrangements.

5. (a) Related, it is unclear if the assumption of arsenic being distributed as an iid process is plausible. We are told that 40% of wells in a village are safe, but is there spatial correlation in arsenic? If so, neighboring wells of an unsafe well will tend to be unsafe as well. Since the cost of transporting water are high, water-sharing will tend to be local and dictated by distance, and so the scope for risk sharing will be limited if spatial correlation is high. This is data that the authors have, so they should report it.

Response

Multiple studies have shown that safe and unsafe wells are usually distributed across the village (e.g. van Geen et al. 2002). As we have now collected the arsenic test results for all the wells in the sampled villages, we were able to test the randomness of arsenic distribution through simulation. We did so by first calculating a referential statistic based on the actual geographic distribution of safe and unsafe wells using the 50ppb threshold: The distance of an unsafe well to the nearest safe well averaged for all unsafe wells in the village. We then randomly reallocated well status (unsafe or safe) within the village while keeping the proportion the same as the actual data. We repeat it a total of 200 times for each village. We calculated the referential statistic in each iteration and simulated a distribution as if arsenic is distributed randomly.

The comparison between the actual data and the simulated distribution is shown in Fig.1. The x-axis orders the subset of 122 out of 135 villages with at least one unsafe well and at least one unsafe well (1 village turned out to contain only safe wells, and 12 villages did not have a single safe well) according to the distance from each unsafe well to the nearest safe well averaged for the village. The y-axis shows the distance in meters from each unsafe well to the nearest safe well averaged over all unsafe wells in the village. In each graph, the shaded area is the simulated interval created by 200 iterations for each village. Even though

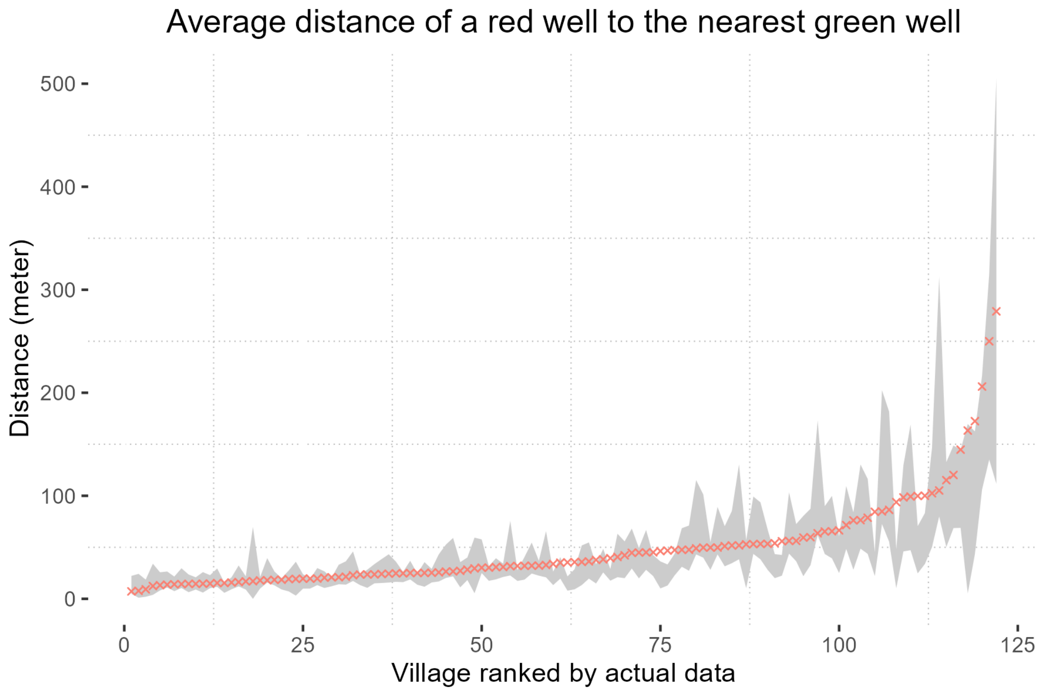


Figure 1

most of the realized data lies inside of the shade, 24 villages show extreme realized values. This means that for these many villages, there are geographic redistribution of well status that produces a considerably lower average distance from unsafe wells to the nearest safe wells, and there are redistributions that produce considerably higher average distances. By and large, however, the statistic based on the actual distribution of well status is roughly mid-way within the range produced by the simulations. This analysis indicates that the distribution of well status is spatially approximately random in most of the villages. If all the safe wells were clustered in one half of the village and the unsafe wells in the other, this would have curtailed opportunities for switching.

At the same time, it is worth emphasizing that in our study villages, almost no wells were tested before the intervention, as per the data collected. Villagers are also informed that arsenic in wells may vary at the neighbor-to-neighbor level. Hence villagers are relatively unlikely to correctly estimate the arsenic risk as well as the spatial distribution. Even when deeper wells are more likely to be safe, it's far from certain. Due to such uncertainty, it is a reasonable assumption that villagers understand the arsenic distribution as being idiosyncratic.

(b) In addition, do these wells eventually run out? Does the lifespan of a well depend on the amount of water drawn? In other words, is water from a well a rival good so that there is a limit to how many households can share a particular well?
Alternatively, are there public wells in these villages? Is investment in public wells expected? Or could the community "buy" a safe private well by compensating the owner in exchange

for public access to water?
Response On average, wells installed privately using a manual technology that reaches depths up to 300ft last 10 years. Hand-pumping a well will not significantly draw down the water table. Some wells may go dry during the dry season but for reasons unrelated to hand-pumping by the owner or neighbors. What might limit access to a neighbor's well, instead, is a desire for privacy, the extra time required to fetch water or the concern about reciprocal transfer of one type or another. To sum up, wells are not likely to run out of water due to sharing. Rather it may be the desire for privacy and other factors that may drive households' decision to share.

There can be a limited number of public wells in the village, usually with a technology that reaches the 500-1000 ft depth range. These types of wells are usually installed by the government or an NGO, rarely by a household, due to high cost. In a limited way, a government subsidy program (with 10% copay) for building public wells for a group of households exists, but in practice, public wells are frequently captured by elites, effectively making them private well. Several field studies have been conducted in Bangladesh to reduce this form of elite capture (van Geen et al. 2016). Community installing a public well or buying a safe private well for common usage would be a great solution. However, it is also logistically difficult for neighbors to coordinate on this. We would love to include analysis on this, but it is too late for us to include questions on community investment in public wells in the endline survey. We plan to study the installation of new wells in a follow-up study.

Added text to manuscript p.8: We assume that the marginal cost of sharing water is not high for households with safe wells.

(c) Finally, the outcomes of interest are limited to the extent of sharing water but one could view learning about the arsenic content of the well as the drying up of irrigation wells studied in Blakeslee et al (2020). It would be interesting to compare how this negative shock affects the outcomes considered in Blakeslee et al. (2020) as well as the value of the house, access to credit, child outcomes such as schooling and health, etc.
Response It will be certainly interesting to see the impact of this water safety shock on other outcomes. Our current RCT does not focus on it. Two notable published papers that provide quasi-experimental evidence of ingesting arsenic are Keskin et al. (2017) on infant mortality and Pitt et al. (2021) on labor market-related outcomes. Note that our context is very different from irrigation wells (as in Blackeslee et al. 2020) which are required to withdraw a much larger amount of water and require electricity to run it. Also, it is unlikely that people will migrate in response to high arsenic in water.

References

Blakeslee, David, Ram Fishman, and Veena Srinivasan. 2020. "Way Down in the Hole: Adaptation to Long-Term Water Loss in Rural India." American Economic Review, 110 (1): 200-224.

Jameel, Yusuf, M. Rajib Hassan Mozumder, Alexander van Geen, and Charles F. Harvey. "Well-Switching to Reduce Arsenic Exposure in Bangladesh: Making the Most of Inaccurate Field Kit Measurements." GeoHealth 5, no. 12 (2021).

Keskin, Pinar, Gauri Kartini Shastry, and Helen Willis. "Water quality awareness and breastfeeding: Evidence of health behavior change in Bangladesh." Review of Economics and Statistics 99, no. 2 (2017): 265-280.

Madajewicz, Malgosia, Alexander Pfaff, Alexander Van Geen, Joseph Graziano, Iftikhar Hussein, Hasina Momotaj, Roksana Sylvi, and Habibul Ahsan. "Can information alone change behavior? Response to arsenic contamination of groundwater in Bangladesh." Journal of Development Economics 84, no. 2 (2007): 731-754.

Pitt, Mark M., Mark R. Rosenzweig, and M. Nazmul Hassan. "Identifying the costs of a public health success: Arsenic well water contamination and productivity in Bangladesh." The Review of Economic Studies 88.5 (2021): 2479-2526.

Van Geen, Alexander, Kazi Matin Ahmed, E. B. Ahmed, Imtiaz Choudhury, M. R. Mozumder, B. C. Bostick, and B. J. Mailloux. "Inequitable allocation of deep community wells for reducing arsenic exposure in Bangladesh." Journal of Water, Sanitation and Hygiene for Development 6, no. 1 (2016): 142-150.

Van Geen, Alexander, Habibul Ahsan, Allan H. Horneman, Ratan K. Dhar, Yan Zheng, Iftikhhar Hussain, Kazi Matin Ahmed et al. "Promotion of well-switching to mitigate the current arsenic crisis in Bangladesh." Bulletin of the World Health Organization 80, no. 9 (2002): 732-737.

Reviewer 2: Summary: *This Stage 1 registered RCT proposal studies the role of ex-ante commitment in securing social insurance against an adverse environmental outcome, in this case, arsenic contamination of wells.*
Thank you very much for reading the paper and sharing your comments. We reproduce your comments in italics below, followed by our point-wise response.

Concerns:
1) Given that this is a proposal, I would have liked more specifics about attrition over the course of the different stages of the intervention. 1) For example, the authors report that they successfully collected WSC exchange information for 7216 wells in the 99 treated villages, but they do not report what was the share of the initially identified 10098 wells that fall in the treatment villages. Importantly, we do not know what is the number of households that corresponds to these 7216 wells. This is important because specification (1) on page 15 uses households as the observation.
Response To address the concern about attrition, we have now added the following table to the appendix that shows the count of private wells recorded in the census and at the time of well testing. The proportion of total wells is similar across all groups.

Table 1: Table: Number of wells recorded at each stage of the intervention

Treatment Arms	**Number of villages**	**Number of private wells census**	**Number of private wells arsenic test survey**
Control (C)	36	3,028	2,692
No notification or peer-monitoring (T1)	33	2,874	2,455
Only notification but no peer-monitoring (T2)	33	2,547	2,283
Notification and peer-monitoring (T3)	33	2,705	2,409
Total	**135**	**11,154**	**9,839**

2) They document a moderate amount of attrition in the phone surveys, where the sample fell from 16,054 to 14,551 and 11,933 in the two phone surveys conducted in 2020. I imagine the endline will be an in-person survey and the attrition would be lower. But I would have liked the authors to comment on the possible sources of attrition in order to evaluate attrition for the endline. Projected attrition should have been used for the power calculations.
Response We have now added a subsection on Attrition in Section 3 (discussed in detail in response to your next comment). As shown in the Table above, about 10% of private wells observed at census are not found at the time of well testing. The wells attrit naturally in our sample over time due to corrosion, malfunction, and drainage. However, the attrition rate of wells is mostly similar across all four groups.

As you have correctly noted, in-person surveys are likely to have lower attrition. The main source of attrition is the non-availability of adult household members in their houses at the time of the in-person survey. This could be due to permanent or temporary migration. In the case of very small households, usually consisting of one or two senior citizen members, attrition is also more likely due to natural deaths. Our survey team made multiple attempts to survey households when the first attempt failed. An additional unexpected source of attrition is due to well attrition either at baseline or endline. For example, if the survey team could not find the well used by a household at the baseline and/or its arsenic test outcome, that household will be dropped from the regression. One approach we take is to also show results for all households captured at the endline (irrespective of their baseline well arsenic status) as we have mentioned in our specification section.

We, unfortunately, did not use projected attrition in our power calculations. However, please note that our treatment assignment is at the village level and our surveys have covered all villages throughout. A small decrease in sample size at the endline is unlikely to matter, as long as we have all villages covered and the attrition is not correlated with the treatment assignment (see our response to your next comment).

3) In addition to lowering power, a potential concern is that attrition is differential across treatments. The authors have enough data to evaluate whether attrition in the phone surveys, and in the WSC exchange information is differential across treatments. For example, households might have been reluctant to disclose WSC exchange information if they were in the peer monitoring sub-treatment.

Response We thank you for highlighting this concern. We have now added the following text discussing how we are going to address the attrition issue on p. 23 in the revised draft.

Added text [p. 23]: We will quantify and adjust for attrition in the following way. First, we will report the attrition rate of households at the endline. For well-owner households, attrition at the endline will be estimated with respect to the in-person visits at the time of coupon distribution. For all households (including households not owning well), attrition at the endline will be estimated with respect to the in-person visits during the census survey.

Second, we will assess whether attritted and non-attritted households are significantly different using data collected on household characteristics in baseline surveys (Equation 1).

$$attrit_{iv} = \beta_0 + \beta_1 * char_{iv} + \xi_v + \epsilon_{iv} \tag{1}$$

The $attrit_{iv}$ is the binary variable that indicates the attrition status of household i in village v at the endline. $Char_{iv}$ is the list of household characteristics we included as controls in our main specifications. We will cluster the standard errors at the village level. Note that in-person visits and data collection during coupon distribution were conducted only for well owners identified in the census survey. So, for households who didn't own a well, we will use (1) the household's primary education completion rate, (2) the child ratio, and (3) health risk preferences, as collected in the baseline census survey.

Third, to evaluate whether the differential attrition potentially confounds our results, we test if the attrition rates are significantly different across treatment groups through the equation:

$$attrit_{iv} = \beta_0 + \beta_1 * T1_v + \beta_2 * T2_v + \beta_3 * T3_v + X_{iv}\gamma + \delta A_v + \eta_u + \epsilon_i v \tag{2}$$

T is the indicator for the treatment group, and likewise, we will cluster the standard errors at the village level.

Finally, if a concerning level of attrition is detected, we will apply Lee bounds to correct the attrition bias.

Missing values: In addition to attrition, missing values in the control variables collected during baseline surveys may also affect the sample used in the final analysis. To address this issue, we will present estimates after substituting the missing observations with the non-missing sample means and add binary indicators for missing values to the regression.

Suggestions:

4) I really liked that the randomization design will allow the authors to test whether peer *monitoring affects their ex-ante risk-sharing choices. The outcome they propose to measure is the aggregate number of WSC. However, another relevant outcome is the quality of the water ex-post driven solely by selection (i.e. comparing treatments T1 and T2). This is relevant because a possible outcome of peer monitoring is both that agreements are more likely to be enforced but also that there are less agreements to begin with. In order to evaluate whether the reduced number of agreements is enough to counteract the effect on enforcement, it would be important to know whether the reduced number of agreements had any effect in water quality.*

Response The selection effect on ex-post water quality is indeed important. We will report the differences between T1 and T2 coefficients, as we discuss in H3 on p. 19-20.

Beyond the aggregate effect effects due to the selection, the two specifications from H6 help us to understand to what extent the selection is driven by the awareness of peer-monitoring, and whether the selection can be predicted by certain characteristics.

5) An important question that emerges from this intervention is whether there are any downsides from sharing a water well with a neighboring family in order to honor a pre-commitment. At the heart of this question is whether there are any quality/quantity trade-offs. An important question for the endline would be the quantity of water individuals are consuming. It is likely to be measured with noise, but I think this is a question that will naturally come up in this context. They also have health measures, but I am not sure they'll be able to pick up health effects.

Response This is indeed an interesting and important question. It is likely that households are only able to substitute a part of their drinking water needs from safer sources and in some extreme cases, they may even limit the amount of water they drink. It's plausible.

In this study, we didn't collect water consumption data - before or after the intervention. Another study published by coauthor van Geen documents about 3 L/day consumption but the variation is large - some of it due to measurement error (Fig.S2 in Beene et al. 2022).

On health effects, you are correct that our sample of 135 villages won't allow us to capture health effects. We haven't collected detailed data on health outcomes from our respondents. We do however have collected data on mortality and its causes. We can potentially attempt to see the difference in mortality and its causes across primary wells with different arsenic levels, though given the sample size issues we won't be able to make strong claims here.

References

Beene, Daniel, Philip Collender, Andres Cardenas, Charles Harvey, Linden Huhmann, Yan Lin, Johnnye Lewis et al. "A mass-balance approach to evaluate arsenic intake and excretion in different populations." Environment International 166 (2022): 107371.

Manuscript File Click here to view linked References

Registered Report (Pre-results Review)

Informal Risk Sharing to Mitigate Local Environmental Risks*

Prabhat Barnwal† and Alexander van Geen‡ and Yiqian Wang§

First draft: September 11, 2022
Revised: April 20, 2023

Pre-registration: Barnwal, Prabhat, Alexander van Geen and Yiqian Wang. 2021. "Informal Risk Sharing to Mitigate Local Environmental Risks." AEA RCT Registry. August 2021. https://www.socialscienceregistry.org/trials/5071

Keywords: Informal risk sharing, commitment, peer-monitoring, social network, arsenic
JEL Codes: D16, D81, Q54

*We thank Chris Ahlin, Arun Chandrasekhar, Ahasan Habib, Mushfiq Mobarak, Hanzhe Zhang, and participants at Pre-Analysis Plan Mini-Conference 2019, Northwestern University, for useful feedback. We thank IPA Bangladesh and NGO Forum team for rigorously implementing the fieldwork. This work could not have been possible without the excellent research support provided by Mir Raihan, Tonmoy Kumer Saha, Sultan Mahmud, Shabib Raihan, Nishat Juy and the rest of the NGO Forum and IPA Bangladesh field team. Providence Weatherwax at MSU provided excellent help with the data. All errors are ours.

†Department of Economics, Michigan State University. 486 W. Circle Drive 110 Marshall-Adams Hall, East Lansing, MI 48824-1038 (email: prabhat@msu.edu.)

‡Lamont-Doherty Earth Observatory, Columbia University, Palisades, NY 10964 (e-mail: avangeen@ldeo.columbia.edu.)

§Department of Economics, Michigan State University. 486 W. Circle Drive 110 Marshall-Adams Hall, East Lansing, MI 48824-1038 (email: wangy160@msu.edu.)

Abstract

Informal risk sharing between households can help smooth consumption during shocks. We study how *ex-ante commitments* to mutually insure each other's consumption shape risk-sharing in the context of an environmental disaster – arsenic poisoning of groundwater wells in Bangladesh. Arsenic contamination of wells occurs naturally, varies at short distances, and is hard to predict without testing. Once households find out their well is contaminated, they can potentially switch to a nearby well that is lower in arsenic. In a field experiment conducted across 135 village communities in rural Bangladesh, we evaluate the impact of facilitating ex-ante commitments between household pairs to share safe water. These commitments – formed before households get to know their own well's arsenic status – are equivalent to a contract to share water. We implemented this intervention right before the roll-out of a nationwide program to test wells for arsenic. In addition, a two-part intervention informs households about subsequent *peer-monitoring* of these commitments before testing and implements the peer-monitoring after testing. This design allows us to study how risk-sharing networks evolve under different enforcement environments.

Timeline. Multiple rounds of surveys to collect baseline data extended over 2020-21 due to COVID-related delays. The intervention facilitating ex-ante commitments between households was carried out in March-August 2021. This was followed by well testing for arsenic in December 2021 - July 2022. Once well testing was completed in our sample villages, we implemented the peer-monitoring intervention in March-July 2022. The endline survey to collect data on our main outcome, i.e., well switching and water sharing, starts in September 2022 and ends in October 2022. We expect to complete the analysis for this study by July 2023.

1 Introduction

Informal risk-sharing within social networks is observed in low-income countries where public goods, institutional protection, and formal insurance markets are often inadequate. Households secure consumption by helping each other during income, health, or environmental shocks. Classic models predict, for instance, that pooling risks by a whole community would lead to a more efficient level of risk-sharing than pooling by small subgroups. However, the degree to which the risk is being shared and mitigated informally in practice is usually lower than the most efficient level of risk-sharing (Townsend, 1994; Udry, 1994; Fafchamps and Lund, 2003). This observation motivates questions on how informal risk-sharing evolves and whether it can be expanded through simple interventions.[1]

Our primary motivation is understanding how informal risk-sharing bonds form and are sustained in a community. First, we study the role of commitments in increasing informal risk sharing and eventual risk mitigation. Commitments here refer to explicit agreements (commitments) households make to share resources before the realization of idiosyncratic environmental risk. On the one hand, commitments may increase the likelihood of co-operating to reduce risks. On the other hand, people may be reluctant to make explicit commitments when they are concerned that explicit commitments may increase reprisals for deviations. Second, we explore how such commitments are sustained in communities. Specifically, we evaluate the role of peer monitoring within communities in following up on ex-ante commitments and mitigating risk.

In our setting of rural Bangladesh, households consuming groundwater face the idiosyncratic risk of arsenic poisoning by drinking water from their household well. A considerable proportion of these wells are contaminated with naturally occurring arsenic (Fig. 1). Chronic exposure to arsenic by drinking from contaminated well water damages human health early (spontaneous abortions, still-births, increased infant mortality, diminished intellectual function) and later in life (cardiovascular disease, cancers of the lung and liver), reduces labor productivity and impedes the accumulation of human capital (Carson et al., 2011; Abdul et al., 2015; Pitt et al., 2021). However, given that arsenic is a colorless and odorless contaminant, a vast majority of households in our sample did not know their well's arsenic status before our intervention. Therefore, testing a well for arsenic and sharing the result with users of the well naturally creates consumption shocks in these households.

One way for households to reduce arsenic exposure and mitigate health risks is to 'switch' to nearby low-arsenic wells (van Geen et al., 2002). Arsenic contamination in groundwater aquifers varies at short distances, even within a village. This means an affected household can potentially use safe water from nearby wells. Prior studies indeed show that households

[1] Limited commitment and hidden income are often cited in the economics theory literature as critical factors that restrain the informal risk-sharing (Coate and Ravaillon, 1993; Kocherlakota, 1996; Ligon, 1998; Ligon, Thomas, and Worrall, 2002)

switch to nearby wells in response to arsenic testing of their wells. However, the switching level varies widely from village to village – from 20% to 70% (Madajewicz et al., 2007; Barnwal et al., 2017, Tarozzi et al., 2021), and apparently not only because of differences in the distance to a safe well (Pfaff et al., 2017). This suggests other barriers to reducing arsenic exposure and therefore, insufficient risk-sharing.

We implement two interventions through a randomized controlled trial in 135 rural communities in Bangladesh (Fig. 2). By 'community', we refer here to a whole village or a village subdivision known as a para when the village is too large. In the control group of 36 communities, we sent generic voice call and text messages regarding the arsenic contamination, tested wells for arsenic, and informed households of the result. We expect some informal risk-sharing to emerge naturally from implicit commitments through existing social ties. Specifically, some safe-well-owning households may voluntarily share their well water with neighbors who own poisonous wells. The sharing and switching level in the control group communities provides the business-as-usual counterfactual scenario.

In contrast to implicit commitments, our first intervention is to facilitate the formation of ex-ante mutual commitments between households. This intervention is *ex-ante* because the participating households commit to sharing risk *before* arsenic tests are conducted for their wells. We implement the intervention using a water-sharing commitment contract called Water Sharing Coupons (WSCs). Pre-printed WSCs state that when two well-owning households exchange WSCs with each other before testing, and one of them turns out to have high arsenic well, the household owning the safe well will allow the household with the unsafe well to collect water from the safe well for drinking and cooking. We distribute these WSCs among well-owning households in 99 communities and ask households to exchange them with neighbors of their choice.

In a subset of 33 village communities (out of 99 communities with WSCs), we implement our second intervention by enhancing peer monitoring. This entails making WSC exchange information public within the community but in a limited way only. We ask for households' consent for this future revelation in advance. To further isolate the effect of selection into commitment contracts due to the consent for peer monitoring, we ask for consent in an additional 33 communities (out of 99 communities with WSCs) but do not actually implement the peer-monitoring in these communities. Fig. 3 shows the experiment design.

We timed our intervention before Arsenic Risk Reduction Project (ARRP), an ongoing blanket testing of 8 million wells for arsenic across the country administered by the Department of Public Health Engineering of Bangladesh. The arsenic in South Asian groundwater is of natural origin (Fendorf et al., 2010). Although concentrations of arsenic are generally stable over time, concentrations can vary even at short neighbor-to-neighbor distances (van Geen et al. 2002). These tests are not regularly available to households. The last blanket testing of wells for arsenic was conducted by the government about two decades ago.

Due to replacement and new installations, almost all of the wells in rural Bangladesh were untested before ARRP. Therefore, the testing program naturally created a welfare shock to households by revealing the tubewell's arsenic status, which they typically did not know. During the testing, a team of NGO workers tested each well using a field kit for arsenic in water (Pfaff et al., 2017). These tests provide information on arsenic in three ways – verbal communication to the well owner, painting the well spout red (more than $50\mu g/L$ arsenic) or green (less than or equal to $50\mu g/L$ – the Bangladesh national standard for arsenic in drinking water), and a printed result card (Fig. 6).

We collect data in multiple baseline surveys. We first map all wells in the study community to the household that owns them at baseline. We also collect social network information from all households in our sample. Our study communities have about 120 households per community on average. Out of 16,054 households, 11,975 reported complete or partial ownership of at least one tubewell. We collect households responses to questions measuring altruism, reciprocity, and several other critical behavioral traits using the toolkit introduced by Falk et al. (2016, 2018). We also collect data on norms about well-sharing in the community. In the endline survey, we collect data on the household's current water source to construct a binary variable indicating whether the household uses a high or low arsenic well. In a sample of households, we also conduct audit testing by asking for a glass of water and testing that water.

Since WSCs are exchanged before the status of the wells is known to households, the ex-post access to safe wells may be suboptimal for some households. That is, owners of unsafe wells who ended up sharing WSCs with owners of unsafe wells will have less access to safe water ex-post compared to owners of unsafe wells who ended up sharing WSCs with owners of safe wells. In this experiment, we try to mitigate this concern by providing a relatively large number of WSCs to households (i.e., 10 WSCs per household). The WSC exchange data confirms that the number of WSCs provided is hardly binding for most households – households on average exchange less than 6 WSCs and a relatively small proportion of households exhaust all their WSCs. More importantly, exchanging WSCs does not tie households to only their WSC network. Households are free to share and switch to other wells through other business-as-usual sharing arrangements.

We evaluate our intervention on several outcomes. First, we evaluate the reduction in exposure from switching and sharing wells in the different treatment arms. Second, we evaluate the formation of mutual risk-sharing agreements and we measure the impact of these agreements on well switching. Specifically, we estimate the impact of consent for peer monitoring on the likelihood of the formation of agreements between two households in the same community.

Our study provides experimental evidence on a relatively less-studied topic – how to encourage informal risk-sharing among households facing idiosyncratic risks.[2] Our contribution here is to evaluate whether facilitation of the ex-ante commitments can lead to more informal risk-sharing. Specifically, we contribute to three strands of literature.

First, we contribute to the understanding of limited commitment in informal risk-sharing within communities. In previous studies, commitment devices and contracts help improve savings (Ashraf et al., 2006), quit addictive products (Gine et al., 2010), and increase labor productivity (Kaur et al., 2015). At the same time, other papers (e.g., Kinnan 2022) reject the role of limited commitment for low risk-sharing between households. We extend the knowledge about whether simple commitment contracts can overcome the commitment problems that hinder informal risk-sharing.

Second, we provide empirical evidence on how peer monitoring shapes risk-sharing between individuals in the absence of institutions. Classic studies emphasized peer monitoring's crucial role in the (micro) credit market (Stiglitz, 1990; Hermes et al., 2005). More recently, the impact of peer monitoring on individual financial decisions has been studied (Breza and Chandrasekhar, 2019). We contribute to this literature by testing the effectiveness of peer monitoring on informal risk-sharing and studying the channels through which peer monitoring operates.

Third, we contribute to the risk-sharing literature in two separate ways. One, our dyad-level data allows us to experimentally trace how risk-sharing networks formed under a manipulated enforcing environment. The two most related papers provide empirical evidence from framed lab-in-field experiments (Barr and Genicot, 2008; Attanasio et al., 2012). Two, we add to the literature on assessing the role of social networks and preferences in forming mutual insurance (Fafchamps and Gubert, 2007; Attanasio et al., 2012).

2 Research Design

We implement an intervention to answer the proposed research questions and evaluate them using a clustered-randomization trial in rural Bangladesh. Our first treatment is on increasing commitment before wells are tested using WSCs. A successive second treatment aims to enhance peer monitoring by making commitment information public in a limited way.

2.1 Sample selection

The experiment takes place in a central region of Bangladesh affected by arsenic. We selected a subset of villages with an intermediate proportion of wells contaminated by arsenic relative to the national standard of $50\mu g/L$ ((or parts per billion - ppb) based on blanket testing

[2]Feigenberg et al. (2013) provide experimental evidence on economic returns of increasing social interaction. Meghir et al. (2022)**meghir2020migration** study the effect of migration subsidies on informal risk-sharing within villages using a field experiment.

under Bangladesh Arsenic Mitigation and Water Supply Program (BAMWSP) conducted in 2000-05 (Fig. 1). Most households no longer knew the status of their well by the time of the baseline survey conducted in January 2020 as 5-10% of these wells are replaced each year (van Geen et al ., 2014). Whereas BAMWSP testing and various interventions reduced arsenic exposure throughout the country, there are reasons to believe that level of exposure to arsenic was still significant in the study region (Jamil et al., 2019).

We selected five contiguous Upazilas (sub-districts) based on BAMWSP data and geological similarity to our previous studies conducted in Araihazar Upazila. We first shortlisted villages by excluding villages with less than 20% of wells elevated in arsenic. We then collected central GPS coordinates for these villages to determine in Google Earth which villages were not too large or too small for blanket testing. We eventually narrowed down the sample to 135 villages with 25-95% of wells high in arsenic according to BAMWSP (Fig.2). Some of the villages in this sample were still too large to survey entirely, and we conducted additional field work to delineate individual paras, which are informally demarcated geographical partitions of a village. We then randomly chose one para per village in the case of these large villages. Our final sample of 135 village communities includes 103 full villages and 32 village paras.

The first survey entailed a complete census of 16,054 households, defined as immediate relatives sharing the kitchen, and mapping and tagging a total of 11,154 wells with a unique ID embossed on a stainless steel tag (Fig. 4). Overall, we identified a subset of 11,975 households who fully or partly owned at least one well in the survey. While most households (55%) own exactly one well, about 4% households, own more than one. Table 7 and Table 8 summarize the household demographics, primary well characteristics, and household assets.

Well ownership can be complicated sometimes. When multiple families claim one specific well as their own (e.g., people who contributed towards digging the well, or, siblings who inherited the well), we determine the main ownership after discussing it with all stakeholders.

A significant number of households use more than one well. In the census, we asked households to identify the well they primarily use for drinking water. We call them households' "primary well". We also asked about other wells households use for drinking and called them secondary wells.

A well is defined as safe if the test kit shows an arsenic concentration of $50\mu g/L$ or less. For all higher values, the well is defined as unsafe.

2.2 Interventions

Commitments to share water

During our first intervention, we asked households to exchange WSCs as a mutual commitment contract. The exchange of WSC between two households indicates the agreement that households will share safe wells water, regardless of the outcome of the arsenic test. We assume that the marginal cost of sharing water is not high for households with safe wells. Thus WSC can possibly extend the risk mitigation for reasons discussed in the following subsection. In most cases, we distributed 10 WSCs to 6,979 well-owning households and asked them to exchange them with each other forming a mutual insurance pair.

A WSC is illustrated in Fig. 5. It states that when two well owners exchange their coupons, the safe well owner is expected to share the well water with the unsafe well owner after the testing for arsenic. Following the statement are four lines. The first line is printed with the well owner's name. The second line is printed with the coupon owner's father's or husband's name. The third line is printed with the well tag number, and the fourth line dates the distribution of the coupon.

We provided 10 WSCs to each household owning a single well. Ideally, households should be allowed to exchange as many coupons as they like, but this would have become logistically difficult. Our pilot work and calculations considering the arsenic risk indicated that ten WSCs per well would adequately insure households in most areas against arsenic risks. In practice, since several types of well-ownership were identified in the baseline survey, we slightly modified the number of coupons distributed according to self-reported well ownership. For those wells that claimed sole ownership, the owners received ten coupons for them to share. For the jointly-owned wells, each owner received five coupons. The number of coupons each well-owned household receives is based on the number of wells claimed by the household. For example, if a household claimed primary ownership of one well and joint ownership of another, then the household would receive 15 coupons.

The field team gave households three days to exchange coupons with other households in their villages. On the first day, the field agents distributed WSCs and encouraged well-owning households to exchange them with other well-owning households. Three days later, field agents returned to the village and recorded the well and household IDs from the coupons the households received. Field agents checked for consistency by determining if the number of received and remaining coupons of the household stilled summed to ten for each well (or five for joint ownership). Households kept all exchanged or not-exchanged WSCs as a record of the agreements made.

Peer-Monitoring

The peer-monitoring intervention sends information about WSC exchanges for a given household to at most two other households ('monitors') in the same village. We hypothesize that these monitors may help increase the extent to which previous commitments are respected. The monitors are randomly chosen from the first- and second-order neighbors in the WSC network of a given household.[3]

Households in the 66 study villages that were treated with peer-monitoring or peer-monitoring notification were invited to participate before the WSC distribution. Table 3 describes the contents of consent received by households living in different treatment arms.

The monitoring information was sent by text messages. Specifically, all well-owning households in the 135 study villages received a generic automated voice phone message describing the impact of arsenic on human health. The 33 villages assigned to the peer-monitoring notification village do not receive any additional messages. Meanwhile, in the 33 villages assigned to the peer-monitoring treatment group, households received two additional types of customized text messages: The first type of message we call a "monitor message" was sent to randomly selected monitors of a named household. The message includes the total number of WSC exchanges a named household head made along with the names of at most two other household heads who exchanged WSCs with the named household.[4]

The second type of message we called a "receipt message" was sent to the named household itself. The receipt message informed these named households of who their monitors were and what information was sent to these monitors.

Table 4 describes the text messages and the voice call we sent to households. Whereas each household only received one receipt message, it could simultaneously be a monitor to several other households. For example, if household A is the center node of a star-style WSC network, where all the other households in the network only exchange with household A, household A becomes the monitor of all these other households.

2.3 Theory of Change

Households may start using water from nearby safe wells after they get to know that their own wells are high in arsenic. Complete risk-sharing in this context would mean that if at least one well is safe in the entire village and all other wells are unsafe, all households can take water from that well and access the safe water, ignoring any costs. A sub-group version of complete risk-sharing would mean that, within a group of n nearby households, if at least one household has a safe well, other $n-1$ households can access water from that well. However, as prior studies show, not all households using an unsafe well before testing

[3]WSC networks are networks generated by exchanging WSCs by households. A given household A's Nth-order-(WSC)-neighbors are all the others in the WSC network connected to household A with a length-N shortest path. For example, the first-order neighbors are the households that exchanged WSCs with household A. The second-order neighbors are the households exchanging WSCs with household A's first-order neighbors but not household A itself. If household A exchanged at least one WSCs and had more than one second-order neighbor, one of the first-order neighbors and one of the second-order neighbors were randomly selected as household A's monitor. If household A has at least one first-order neighbor and no second-order neighbor, one of its first-order neighbors was randomly selected as the monitor.

[4]The message would include two names if the household exchanged WSCs with at least two other households. If the named household only exchanged a WSC with one other household, the message includes one name only.

are able or willing to switch to nearby low-arsenic wells after well tests are conducted.

In the business-as-usual scenario, a subset of households are able to switch from unsafe wells to safer wells. Usually, once wells are tested and as the status of wells becomes known, households may discuss with each other, and safe well-owners may agree on sharing water (ex-post). Another approach could be that households discuss the possibility of sharing water *before* well tests are conducted (ex-ante). However, there is a difference between making an ex-ante agreement and trying to reach an agreement ex-post. Ex-ante, both households are on roughly equal footing – each is equally likely to be lucky or unlucky regarding the arsenic shock, assuming arsenic strikes at random. So, it is in a sense an equal exchange – ignoring outside options – and thus more likely to be mutually beneficial. Contrast that with trying to strike an agreement ex-post, when there will be an inherent asymmetry, and it will be less obvious that the lucky party will be able to benefit from helping the unlucky party, so a sharing agreement may be harder to reach. In a sense, ex-ante both parties clearly have something to offer (probabilistically) while ex-post, it is not guaranteed that both parties have something to offer, so an agreement is less likely.[5]

The WSC (coupon) intervention aims to improve risk-sharing by facilitating explicit ex-ante (i.e., before well arsenic status is known) commitments between households. The explicit mutual insurance phrasing pre-written on WSCs could potentially increase the participation of households who otherwise may not prefer sharing or switching wells. WSCs provide a way for households to make commitments and also act as a nudge to encourage households to initiate discussions with others regarding such commitments. Households on the margin may want to enhance their chances of gaining access to safe water by making commitments with WSCs. In the end, WSCs may help expand the choice set of potentially accessible safe wells ex-post.[6]

Furthermore, WSCs themselves do not have any mechanism to enforce commitments. Lack of enforcement may affect the household participation at the WSC exchange as well as their compliance after well testing. So we added the treatment T2 to study this. Specifically, our two peer monitoring treatments increase the expected enforcement of these commitments and potentially increase the cost of deviation.

[5]Some households will still be successful in accessing low-arsenic water through ex-post agreements. For instance, while finding arsenic in one's own well is a one-time shock, the possibility of reciprocal transfers during other future shocks may also encourage households to engage in such ex-post agreements to some extent. Likewise, social and kinship ties between potential sharers and switchers may play an important role.

[6]WSCs, however, may not have any additional impact on households already sharing strong social- and kinship ties since they will share wells anyway. WSCs are also unlikely to help when there are other strong barriers to overcome (e.g., neighboring households not getting along well, no nearby well).

2.4 Experimental Design

Our interventions follow a clustered-randomization design (Fig. 3 and Table 2). We randomly assigned 135 villages to the following three treatment arms and one control arm. In all 135 villages, the study team informed people about the adverse health impact of arsenic and recommended households switch to low arsenic wells nearby if the well test shows high arsenic in their primary well. All household wells in 135 villages were tested for arsenic.

Control (C): 36 villages were randomly assigned to the control group, where we did not implement any intervention. We will record well switching and sharing at the same time as in treatment villages to measure the magnitude of business-as-usual risk-sharing.

WSCs (T): 99 village villages were randomly selected to the WSC group. We distributed WSCs to well-owning households and asked them to exchange them with other households as a form of commitment to share wells.

From these 99 villages, we further assigned villages to the second set of treatment groups.

WSCs only (T1): In 33 WSC villages, no other treatment was provided.

WSCs + Peer Monitoring Notification (T2): In this group of 33 WSC villages, we notified well-owing households that a peer-monitoring program may be implemented in the future that would make their WSCs exchanges public in a limited way. This group only received the notification, but not the actual peer monitoring treatment.

WSCs + Peer Monitoring Notification+Peer Monitoring (T3): In the remaining 33 WSC villages, we first notified well-owning households about the peer-monitoring program and then implemented it once wells were tested.

2.5 Survey

We collected data in multiple rounds of surveys in 2020-22.

Household census and well listing

Between January 15 and February 3, 2020, enumerators were able to reach 16,054 households in 135 villages in a door-to-door campaign, accounting for 92% of the total of 17,538 households identified in these villages. Following consent, the enumerator electronically recorded the name, age, gender, and relationship of each household member, as well as the GPS coordinates of each house. Up to two mobile phone numbers from all consenting households were also recorded. Most households were subsequently recontacted using one of these numbers or in some cases, alternative numbers provided by their neighbors.

During the same household and well survey, enumerators attached one metal well tag to each well owned or partially by that household. Each stainless steel well tag was embossed with a unique number, and that number was recorded by the enumerator, along with a photo of the well and another of the mounted tag. After mounting a tag, the enumerator asked questions regarding well ownership. 11,154 wells were recorded to be privately owned. For the 10,098 wells privately owned by a single household, the well tag number was linked to that household. For the 1,056 wells jointly owned by multiple households, the well tag number was linked to these households. A subset of 4,077 households did not report owning a well and used the well owned by a neighbor. In general, we collected data for a total of 11,459 wells with 11,154 wells privately-owned and 305 public wells[7].

Well naturally wear out. Among the 11,154 privately-owned well, we successfully tracked the testing results of 9,839 of them, with the missing ones being abandoned due to malfunction, drainage, and multiple other factors. Table A1 summarizes the number of wells we recorded by the census and endline survey in each treatment arms.

Baseline Surveys

We conducted two subsequent household surveys over the phone to comply with the government lockdown order and out of concern for the safety of enumerators and participants. We recorded a detailed set of household-level demographics, including deaths, illness, migration, health, asset ownership, well information, risk preferences, and social contacts within the community. As summarized in Table 6, we also mapped the social networks of each community based on the social contacts we collected from each household. These questions are slightly modified from the social network questionnaire used by Banerjee et al. (2013), from which we record that on average, each household has 4.1 close social contacts. Finally, we addressed a series of COVID-19-related questions, including local COVID exposure, knowledge of infection, adoption of preventive measures, and economic impact to 20% of sampled households in the first round and 28% of sampled households in the second round.

During the first phone survey conducted from May 8 to June 7, 2020, 14,551 (91%) of the households surveyed in person in January 2020 could be contacted and consented to respond. During the second phone survey conducted between October 27 and December 14, 2020, 11,933 (74%) households responded and consented. The repeated household census and COVID-related questions led us to show that there was no detectable increase in COVID-related mortality in our 135 study communities in 2020. However, there was a significant economic impact (Barnwal et al., 2021).

WSCs distribution and information recording

The first round of WSC distributions and recording occurred in 44 villages between March 1 and April 3, 2020, and the second round in another 52 village communities between May 25 and June 26, 2021. The third round in the last 3 village communities took place in between August 21 and August 28, 2021. Enumerators entered the targeted villages with

[7]The public wells are installed by the government or NGOs in the study villages.

well maps drawn using the GPS data collected from the listing survey. Upon reaching a well, the enumerator found its owner(s) and invited them to the coupon exchange program. Three days after the enumerator's visit for coupon distribution, the enumerator returned to these villages to record the coupon exchange information and ask a few more questions regarding coupon sharing. In 94 out of 135 villages, water samples were collected from a random subset of 400 wells for laboratory testing and comparison with the kit results after recording the coupon exchange information.

In the coupon distribution, we first recorded how participants perceived the extent of arsenic contamination within the neighborhood. Second, we asked households for information about their well, including its respect to arsenic, well depth, age, and location (locked inside the living place or exposed to the public). Third, we elicited households' social preferences, including altruism, trust, reciprocity, and risk preference, using the survey toolkit developed by Falk et al. (2016, 2018). We then elicited households' willingness to switch to and share water sources through willingness-to-pay and privacy concern questions. In the end, we elicited households' perceptions of social norms regarding sharing and compliance to publicly accepted actions.

In the coupon information recording survey, we first recorded each participating household's coupon exchange information. Photos of exchanged coupons were taken for the record and back-checking if needed. We then asked participants to list the primary reason for each coupon exchange, the chance of the exchanged well contains arsenic, and to what extent the household knows its risk-sharing partner's exchanges. Further, we asked norm questions regarding complying or reneging on contracts. We collected the WSC exchange information from 7,216 wells in the 99 treated villages. As shown in Fig. 7, on average, each household exchanged 5.56 coupons, with 9.9% of households did not exchange while 17.3% of households exchanged all 10 coupons.

New wells continue to be installed in these villages for greater convenience or to replace old ones that no longer function (van Geen et al., 2014). Enumerators also encountered undocumented wells that were missed from the listing survey. We managed both these situations by attaching new well tags to the handpumps. We repeated the asset questions to a small but random set of households to check the quality of the phone call surveys.

Well testing

All functioning wells in the 135 study village communities were tested with a colorimetric field kit for arsenic. The result was reported orally to the household on the spot and by leaving a card with the household showing both the categorical (safe/unsafe) outcome and the arsenic concentration that was measured (Fig. 6).

Due to long delays in government testing in a subset of our study villages, our implementing partner NGO Forum carried out the well test. In the remaining villages, our enumerators shadowed the government's well-testing team to collect the data. This also means that two different types of kits were used for testing. In 95 villages, the established ITS EconoQuick kit (George et al., 2012) was used by field staff trained and hired by NGO Forum. In the remaining 40 villages, government testers were shadowed and the results were recorded electronically by NGO Forum staff to be able to access the data as in the other villages. In these 40 villages, the testing was conducted with the Macherey-Nagel QuantoFix kit, which is more recent but relies on the same Gutzeit reaction by staff hired by the local government. The EconoQuick kit testing comes with a visual calibration scale discrete bins at 0, 10, 25, 50, 100, 200, 300, 500, and 1000 $\mu g/L$. The calibration scale of the QuantoFix kit is expressed in milligrams per liter (or parts per million - ppm) in bins similar to those of the EconoQuick kit but with one additional bin at the low end of the spectrum: 0.000, 0.005, 0.010, 0.025, 0.050, 0.100, 0.250, 0.500 mg/L.

Well-testing was conducted in 108 villages from December 18, 2021 to February 15, 2022. In the remaining 27 villages, testing was delayed and extended fitfully between May 22, 2022 and July 3, 2022 because of a shortage in the government's supply of kits.

In all study villages, our well-testing team wrote the kit reading on the results card along with a reminder that the national standard is 50 $\mu g/L$ (Fig. 6). In addition, the spout of each tested hand pump was painted green or red, depending on whether the test result was $<= 50$ or $>= 100 \mu g/L$, respectively. We expect in a subset of 95 villages, our testing campaign will be followed by the government's testing and potential repainting of the spout of the well according to their new test result.

The number tested was somewhat lower than the original number wells surveyed because of disrepair or because wells could not be found again. As summarized in Table 5, we find that about 40% of the 9,839 tested wells contain less than $50\mu g/L$ arsenic, which is the Bangladesh national standard for drinking water. For consistency with government policy, we use this definition for safe household wells. The World Health Organization guideline for arsenic is $10\mu g/L$ which provides one more threshold to define a safe well. Nevertheless, we expect that households will consider moving down to lower arsenic water sources irrespective of these cutoffs.

Since the randomization was carried out before well testing, some imbalance across villages is likely. Specifically, the proportion of safe wells is not fully balanced across our treatment groups.

Endline Survey

After the well-testing and text message interventions are completed, the experiment will conclude with the endline survey. The main goal of this survey is to elicit the switching and sharing status for each household, well-owning households, and households that do not own a well. In addition to questions about well switching and sharing, we will ask each household the reasons to switch or share and the frequency of switching or sharing. We will also elicit each household's perceptions of the local severity of the arsenic problem. These perceptions include some direct questions such as the average arsenic status in the community and the arsenic status of their coupon cosigners, and some indirect questions such as their willingness to pay for new wells and the local depth below which a new well would have to be installed to likely be low in arsenic [8].

Following the switching and sharing questions, we will repeat the social preferences and norms elicitation conducted during the coupon distribution and information collection phase. Further, we will test each participating household's knowledge of the extent to which other households switch or share wells with increasing network distance to document information dissemination within different treatment groups.

2.6 Randomization and power calculation

Randomization

The treatment arms are randomized through a Stata 16 built-in command, *splitsample*, which randomly allocates a treatment to each community. We stratified villages within upazillas (sub-districts).

In Table 9 and Table 10, we summarize the balance across different treatment arms. In the first 12 columns, we show the number of observations, mean, and standard deviation of relevant household characteristics of four treatment arms. Then, we compare the balances across different treatment arms. These comparisons include the balance between each treated village with the control, the balance between treated villages, and the balance between any treated villages with the control. The vast majority of the comparisons show small differences, suggesting that the interventions are randomly distributed in the sample.

Power calculation

The ratio of unsafe owners eventually switching to safe wells, or the switching rate is the outcome of interest. This ratio not only depends on the actual ratio of safe wells in the community but also on the number of coupons the villagers exchange with each other. We predict the switching rate based on a stylized model by fixing the number of coupons villagers on average exchange and how likely they eventually switch when there are any available safe wells. We show that the switching rate will be high enough for our sample to achieve a conventional level of power ($\beta = 0.8$).

We assume every community has the same average number of 80 well-owning households. Every household has one well and exchanges the same number of WSCs. The probability

[8] Due to geological factors, there is within a given village often a well-defined depth below which groundwater is likely to be low in arsenic. This depth can vary considerably between neighboring villages, however (Gelman et al., 2004)

of having an unsafe well is p, commonly known by households. However, households do not have private information about the arsenic status of their own well or that of other wells. We set the baseline switching rate to 0.28 based on Barnwal et al. (2017). The sharing by exchanging WSCs is fully enforced so that everyone follows the coupon exchange result and testing outcome.

For example, suppose that the targeting community has 80 well-owning households, and the probability, unobserved to the villager, of having a safe well is 0.3. If we assume each household exchanges 5 coupons, the probability after testing of accessing no safe well is $0.7^5 \approx 0.17$. Therefore, 83% of households will be able to access at least one safe well. Conservatively, we assume half of the unsafe well households will not be able to switch because the agreement breaks down for some reasons. The switching rate will then be around 40%, a 12 percentage point increase from the baseline.

Minimal number of villages We calculate the minimal number of treatment villages to guarantee enough power ($\beta = 0.8$) to reject the null at the significance level $\alpha = 0.05$. To reflect our design, we set the number of control clusters equal to the number of treatment clusters with each cluster containing 80 participants.

Conservatively assuming that half of the risk-sharing agreement made by exchanging WSCs will be executed, we calculate the minimal number of treatments to detect the predicted treatment effects calculated from different combinations of safe well probability, number of WSCs exchanged, and intracluster correlations. In Fig. A1 to Fig. A3, we show the minimal number of treatment clusters with the probability of safe well ranging from 0.2 to 0.6, the number of coupons exchanged ranging from 2 to 10, and the ICC ranging from 0.05 to 0.5. These figures show that the current number of treatment (99 villages) and control (36 villages), should guarantee enough power to detect the predicted treatment effects in the majority of cases [9].

3 Specifications

In this section, we map each core research question to specifications and data obtained from the survey.

Ex-ante commitment and risk mitigation

H1: Making ex-ante commitment improves risk mitigation

In the experiment, two households make an ex-ante commitment to share safe wells to reduce their arsenic risk by exchanging WSCs. We postulate that WSCs lead to more sharing of safe wells. On the one hand, committing to share risk explicitly before revealing the status

[9] Given ICC, the minimal number of the clusters needed is decreasing with the proportion of safe wells and the number of WSCs exchanged.

of wells could possibly increase the cost of deviation afterward. This partially resolves the limited commitment problem. On the other hand, WSCs may not increase risk sharing if households only exchange them with other households with whom they would share even without the coupons. Hence, we test the hypothesis with the specification:

$$y_{iv} = \beta_0 + \beta T_v + X_{iv}\gamma + \delta A_v + \eta_u + \epsilon_{iv}. \tag{1}$$

Outcome variable y_{iv} indicates the consumption of safe water by household i in village v, as collected in the endline survey. We define this variable in the following ways –
(1) the status relative to the 50μg/L standard of the primary well households use for drinking water
(2) the arsenic content (continuous variable) of the primary well households use for drinking water at the endline

T_v indicates the WSC treatment assignment i.e., whether village v receives WSC intervention. Hence β is the estimated treatment effect that compares the outcome in treatment (99 communities) with the same in control (36 communities). η_u is the Upazila-level FE. We cluster standard errors at the village level.

Covariates. We include an array of household-level baseline covariates, denoted by X_{iv} above:

- Wealth proxy: asset index
- Demographic variables: Household members' primary education completion rate, household size, male ratio (number of males over the size of household), child ratio (number of children over the size of household)
- Health risk preference (Falk et al., 2016, 2018)

We will also include a control for the village-level proportion of safe wells at the baseline (A_v). It allows us to address concerns about baseline imbalance in arsenic levels across our treatment and control groups.

Sample. We use data from all well-owning households in the whole 135 village communities to estimate Eq. 1. We primarily focus on well-owning households identified at the baseline since only these households were provided with WSCs to exchange. We will also show results by baseline well ownership status

Heterogeneity. We consider two sets of heterogeneity tests by including the interaction of the treatment dummy with the following. First, Households' belief about the severity of arsenic risk may largely shape their motivation to share/switch. Some households may continue to perceive a low risk of arsenic simply because they have been drinking water from the high-arsenic well for a long time without any visible health impact. Others may discount it because arsenic is invisible in water. Second, household demographics including the gender composition of the household and the number of young children may affect their decision. We expect households with more children to be more concerned about arsenic's adverse impact of health (Keskin et al., 2017).

Heterogeneity by well ownership It is important to understand the spillover effect on households that do not own a well at baseline. We have data showing full, shared, or no ownership of wells at the baseline. We will include a full specification showing the ownership heterogeneity in the treatment effect to study whether the effect of WSC program spillover varies with ownership. Note that while the endline survey includes all households, baseline data collection at the time of well testing covers only well owners identified in the census survey. So, for households who didn't own a well, we will have a smaller set of control variables, namely, (1) the household's primary education completion rate, (2) the child ratio, and (3) health risk preferences, as collected in the baseline census survey.

H2: Ex-ante commitment contract complements pre-existing social network's role in sharing risks

One primary channel through which we postulate WSCs improves arsenic mitigation is that exchanging WSCs overcomes the limited commitment problem. We will leverage the social networks data that we collected before the intervention to examine the importance of this channel over and above the existing social network. Social networks support risk-sharing due to stronger trust and commitment between the connected household dyads (Karlan et al., 2009). To test this idea, we estimate the following specification using household pairs (dyads) from the 99-village sample that received WSC treatment (Treatment T):

$$y_{ijv} = \beta_0 + \beta_1 T_v + \beta_2 G_{ijv} + \beta_3 T_v \times G_{ijv} + \eta_u + \epsilon_{ijv} \tag{2}$$

Outcome variable y_{ijv} indicates either household i in village v *shares* safer water with household j or *switches* to a safer well owned by household j. Note that 'safer' here means lower in arsenic.

G_{ijv} indicates whether two households reported being socially connected in response to at least one of the social network survey questions at the baseline. T_v indicates whether households in village v received WSCs treatment.

β_3 captures the interaction effect. We postulate that socially connected households in treatment villages are more likely to share water with each other, i.e., $\beta_3 > 0$. Our prior is that β_1 and β_2 both will be positive. i.e., ex-ante commitment and social connections both increase safe water switching and sharing.

Sample We use all household pairs from each of the 99 WSC village communities to estimate this specification. In an alternative specification, we will restrict this sample to pairs within distances of 50 and 100 m of each other, considering that household members are less likely to fetch water over longer distances on a regular basis in rural Bangladesh.

Peer-monitoring and risk-sharing

H3: Peer-monitoring facilitates risk-sharing

We estimate the impact of induced peer-monitoring on the formation of risk-sharing networks as well as the eventual switching/sharing (treatment T3). Peer-monitoring may potentially strengthen cooperation by increasing the expected cost to a deviating household (i.e., the household that promised to share water ex-ante, but didn't do so ex-post). The monitoring households ('monitor') were randomly selected conditional on their connectedness to the monitored household in the WSC networks. Given that the peer-monitoring was only implemented in half of the 66 villages that were notified about the peer monitoring program, there are three treatment arms considered in this specification: 33 villages with only WSCs (treatment T1), 33 villages with WSCs and peer monitoring notification (treatment T2), and 33 villages with WSCs and peer monitoring (treatment T3). Control villages are in the omitted group. We estimate the treatment effects of peer-monitoring using the following household-level regression specification (Eq. 3):

$$y_{iv} = \beta_0 + \beta_1 T1_v + \beta_2 T2_v + \beta_3 T3_v + X_{iv}\gamma + \delta A_v + \eta_u + \epsilon_{iv} \quad (3)$$

Outcome variable y_{iv} is as defined in Eq. 1. $T1_v$ is the binary variable that indicates whether village v received the WSC intervention only. $T2_v$ indicates that households in village v received the WSC intervention along with only the notification for peer-monitoring. $T3_v$ indicates that households in the village v received WSCs, notification and the peer-monitoring treatment.

X_{iv} contains the household-level characteristics defined in H1. The βs capture the treatment effect of each intervention. β_1 would show the direct effect of exchange WSCs on risk mitigation. β_2 would indicate the mean water sharing/switching in the WSCs+Notification group. β_3 captures the treatment effect of providing peer-monitoring treatment along with the notification and WSCs.

The primary insight of this hypothesis is that we use a two-stage design to identify the effect of anticipated monitoring and the effect of actual monitoring on risk mitigation separately. Here, β_2 captures the effect of any change in strategy during WSC exchange, in response to anticipated monitoring. Anticipating peer-monitoring in the future, households in T2 villages may seek a different set of neighbors when making commitments for water sharing. Since peer monitoring was eventually not implemented in T2 villages, any change in risk mitigation can be attributed to the effect of anticipated monitoring through altered risk-sharing networks structure (also see Hypotheses H6). We test this hypothesis by comparing the corresponding coefficient on T2 with the coefficient on T1, i.e., $\beta_2 - \beta_1 = 0$.

Further, to test whether peer monitoring alone (i.e., net of any anticipation effect at WSC exchange stage) has a significant impact on water sharing or switching, we test whether $\beta_3 - \beta_2 == 0$. The net effect of peer monitoring, however, would include both anticipated and actual peer monitoring. To that end, we test whether $\beta_3 - \beta_1 == 0$ [10]

Similar to the first hypothesis, we include Upazila FE and cluster standard errors at the village-level. We will test for heterogeneous treatment effect in the same way as we specified for Eq. 1.

Sample The sample we used to estimate Eq. 3 is the same as the sample we used in Eq. 1.

We further conduct two tests to understand the mechanisms through which peer monitoring of commitments may work – information transmission and the role of socially influential monitors.

H4: Peer-monitoring increases information transmission

We test whether increasing peer monitoring enhances transmission of relevant information in the community. In the 99 WSC villages, the endline survey elicits each participant's knowledge of some of their first-, second-, and third-degree WSC neighbors' (a) well arsenic and (b)switching/sharing. If peer monitoring indeed enhances information transmission, then households in the peer-monitoring communities should know more about their neighbors' well status, whether the neighbor is sharing water (if low arsenic), and whether the neighbor is able to switch to a safe well (when high arsenic). The omitted group here are the village communities that were only treated with WSCs (treatment groups T1).

We estimate:

$$y_{iv} = \beta_0 + \beta_1 T2_v + \beta_2 T3_v + X_{iv}\gamma + \delta A_v + \eta_u + \epsilon_{iv} \quad (4)$$

[10]Two scenarios of conflict of interest (CoI) deserve our attention here. Say, a 'linking household' is a household that is linked between a monitor and a monitored household via WSC exchange. In the first case, when only the linking household has the unsafe well, the monitor is incentivized to enforce the switching. This is because enforcing the switching between the monitored and linking households will excuse the monitor from sharing the safe well with the linking household (positve CoI). In the second case, only the linking household in the triple has a safe well. If the monitor enforces the switching between the linking and monitored household, the monitor himself or herself could be excluded from using the linking household's safe well. Then the monitor is incentivized not to enforce the switching (negative CoI). We argue that these two situations are unlikely to drive the outcomes for two reasons. First, these two cases are the only two triples that contain a CoI relation among all eight types of triples. Second, a household could be simultaneously monitored by a positive CoI monitor and a negative CoI monitor, then the contradicting forces alleviated or even canceled. Rigorously studying this question is hard in our setting as the monitor is not strictly randomly assigned. We can however take the following approach to assess the influence of CoIs. We will construct a set of CoI indexes that counts the number of positive CoIs and negative CoIs for each monitored household. The PCol (NCoI) counts the number of distinct positive (negative) CoI triples that contains the household. We will show estimates of the correlation of switching outcomes with both COIs.

Outcome variable y_{iv} now measures the household's knowledge about its neighbors' arsenic and risk-sharing status. We construct a score for each household indicating whether they could correctly report the well status and the risk-sharing status of their first-, second-, and third-order neighbors in the WSC networks. The parameter of interest β_2 reflects the level of well and switching-related information disseminated in the local community in peer-monitoring villages (T3), when compared with non-peer-monitoring villages (T1). If the peer-monitoring indeed enhanced information transmission, β_2 should be positive.

Sample We use all households that exchanged WSCs in the 99 WSC villages to estimate this equation.

H5: Socially influential monitors have a stronger effect on the monitored households' well-switching and sharing

In the peer-monitoring intervention, monitors for a given household were randomly selected. Thus, some monitored households may have been assigned more socially influential monitors than others. Such socially influential monitors are likely to have a greater ability to propagate information about the household's risk-sharing status. Since we do not observe social influence directly, we construct a measure of the social influence of a given household head in terms of their network centrality using social network data.[11]

Using the random variation in having socially influential monitors, we test whether peer-monitoring is more effective when socially-influential people monitor in a community (Eq. 5). Specifically, we test whether safe water usage and sharing increase when households are monitored by neighbors with a high degree of network centrality.

$$y_{iv} = \beta_0 + \beta_1 \frac{\sum_{j \in N_{iv}} \omega_j}{|N_{iv}|} + X_{iv}\gamma + \xi_v + \epsilon_{iv} \tag{5}$$

Outcome variable y_{iv} measures consumption of safe water in different ways, as described in the text following Eq.1. X_{iv} is defined earlier. ξ_v is a village-level FE, and standard errors are clustered at the village level.

The key component of the specification is ω_j, which is a proxy for the social influence of i's monitor j. We define it in three different ways – degree, eigenvector, and Katz centrality using the social network data (Banerjee et al., 2013). N_{iv} is the set of i's monitors. Therefore, β measures how the average centrality of monitors affects the realized switching (or sharing) of the monitored household [12].

Sample We use the households from the 99 WSC communities to estimate this equation.

[11] Chandrasekhar et al. (2018) study how social proximity and network centrality may affect cooperation between households. Beaman et al. (2021) provide experimental evidence on network-theory-based learning of new technology.

[12] We will also test for robustness using much denser WSC-network data.

H6: Peer-monitoring affects risk-sharing networks formation

Does the formation of risk-sharing agreements depend on social enforcement? When a household knows that their compliance with ex-ante commitments is eventually going to be monitored by their peers, they may alter the strategy in deciding with households to choose for ex-ante commitments. Thus, the peer-monitoring treatment may alter the formation of risk-sharing networks. Our hypothesis is that this information – that commitments are going to be made partially public – may have a significant effect on a household's willingness to commit and on decisions about whom to commit with.

We informed households in 66 villages about peer monitoring and obtained their consent right before WSC distribution (treatment groups T2 and T3), while 33 villages were provided WSCs only (treatment T1). Thus, by comparing WSC exchanges in peer-monitoring notification villages (66 villages in T2+T3) with the same in non-peer-monitoring-notification villages (33 villages in T1), we can test our hypothesis.

We present two specifications. In the first specification (Eq. 3), we test whether the aggregate level of WSCs exchanged in the communities is affected by peer-monitoring commitment. In the second specification (Eq. 3), we estimate a richer household dyad-level model that allows us to control for household pair-specific factors.

Aggregate-level

$$y_{iv} = \beta_0 + \beta_1 * \mathbb{1}[T2_v = 1 \textit{ or } T3_v = 1] + X_{iv}\gamma + \xi_v + \epsilon_{iv}. \tag{6}$$

Outcome variable y_{iv} is the number of WSCs exchanged by household i in village v. X_{iv} controls for household-level characteristics as in Eq. 1. We include village-level FE, and cluster standard errors at the village-level. The variable $\mathbb{1}[T2_v = 1 \textit{ or } T3_v = 1]$ indicates whether the village receives peer-monitoring notification only (treatment T2) or notification as well as the peer-monitoring treatment (treatment T3). β_1 captures the treatment effect of peer-monitoring notification on number of risk-sharing partners the household seeks. Hence β_1 captures a aggregate-level change in the network.

Similar to H1 (Eq.1), we also test heterogeneous treatment effects with respect to household's arsenic belief, and demographics.

Sample We use data from 99 WSC villages to estimate Eq. 3. By design, we do not have data on WSCs from the 36 villages in the control group.

Household pair(dyad)-level

$$y_{ijv} = \beta_0 + \beta_1 * \mathbb{1}[T2_v = 1 \textit{ or } T3_v = 1] + \beta_2 G_{ijv} + |X_{iv} - X_{jv}|\gamma_1 + |X_{iv} + X_{jv}|\gamma_2 + \xi_v + \epsilon_{ijv}. \tag{7}$$

Outcome variable y_{ijv} in this model indicates whether two households, i and j, in village v, exchanged WSCs. The components $|X_{iv} - X_{jv}|$ and $|X_{iv} + X_{jv}|$ captures the "difference" and "average characteristics" of household i and j . For example, if X is the wealth level, then the first component measures the wealth difference and the second component measures the average wealth level. The corresponding βs hence capture the magnitude of assortative matching and the level effect. Our coefficient of interest is β_1.

In an extension of this empirical specification, we will include an interaction of the difference component with the treatment indicator $\mathbb{1}[T2_v = 1 \textit{ or } T3_v = 1]$. The corresponding estimated coefficients would indicate in which direction and through which channel peer-monitoring notification affects the formation of risk-sharing networks. If the estimated coefficients on the interaction terms are negative, it would suggest that when households expect stronger monitoring in the future, they become more homophilic when selecting their risk-sharing partners.

Sample We use all household pairs (dyads) from 99 WSC villages to estimate Eq. 3.

Attrition

We will quantify and adjust for attrition in the following way. First, we will report the attrition rate of households at the endline. For well-owner households, attrition at the endline will be estimated with respect to the in-person visits at the time of coupon distribution. For all households (including households not owning well), attrition at the endline will be estimated with respect to the in-person visits during the census survey.

Second, we will assess whether attritted and non-attritted households are significantly different using data collected on household characteristics in baseline surveys (Equation 8).

$$attrit_{iv} = \beta_0 + \beta_1 * char_{iv} + \xi_v + \epsilon_{iv} \tag{8}$$

The $attrit_{iv}$ is the binary variable that indicates the attrition status of household i in village v at the endline. $Char_{iv}$ is the list of household characteristics we included as controls in our main specifications. We will cluster the standard errors at the village level. Note that in-person visits and data collection during coupon distribution were conducted only for well owners identified in the census survey. So, for households who didn't own a well, we will use (1) the household's primary education completion rate, (2) the child ratio, and (3) health risk preferences, as collected in the baseline census survey.

Third, to evaluate whether the differential attrition potentially confounds our results, we test if the attrition rates are significantly different across treatment groups through the equation:

$$attrit_{iv} = \beta_0 + \beta_1 * T1_v + \beta_2 * T2_v + \beta_3 * T3_v + X_{iv}\gamma + \delta A_v + \eta_u + \epsilon_i v \tag{9}$$

T is the indicator for the treatment group, and likewise, we will cluster the standard errors at the village level.

Finally, if a concerning level of attrition is detected, we will apply Lee bounds to correct the attrition bias.

Missing values: In addition to attrition, missing values in the control variables collected during baseline surveys may also affect the sample used in the final analysis. To address this issue, we will present estimates after substituting the missing observations with the non-missing sample means and add binary indicators for missing values to the regression.

4 Administrative Information

Funding: The project is supported by National Science Foundation (NSF) grants 1851928 (PI Barnwal) and 1851929 (PI van Geen).

Institutional Review Board (ethics approval): This study obtained IRB approvals from Columbia University (IRB-AAAS0311), Dhaka University (IHE/IRB/DU/33/2022) and Michigan State University (STUDY00002059).

Declaration of interest: Authors have no financial interest in the research.

References

Townsend, R. M. (1994). Risk and insurance in village india. *Econometrica: journal of the Econometric Society*, 539–591.

Udry, C. (1994). Risk and insurance in a rural credit market: An empirical investigation in northern nigeria. *The Review of Economic Studies*, *61*(3), 495–526.

Fafchamps, M., & Lund, S. (2003). Risk-sharing networks in rural philippines. *Journal of development Economics*, *71*(2), 261–287.

Coate, S., & Ravallion, M. (1993). Reciprocity without commitment: Characterization and performance of informal insurance arrangements. *Journal of development Economics*, *40*(1), 1–24.

Kocherlakota, N. R. (1996). Implications of efficient risk sharing without commitment. *The Review of Economic Studies*, *63*(4), 595–609.

Ligon, E. (1998). Risk sharing and information in village economies. *The Review of Economic Studies*, *65*(4), 847–864.

Ligon, E., Thomas, J. P., & Worrall, T. (2002). Informal insurance arrangements with limited commitment: Theory and evidence from village economies. *The Review of Economic Studies*, *69*(1), 209–244.

Carson, R. T., Koundouri, P., & Nauges, C. (2011). Arsenic mitigation in bangladesh: A household labor market approach. *American Journal of Agricultural Economics*, *93*(2), 407–414.

Abdul, K. S. M., Jayasinghe, S. S., Chandana, E. P., Jayasumana, C., & De Silva, P. M. C. (2015). Arsenic and human health effects: A review. *Environmental toxicology and pharmacology*, *40*(3), 828–846.

Pitt, M. M., Rosenzweig, M. R., & Hassan, M. N. (2021). Identifying the costs of a public health success: Arsenic well water contamination and productivity in bangladesh. *The Review of Economic Studies*, *88*(5), 2479–2526.

van Geen, A., Ahsan, H., Horneman, A. H., Dhar, R. K., Zheng, Y., Hussain, I., Ahmed, K. M., Gelman, A., Stute, M., Simpson, H. J., et al. (2002). Promotion of well-switching to mitigate the current arsenic crisis in bangladesh. *Bulletin of the World Health Organization*, *80*(9), 732–737.

Madajewicz, M., Pfaff, A., Van Geen, A., Graziano, J., Hussein, I., Momotaj, H., Sylvi, R., & Ahsan, H. (2007). Can information alone change behavior? response to arsenic contamination of groundwater in bangladesh. *Journal of development Economics*, *84*(2), 731–754.

Barnwal, P., van Geen, A., von der Goltz, J., & Singh, C. K. (2017). Demand for environmental quality information and household response: Evidence from well-water arsenic testing. *Journal of Environmental Economics and Management*, *86*, 160–192.

Tarozzi, A., Maertens, R., Ahmed, K. M., & Van Geen, A. (2021). Demand for information on environmental health risk, mode of delivery, and behavioral change: Evidence from sonargaon, bangladesh. *The World Bank Economic Review*, *35*(3), 764–792.

Pfaff, A., Schoenfeld Walker, A., Ahmed, K. M., & van Geen, A. (2017). Reduction in exposure to arsenic from drinking well-water in bangladesh limited by insufficient testing and awareness. *Journal of Water, Sanitation and Hygiene for Development*, *7*(2), 331–339.

Fendorf, S., Michael, H. A., & van Geen, A. (2010). Spatial and temporal variations of groundwater arsenic in south and southeast asia. *Science*, *328*(5982), 1123–1127.

Falk, A., Becker, A., Dohmen, T. J., Huffman, D., & Sunde, U. (2016). The preference survey module: A validated instrument for measuring risk, time, and social preferences.

Falk, A., Becker, A., Dohmen, T., Enke, B., Huffman, D., & Sunde, U. (2018). Global evidence on economic preferences. *The Quarterly Journal of Economics*, *133*(4), 1645–1692.

Feigenberg, B., Field, E., & Pande, R. (2013). The economic returns to social interaction: Experimental evidence from microfinance. *Review of Economic Studies*, *80*(4), 1459–1483.

Ashraf, N., Karlan, D., & Yin, W. (2006). Tying odysseus to the mast: Evidence from a commitment savings product in the philippines. *The Quarterly Journal of Economics*, *121*(2), 635–672.

Giné, X., Karlan, D., & Zinman, J. (2010). Put your money where your butt is: A commitment contract for smoking cessation. *American Economic Journal: Applied Economics*, *2*(4), 213–35.

Kaur, S., Kremer, M., & Mullainathan, S. (2015). Self-control at work. *Journal of Political Economy*, *123*(6), 1227–1277.

Kinnan, C. (2022). Distinguishing barriers to insurance in thai villages. *Journal of Human Resources*, *57*(1), 44–78.

Stiglitz, J. E. (1990). Peer monitoring and credit markets. *The world bank economic review*, *4*(3), 351–366.

Hermes, N., Lensink, R., & Mehrteab, H. T. (2005). Peer monitoring, social ties and moral hazard in group lending programs: Evidence from eritrea. *World development*, *33*(1), 149–169.

Breza, E., & Chandrasekhar, A. G. (2019). Social networks, reputation, and commitment: Evidence from a savings monitors experiment. *Econometrica*, *87*(1), 175–216.

Barr, A., & Genicot, G. (2008). Risk sharing, commitment, and information: An experimental analysis. *Journal of the European Economic Association*, *6*(6), 1151–1185.

Attanasio, O., Barr, A., Cardenas, J. C., Genicot, G., & Meghir, C. (2012). Risk pooling, risk preferences, and social networks. *American Economic Journal: Applied Economics*, *4*(2), 134–67.

Fafchamps, M., & Gubert, F. (2007). The formation of risk sharing networks. *Journal of development Economics*, *83*(2), 326–350.

van Geen, A., Ahmed, E. B., Pitcher, L., Mey, J. L., Ahsan, H., Graziano, J. H., & Ahmed, K. M. (2014). Comparison of two blanket surveys of arsenic in tubewells conducted 12 years apart in a 25 km2 area of bangladesh. *Science of the Total Environment*, *488*, 484–492.

Jamil, N. B., Feng, H., Ahmed, K. M., Choudhury, I., Barnwal, P., & Van Geen, A. (2019). Effectiveness of different approaches to arsenic mitigation over 18 years in araihazar, bangladesh: Implications for national policy. *Environmental Science & Technology*, *53*(10), 5596–5604.

Banerjee, A., Chandrasekhar, A. G., Duflo, E., & Jackson, M. O. (2013). The diffusion of microfinance. *Science*, *341*(6144), 1236498.

Barnwal, P., Yao, Y., Wang, Y., Juy, N. A., Raihan, S., Haque, M. A., & van Geen, A. (2021). Assessment of excess mortality and household income in rural bangladesh during the covid-19 pandemic in 2020. *JAMA network open*, *4*(11), e2132777–e2132777.

George, C. M., Zheng, Y., Graziano, J. H., Rasul, S. B., Hossain, Z., Mey, J. L., & Van Geen, A. (2012). Evaluation of an arsenic test kit for rapid well screening in bangladesh. *Environmental science & technology*, *46*(20), 11213–11219.

Gelman, A., Trevisani, M., Lu, H., & Van Geen, A. (2004). Direct data manipulation for local decision analysis as applied to the problem of arsenic in drinking water from tube wells in bangladesh. *Risk Analysis: An International Journal*, *24*(6), 1597–1612.

Keskin, P., Shastry, G. K., & Willis, H. (2017). Water quality awareness and breastfeeding: Evidence of health behavior change in bangladesh. *Review of Economics and Statistics*, *99*(2), 265–280.

Karlan, D., Mobius, M., Rosenblat, T., & Szeidl, A. (2009). Trust and social collateral. *The Quarterly Journal of Economics*, *124*(3), 1307–1361.

Chandrasekhar, A. G., Kinnan, C., & Larreguy, H. (2018). Social networks as contract enforcement: Evidence from a lab experiment in the field. *American Economic Journal: Applied Economics*, *10*(4), 43–78.

Beaman, L., BenYishay, A., Magruder, J., & Mobarak, A. M. (2021). Can network theory-based targeting increase technology adoption? *American Economic Review*, *111*(6), 1918–43.

5 Figures

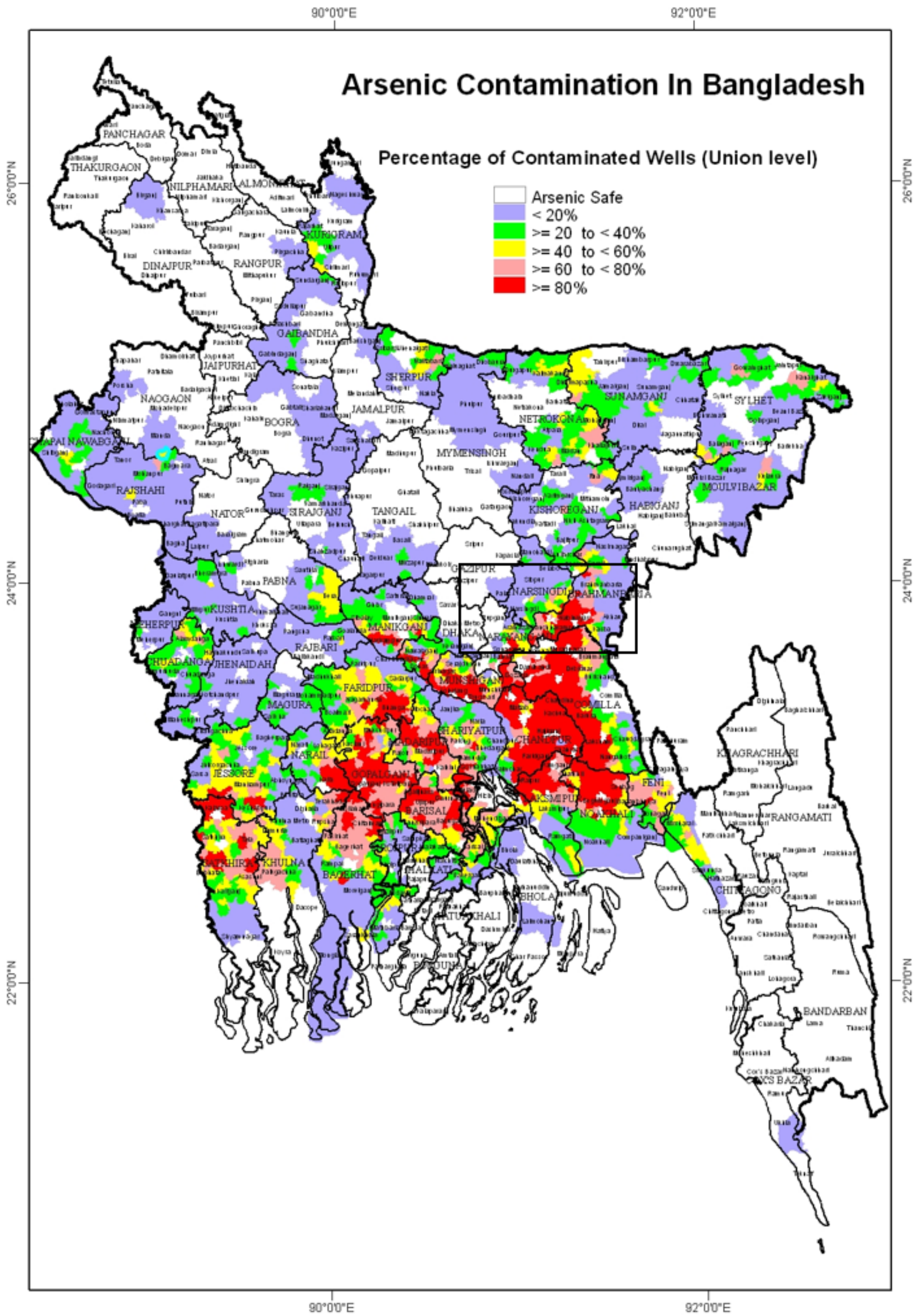


Figure 1: The testing result by Bangladesh Arsenic Mitigation and Water Supply Program (BAMWSP)

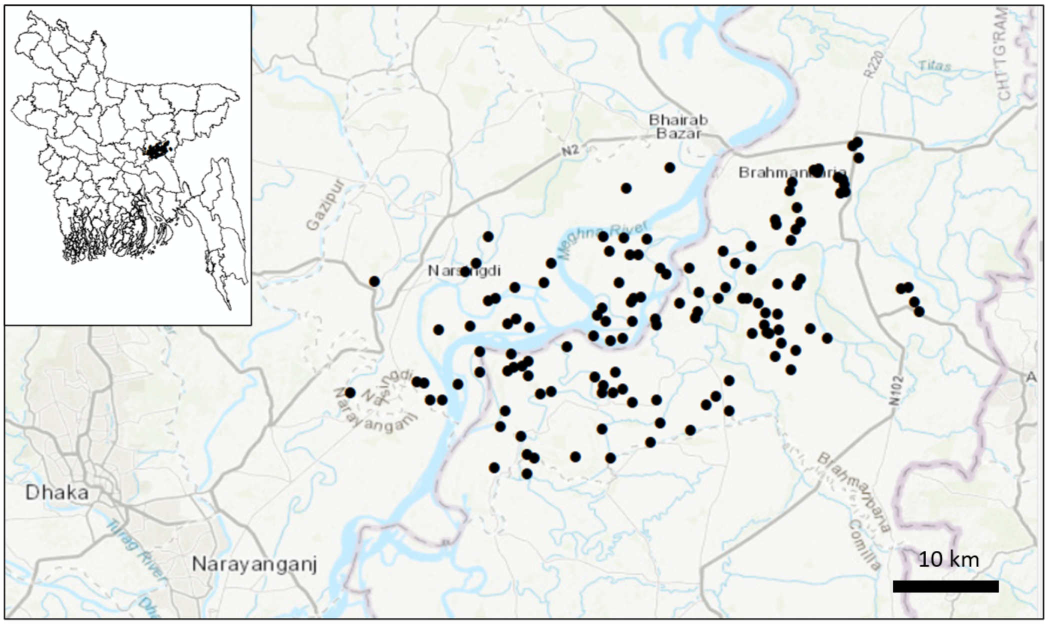


Figure 2: The map of 135 village communities

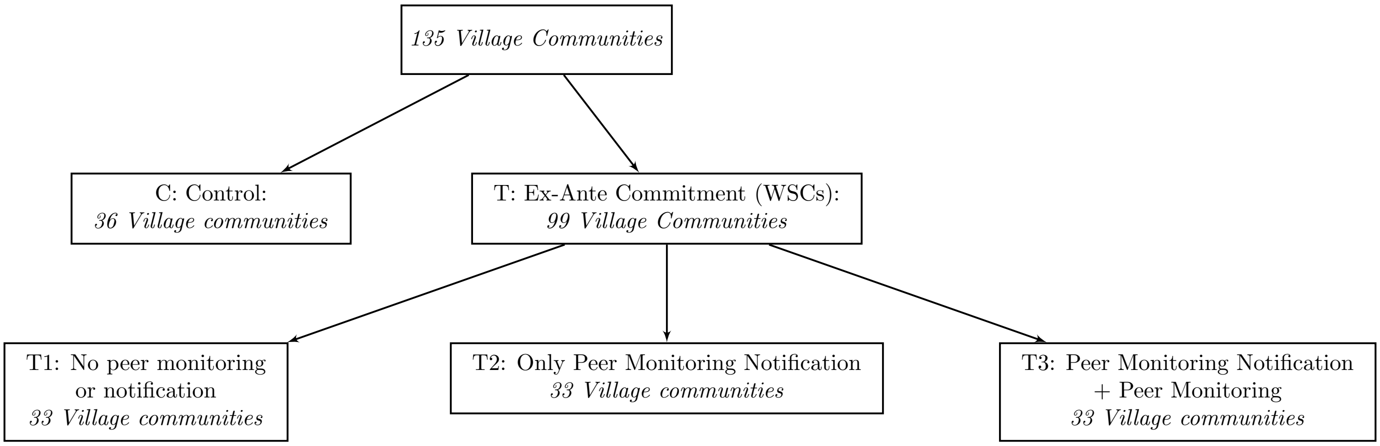


Figure 3: Treatment Arms

Figure 4: A well tag

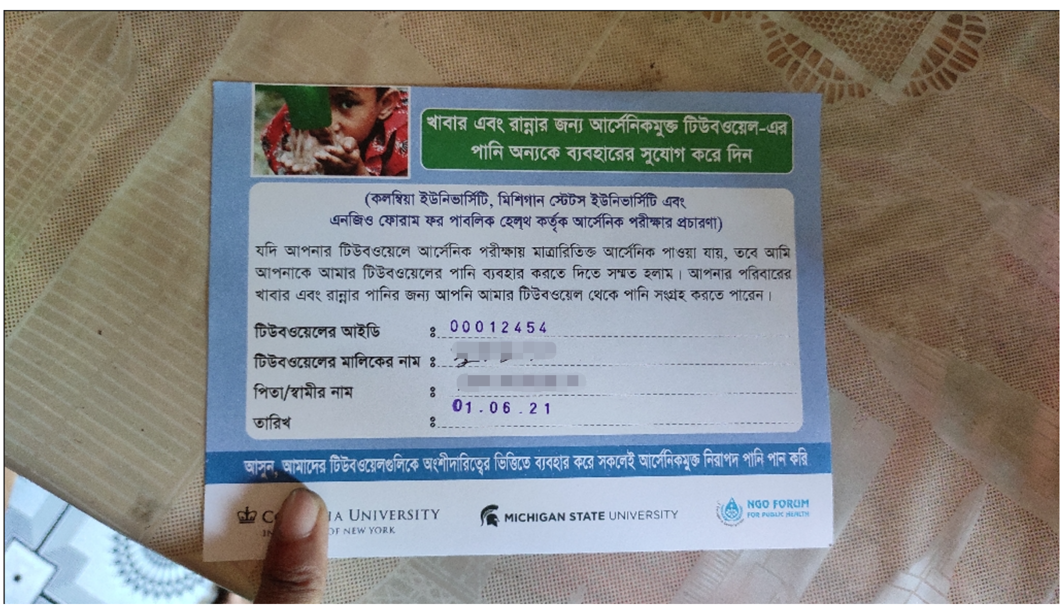
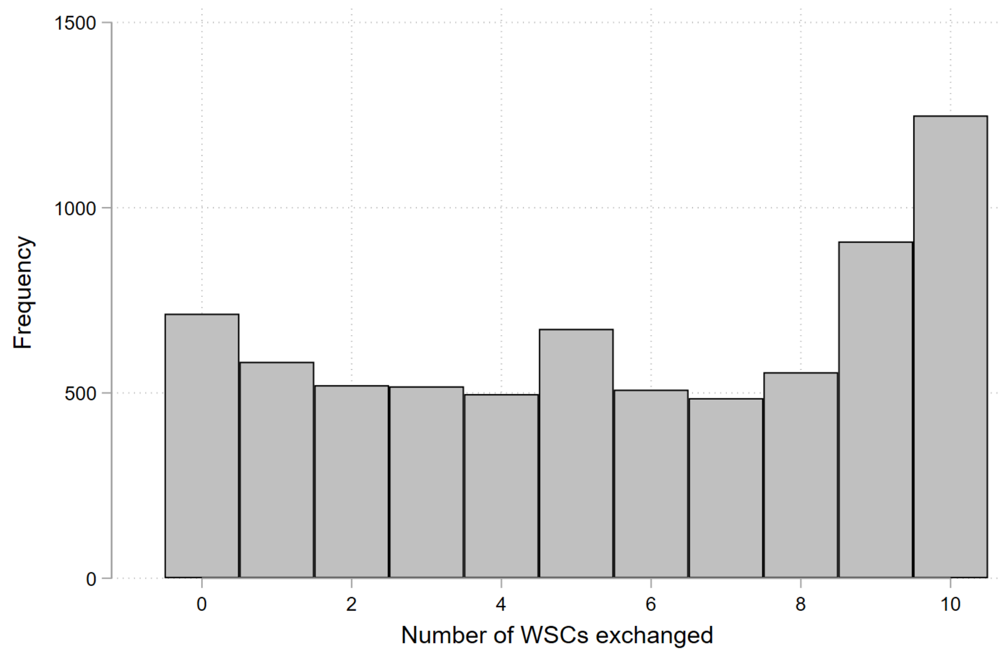


Figure 5: Water sharing coupon

Figure 6: Test result card

Figure 7: The distribution of number of WSC exchanged



NOTE: This histogram shows the distribution of number of commitments (WSCs exchanged) made per well-owning households before well testing. Households were provided 10 WSCs per well.

6 Tables

Table 1: Study Timeline

Phases	Expected Completion	Status
Household listing	January 2020	Completed
Baseline surveys		
First-round	May 2020 - June 2020	Completed
Second-round	November 2020 - December 2020	Completed
Intervention: WSC exchange	March 2021 - August 2021	Completed
Well testing	December 2021 - July 2022	Completed
Intervention: text message	March 2022 - July 2022	Completed
Endline survey	September - October 2022	

Table 2: Treatment arms and number of households

Treatment Arms	Number of Villages	Number of Households
Control (C)	36	4,363
Ex-ante Commitment (T)	99	11,691
No notification or peer-monitoring (T1)	(*33*)	(*4,154*)
Only notification, but no peer monitoring (T2)	(*33*)	(*3,689*)
Notification and Peer-monitoring (T3)	(*33*)	(*3,848*)
Total:	135	16,054

Table 3: 'Notification for Peer-monitoring' treatment in T2+T3 communities

Treatment Arms	Content
135 villages (T+C)	Hello, my name is XXX and I work for NGO Forum for Public Health, Bangladesh, a non-profit organization and collaborating with researchers from Columbia University and Michigan State University. The goal of our project is to reduce exposure to arsenic by drinking contaminated well-water. We expect that the government's Department of Public Health and Engineering will test your tubewell for arsenic in the coming weeks. Members of our team visited you for a household survey a first time in January 2020 and followed up with two rounds of phone calls.
99 villages (T)	This time we would like to invite you to take part in an activity designed to encourage your and your neighbors to share those wells that turn out to be low in arsenic after DPHE testing. If you agree to participate, we will give you 10 water sharing coupons that we encourage you to exchange with any other well-owner who would be willing to let you use his well and with whom you would be willing to share your own well. You will have 3 days for exchanging coupons, after which we will return to record the coupons you have exchanged as well as the coupons you have not exchanged. We will also record your response to a number of questions about your household and water usage using tablets or smartphones. We will recheck your name and phone number(s) for future reference as well. We may occasionally send you text (SMS) messages to remind you of the health risks from drinking tubewell water with toxic levels of arsenic and to share information about this study.
66 villages (T2 + T3)	We may also send you a text (SMS) message to a maximum of 3 of your neighbors listing the names of households you agreed to share your well with using the coupons. Conversely, you may receive a text (SMS) message listing the names of households up to 3 of your neighbors agreed to share their well with using the coupons.
135 villages (T+C)	If you have no objection, please allow NGO Forum for Public Health to take "GPS" coordinates of where we are conducting the interview. This means we will use our phone to mark our interview location on a map. Once the survey work is completed your identifying information will be masked before data is analyzed, so no one will be able to identify you from your answers. The results of this study may be published or presented at professional meetings, but the identities of all research participants will remain anonymous in any research presentation or publication. The following entities will have access to the data: Researchers and Research Staff, Institutional Review Board (IRB). Implementing agencies- NGO Forum for Public Health, Bangladesh and Innovations for Poverty Action (IPA)

Table 4: Text message and voice call

Treatment	Coverage	Content
First General Text Message	135 villages (T+C)	Hello! We, NGO Forum, recently visited your para for arsenic testing of wells. Here is some information regarding arsenic. Arsenic is toxic for your and your children's health and well-being. Drink low arsenic water for the sake of your well-being!
Second Text Message	66 villages (T1+T2)	Recall that, before well testing, you and your peers in this para agreed to share water by exchanging water-sharing coupons.
Third Text Message	33 villages (T3)	Recall that, before well testing, you and your peers in this para agreed to share water by exchanging water-sharing coupons. We will share the names of people, who exchanged coupons in your para through SMS.
Customized monitor message	33 villages (T3)	Hello! Mr/Ms AAA, XXX in your para exchanged coupons with YYY, ZZZ, and N other households.
Customized receipt message	33 villages (T3)	Hello! Mr/Ms XXX, We have shared names of people, who you exchanged coupons with, to ABC and DEF.
Voice message	135 villages (T + C)	Hello! We, NGO Forum, recently visited your para for arsenic testing of wells. Here is some information regarding arsenic. Arsenic is toxic for your and your children's health and well-being. Drink low arsenic water for the sake of your well-being!
Second voice message	66 villages (T1 + T2)	Recall that, before well testing, you and your peers in this para agreed to share water by exchanging water-sharing coupons
Third voice message	33 villages (T3)	Recall that, before well testing, you and your peers in this para agreed to share water by exchanging water-sharing coupons. We will share the names of people, who exchange coupons in your para through SMS.

Table 5: Well arsenic test results

Arsenic concentration (μ g/L)	Count	Percentage	Cumulative
0	1234	12.54%	12.54%
0-10	1130	11.48%	24.03%
10-50	1467	14.91%	38.94%
50-300	4028	40.94%	79.88%
300-	1980	20.12%	100.00%
Total	9839	100%	

Note: We used two testing kits in the field with different scales. Therefore this table consolidates the results from two kits.

Table 6: Social Networks

VARIABLES	N	mean	sd
Socialization	11,933	3.017	2.495
Discuss farming issues	11,933	0.788	1.433
Discuss health issues	11,933	0.879	1.433
Discuss financial issues	11,933	0.84	1.387
Borrow or lend daily necessities	11,933	1.336	1.706
Borrow or lend money	11,933	1.162	1.532
Total Degree	11,933	4.086	3.970

Note: We asked households who they interact with in respect to each category. For example, *Discuss health issues* means that respondents were asked to elicit the households they often discussed health issues with.

Table 7: Summary Statistics: Household and well characteristics

	count	mean	sd	min	max
Household size[1]	16054	5.10	2.05	1	21
Average age	16054	27.07	10.74	6	100
Male ratio	16054	0.48	.18	0	1
Child ratio	16054	0.39	0.21	0	1
Primary edu ratio[2]	16054	0.29	0.26	0	1
Risk tolerance[3]	14039	1.86	1.16	1	5
Asset PCA Index	13294	0.00	1.00	-2.64	9.99
Number of wells	16054	0.80	0.52	0	4
Well depth[4]	7716	131.43	91.99	1	1000
Well age	9732	9.90	7.41	1	81
Well tested for arsenic	10032	0.07	0.263	0	1

[1] A household is defined as a group of relatives that share the same kitchen

[2] Ratio of household member that completed at least primary education;

[3] How much the respondent tolerates health risks, ranging from 1 to 5, with 1 being extremely unwilling to tolerate.

[4] In a few cases that household owned multiple wells, we use the well that reported as the primary well to measure the depth, age, and whether tested for arsenic.

Table 8: Summary Statistics: Assets

	(1)				
	count	mean	sd	min	max
Rooms	13741	2.70	1.30	0	10
Electricity[1]	13823	.98	.15	0	1
Fans	13778	2.33	1.23	0	10
Mobilephone	13733	1.89	1.12	0	10
Smartphone	13720	0.87	0.96	0	10
Cycle or rickshaw	13709	0.11	0.39	0	10
Motorcycle	13709	0.06	0.27	0	8
TV	13759	0.47	0.53	0	8
Refrigerator	13759	0.52	0.53	0	8

[1] Whether the household has access to electricity

Table 9: Balance across treatment arms: Household and well characteristics

	C	T1	T2	T3	Differences p-value					
					T1-C	T2-C	T3-C	T3-T2	T2+T3-T1	T1+T2+T3-C
Household size[1]	5.1 (2.06) [4363]	5.06 (2.09) [4154]	5.11 (2.04) [3689]	5.07 (1.99) [3848]	-0.039	0.008	-0.035	-0.043	0.025	-0.023
Average age	27.1 (10.76) [4363]	27.29 (10.52) [4154]	26.95 (10.84) [3689]	27.24 (10.96) [3848]	0.191	-0.151	0.14	0.291	-0.193	0.066
Male ratio	0.42 (0.2) [4363]	0.41 (0.2) [4154]	0.41 (0.2) [3689]	0.42 (0.2) [3848]	-0.01	-0.003	0.007	0.01	0.012**	-0.002
Child ratio	0.4 (0.21) [4363]	0.39 (0.22) [4154]	0.4 (0.21) [3689]	0.4 (0.21) [3848]	-0.009	0.006	0	-0.005	0.011	-0.001
Primary edu ratio[2]	0.28 (0.25) [4363]	0.3 (0.27) [4154]	0.28 (0.25) [3689]	0.29 (0.26) [3848]	0.022	-0.003	0.009	0.011	-0.019	0.01
Risk tolerance[3]	1.86 (1.15) [3822]	1.85 (1.17) [3616]	1.87 (1.17) [3273]	1.86 (1.14) [3328]	-0.007	0.012	-0.001	-0.013	0.013	0.001
Asset PCA Index	0.00 (1.03) [3547]	0.04 (0.99) [3510]	0.00 (1.00) [3074]	-0.04 (0.97) [3163]	0.04	0.001	-0.035	-0.036	-0.057	0.003
Number of wells	0.81 (0.52) [4363]	0.79 (0.54) [4154]	0.79 (0.53) [3689]	0.81 (0.49) [3848]	-0.019	-0.014	-0.002	0.012	0.011	-0.012
Well depth[4]	124.64 (116.36) [1981]	126.38 (100.28) [1739]	134.64 (103.35) [1785]	123.64 (85.46) [1834]	1.74	9.997	-1.003	-11	2.682	3.552
Well age	10.19 (7.67) [2669]	10.08 (7.55) [2330]	9.85 (7.42) [2249]	10.21 (7.40) [2395]	-0.114	-0.345	0.018	0.363	-0.044	-0.143
Well tested for arsenic	0.07 (0.25) [2768]	0.09 (0.29) [2509]	0.07 (0.26) [2302]	0.06 (0.24) [2453]	0.027*	0.007	-0.003	-0.01	-0.025	0.011

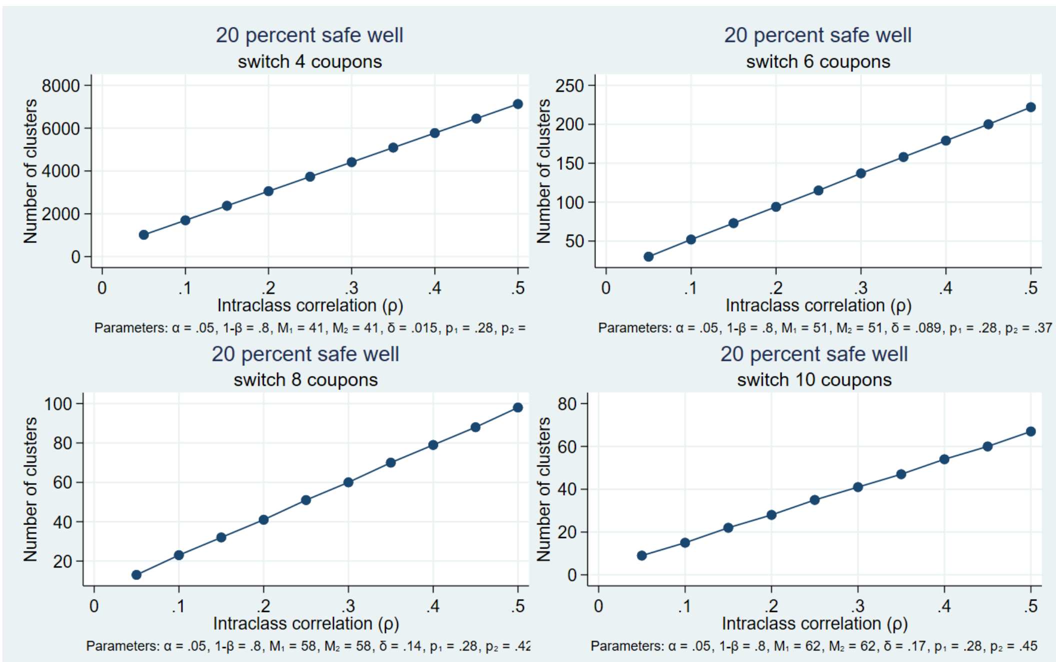
[1] A household is defined as a group of relatives that share the same kitchen
[2] Ratio of household member that completed at least primary education;
[3] How much the respondent tolerates health risks, ranging from 1 to 5, with 1 being extremely unwilling to tolerate.
[4] In a few cases that household owned multiple wells, we use the well that reported as the primary well to measure the depth, age, and whether tested for arsenic.

Table 10: Balance across treatment arms: Assets

	C	T1	T2	T3	Differences p-value					
					T1-C	T2-C	T3-C	T3-T2	T2+T3-T1	T1+T2+T3-C
Number of wells	0.81 (0.52) [4363]	0.79 (0.54) [4154]	0.79 (0.53) [3689]	0.81 (0.49) [3848]	-0.019	-0.014	-0.002	0.012	0.011	-0.012
Rooms	2.71 (1.31) [3698]	2.73 (1.34) [3607]	2.68 (1.31) [3146]	2.66 (1.22) [3290]	0.024	-0.033	-0.053	-0.02	-0.068	-0.019
Electricity[1]	0.98 (0.14) [3718]	0.98 (0.15) [3624]	0.98 (0.15) [3165]	0.98 (0.15) [3316]	-0.001	-0.004	-0.002	0.002	-0.002	-0.002
Fans	2.34 (1.23) [3707]	2.35 (1.25) [3613]	2.34 (1.24) [3156]	2.29 (1.20) [3302]	0.008	0.001	-0.05	-0.051	-0.033	-0.013
Mobile phone	1.92 (1.19) [3694]	1.9 (1.07) [3606]	1.87 (1.13) [3145]	1.86 (1.09) [3288]	-0.019	-0.043	-0.053	-0.01	-0.029	-0.038
Smartphone	0.86 (1.00) [3689]	0.86 (0.91) [3600]	0.9 (0.98) [3144]	0.86 (0.92) [3287]	0.003	0.039	-0.003	-0.042	0.015	0.012
Cycle or rickshaws	0.1 (0.38) [3681]	0.12 (0.37) [3602]	0.1 (0.4) [3141]	0.13 (0.39) [3285]	0.011	-0.01	0.022	0.032	-0.004	0.008
Motorcycle	0.06 (0.28) [3681]	0.08 (0.32) [3602]	0.05 (0.22) [3141]	0.06 (0.24) [3285]	0.02	-0.014	-0.005	0.009	-0.029*	0.001
Television	0.46 (0.53) [3694]	0.51 (0.51) [3613]	0.49 (0.56) [3152]	0.44 (0.54) [3300]	0.048	0.03	-0.013	-0.042	-0.041	0.022
Refrigerator	0.49 (0.53) [3694]	0.54 (0.52) [3613]	0.53 (0.52) [3152]	0.5 (0.55) [3300]	0.051	0.043	0.014	-0.029	-0.023	0.036

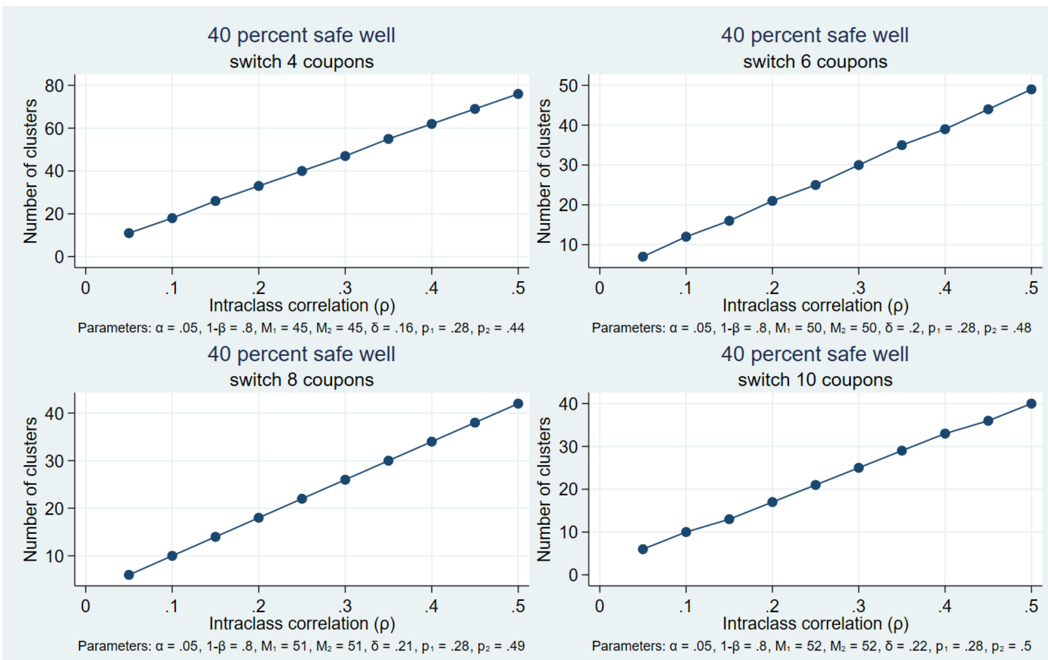
[1] Whether the household has access to electricity

Appendix



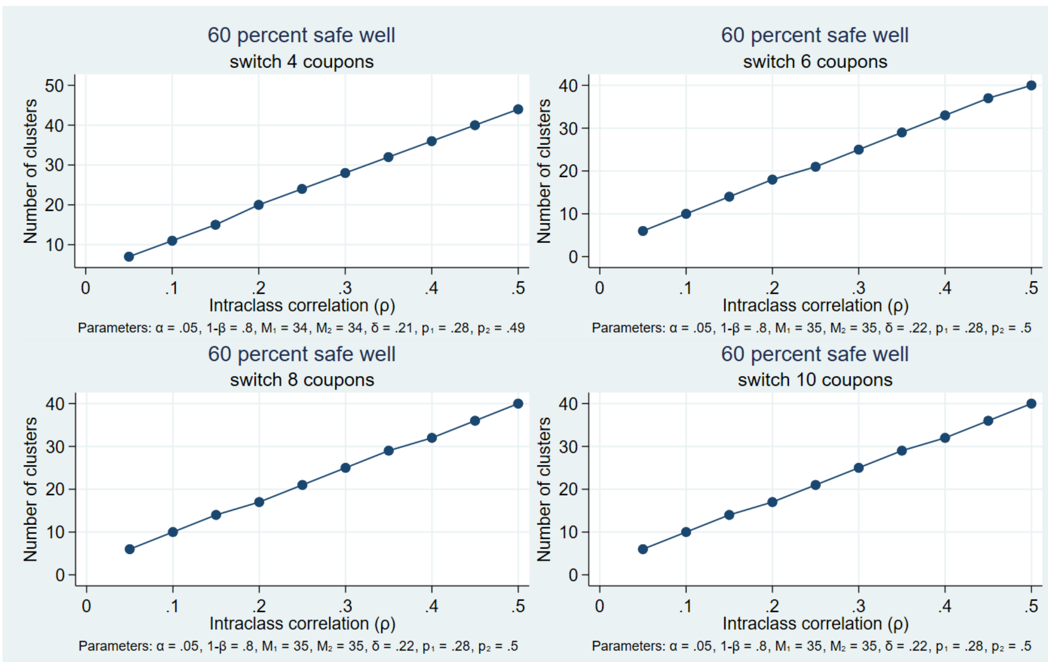
NOTES: $M1$ and $M2$ are the predicted number of population that have at least one safe well to switch to. p_1 is the baseline switching rate. p_2 is the predicted switching rate. Henceforth δ is the predicted treatment effect.

Figure A1: Minimal number of treatment clusters with 20% of safe wells



NOTES: $M1$ and $M2$ are the predicted number of population that have at least one safe well to switch to. p_1 is the baseline switching rate. p_2 is the predicted switching rate. Henceforth δ is the predicted treatment effect.

Figure A2: Minimal number of treatment clusters with 40% of safe wells



NOTES: $M1$ and $M2$ are the predicted number of population that have at least one safe well to switch to. p_1 is the baseline switching rate. p_2 is the predicted switching rate. Henceforth δ is the predicted treatment effect.

Figure A3: Minimal number of treatment clusters with 60% of safe wells

Table A1: Table: Number of wells recorded at each stage of the intervention

Treatment Arms	**Number of villages**	**Number of private wells census**	**Number of private wells arsenic test survey**
Control (C)	36	3,028	2,692
No notification or peer-monitoring (T1)	33	2,874	2,455
Only notification but no peer-monitoring (T2)	33	2,547	2,283
Notification and peer-monitoring (T3)	33	2,705	2,409
Total	**135**	**11,154**	**9,839**

Click here to access/download

LaTeX Source File

NSFBangladesh1922PAPtex.zip

Author Statement

Prabhat Barnwal: Conceptualization, Methodology, Supervision, Analysis, Writing- Reviewing and Editing
Alexander van Geen: Conceptualization, Methodology, Supervision, Analysis, Writing- Reviewing and Editing
Yiqian Wang: Conceptualization, Methodology, Data cleaning, Analysis, Writing- Reviewing and Editing