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Demand for environmental quality information and household response: Evidence from well-water arsenic testing ☆

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ABSTRACT

Access to information about environmental quality may facilitate low-cost preventive measures that protect human health. In this paper, we study the demand for information about environmental quality and the behavioral response to the information provided. With a field experiment conducted in Bihar (India), we estimate the price sensitivity of demand for diagnostic testing of drinking water wells for arsenic of natural origin – a serious threat to the health of tens of millions of villagers across South and Southeast Asia. Demand is substantial but sensitive to price; uptake falls from 68% to 31% of households over our price range (Rs. 10 to Rs. 50). We further assess how households respond to information regarding the contamination level in their wells. About one-third of households with unsafe wells switch to a safer water source. Finally, we demonstrate that households that received adverse test outcomes are more likely to selectively forget test results, and proactively remove evidence of their wells' arsenic status.

1 Introduction

There is considerable policy interest in assessing the demand for information on environmental quality that is relevant to human health, as well as in understanding how individuals respond to this information (Pattanayak and Pfaff et al., 2009; Somanathan, 2010; Greenstone and Jack, 2015). In this paper, we study both issues in the context of arsenic contamination in well water in Bihar, India.

Environmental hazards can be particularly severe in developing countries because of low willingness to pay for environmental quality and poor enforcement of environmental regulations (Greenstone and Jack, 2015). Exposure to such

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hazards can, in turn, have significant health effects, reduce productivity, and adversely impact economic development (Smith et al., 2000; Currie et al., 2013; Hanna and Oliva, 2015; Graff Zivin and Neidell, 2013). It therefore becomes important to understand how to effectively minimize health risks posed by environmental hazards.

In this paper, we consider one possible approach, namely the provision of information on local contamination levels. Information can prompt the use of effective preventive measures, which – as in the case of preventive health products, such as insecticide-treated nets (ITNs) to prevent malaria infection or filters to remove microbial pathogens from drinking water (Ahuja et al., 2010; Sachs and Malaney, 2002) – can have high private benefits.[1] Importantly, when the local environmental quality is heterogeneous, it can be much more efficient for households to acquire information before deciding to invest in potentially costly prevention or treatment. Both the demand for information on local environmental quality and the way information is used therefore matter for health outcomes and allocative efficiency. A body of evidence has established the high price sensitivity of demand for preventive health products: even relatively modest fees reduce take-up significantly (Bates et al., 2012; Kremer and Miguel, 2007; Dupas, 2014a).[2] In contrast, Cohen et al. (2015) document a lack of price sensitivity for a diagnostic test for health. The allocative efficiency aspect of providing local environmental quality (or health status) information is less well-studied; Cohen et al. (2015) being a notable exception.

Our focus is on a diagnostic product for environmental quality which only provides the information that is necessary to take preventive measures for reducing toxic exposure, but does not in itself constitute a way of avoiding exposure. In a field experiment conducted in Bihar, India, we sold a diagnostic test for arsenic levels in well water at five randomly assigned price levels. Next, we explored how households responded to the information about local environmental quality that was revealed by the arsenic tests. We assessed, firstly, the essential question of whether testing had the intended effect, that is, whether the provision of information led to preventive measures; and secondly, whether households proactively concealed the information revealed by these tests because, for instance, revealing adverse information may run counter to social norms, carry stigma, or reduce asset values. This study exploits two sources of exogenous variation, namely experimental variation in prices, and quasi-experimental spatial variation in arsenic occurrence. We use the village-level price variation to estimate demand, and the household-level arsenic occurrence to analyze the behavioral response to the information revealed by tests.

Elevated arsenic concentrations in well water were first reported in the mid-1980s in West Bengal, and subsequently shown to extend over a much broader area (Ahmed et al., 2006; Chakraborti et al., 2003; Fendorf et al., 2010). The contamination of subsurface aquifers in the region is of natural origin and pre-dates any significant perturbation due to human activities. However, the installation of tens of millions of inexpensive tube wells by individual households, and therefore human exposure to arsenic in drinking water, began in earnest only in the early 1980s (van Geen et al., 2002). A key feature of the distribution of arsenic in the groundwater of the region is tremendous spatial variability, but modest, if any, fluctuations in concentrations over time. Early concerns that arsenic levels might be rising systematically have not been confirmed (Fendorf et al., 2010). A well that meets the World Health Organization (WHO) guideline for arsenic in drinking water of 10 μg/l can be located within a few tens of meters of a well with concentrations which are 10 or even 100 times higher (van Geen et al., 2002).

Arsenic testing of well water shares important product traits with other highly efficient preventive health interventions (Pattanayak and Pfaff et al., 2009). Testing is a requirement for identifying a household at risk, but can also often point to a solution in the form of a neighbor's well that is low in arsenic and could, potentially, be shared. Because the distribution of arsenic occurrence in groundwater is difficult to predict and varies greatly even over small distances, the safety of a well cannot be predicted without a test unless, in many areas, the well is over 150 m deep (van Geen et al., 2002; Choudhury et al., 2016; van Geen et al., 2003).

In previous interventions, about one quarter to two-thirds of households with contaminated wells were shown to switch to safer sources after the free provision of arsenic testing (Ahmed et al., 2006; Chen et al., 2007; Madajewicz et al., 2007; Pfaff et al., 2017). Much like other basic preventive health products, arsenic tests are also relatively inexpensive. The cost of the kit is only USD 0.30 per test when purchased in bulk; however, salaries, data collection, and attaching a metal placard with the test result to a well added another USD 2.00 per entry. In terms of the total cost of reducing exposure to households, this does not include the inconvenience of walking further to a different low-arsenic well or the potential social cost of switching to a neighbor's well. (Madajewicz et al. (2007) quantify the extra time; Pfaff et al. (2017) suggest a social cost based on walking further than necessary).

In stark contrast, the health consequences of chronic arsenic exposure are dramatic even if typically, not attributed to arsenic. By combining a national exposure assessment with mortality data from two large cohort studies, Flanagan et al. (2012) attributed 6% of current mortality in Bangladesh to past exposure to arsenic contained in well water. The majority of these deaths are caused by cardiovascular diseases (Chen et al., 2011). Exposure to arsenic from drinking well water has also been associated with impaired intellectual and motor function in children (Parvez et al., 2011; Wasserman et al., 2004) and lower mental health in adults (Chowdhury et al., 2015). As a result, arsenic exposure affects income and labor supply: Pitt et al. (2015) estimate that lowering the amount of retained arsenic among adult men in Bangladesh to levels encountered in

[1] In addition to private benefits, environmental quality information can also have social externalities, if it reduces the cost of avoidance for other households in the community – as is the case in our setting.

[2] Demand for preventive health products such as ITNs and water filters is relatively well studied in the context of cost-sharing, which is often suggested as a way to reduce dependency on public programs without exposing consumers to the full cost of market provision.

uncontaminated countries would increase earnings by 9%. Matching households to arsenic exposure, Carson et al. (2011) find that overall household labor supply is 8% smaller due to arsenic exposure. Chowdhury et al. (2015) estimate the mental health burden of arsenic contamination for affected individuals alone to be as high as the annual household income in Bangladesh.

Because of these health concerns, millions of wells have been tested for arsenic with inexpensive field kits used over the past two decades through a public provision in rural communities across the Indo-Gangetic Plain (Ahmed et al., 2006; Fendorf et al., 2010; Nickson et al., 2007). However, even these large programs have not come close to comprehensively covering the geographical area where arsenic is of concern, including our study area (Chakraborti et al., 2003). Past testing campaigns have also not attempted to keep up with continuing installation of new wells and the replacement of malfunctioning wells. For example, after a single blanket testing covering five million wells by the government of Bangladesh in 2000–2005, no further country-wide public programs have been undertaken as of the time of writing. In consequence, more than half the tube wells currently in use in Bangladesh have never been tested for arsenic (WASH, 2008; George et al., 2012a; van Geen et al., 2014; Ahmed et al., 2006). Public provision has hence not fully met the need for testing, and a permanent network of test providers may be required to ensure coverage. This prompts the question of whether cost-shared private provision might provide a sustainable complement to public provision, and whether there is the prospect of a market for arsenic tests.

In this paper, we describe the outcome of a randomized controlled trial conducted from 2012 to 2015 across 26 villages of Bihar, India. In order to elicit demand for a diagnostic test, we offered a field kit measurement for arsenic to well-owner households at prices between Rs. 10 and Rs. 50 (USD 0.20-0.80) that were randomized at the village level. The highest price level (Rs. 50) was slightly less than one day of per capita income in the Bhojpur district in 2011–12 (Rs. 58), or one-third of the full cost of goods and services (COGS) for testing one well in this study.[3] We ran the sales campaign to sell arsenic tests in two phases, with round 1 conducted in 2012-13 and round 2 in 2014–15. To facilitate switching to safe wells, right after round 1, we exhibited local maps at multiple locations in each village in order to indicate household-level exposure to unsafe wells and availability of safe wells nearby. About three months after round 1, a follow-up was carried out to record household responses to the test results in terms of switching to another well. This follow-up enabled us to collect information on the ex-post source of drinking water at the household level since we were able to match about 90% households on their uniquely numbered well ID tags.

Several limitations arising from the study's implementation are worth noting up front. Discussions with the field team after the surveys were completed revealed that during round 1 of the testing campaign, in 2012–13, enumerators did not systematically collect data from about half the households. This led to partial coverage (primarily, in four out of 26 villages) and incomplete recording of data from areas where well owners showed strong reservations about paying for a test. To address this problem, in round 2 survey conducted after two years, we asked households to recall any arsenic test offer and purchase made in 2012-13. This retrospective data provides us an alternate way to estimate the round 1 demand, but it is also subjected to concerns about recall or response bias which seems to be more salient at higher price levels. We, therefore, report the round 1 demand calculated with both approaches, based on current purchases recorded in 2012, and purchases recalled by households in 2014, respectively. This is explained in Section 2.5. Demand estimates based on recorded purchases are less likely to be affected by a recall or response bias. Secondly, we are unable to create a household-level panel linking households across both rounds of test offers because uniquely numbered well ID tags attached during round 1 proved to be far less durable than expected, and could not be comprehensively tracked. It should be noted that we do have a household-level panel for switching outcome since uniquely numbered well ID tags could be tracked for about 86% of households in the follow-up survey conducted three months after round 1.

We find that there is a considerable demand for arsenic testing: at the mean across price groups and *over the entire duration* of our intervention, 47% of households purchased the test. At the same time, however, even at the lowest price level of Rs. 10, demand was pronouncedly lower, at 68%, than the expected full coverage that public provision of arsenic tests usually attains.[4,5] As noted, we report round 1 demand calculated with two different approaches in order to address limitations in the data collection in round 1 to the degree possible. We prefer the more conservative demand-estimate based on recorded sales in the round 1 because the recall bias appears to steepen the slope under the alternative approach (see Section 2.5). The more conservative estimate suggests 3 pp drop in demand for a Rs. 10 increase in price, but it is statistically insignificant (*t-stat* = *1.24*). We repeated the sales offer two years after the initial campaign, at the same (nominal) sales price, and recorded about 18% additional demand.[6] After two rounds of sales offers, the estimated linear demand suggests 7 pp drop in demand for a Rs. 10 increase in price (*t-stat* = *3.6*). The impact of price on demand is non-linear – demand

[3] Daily per capita income is calculated by dividing annual per capita income by 365 days. Per capita income in the Bhojpur district in 2011-12 was about 14% less than the state average (Finance Department of Bihar, 2016). Data are available at http://finance.bih.nic.in/Documents/Reports/Economic-Survey-2016-EN.pdf

[4] Note that we do not have a control arm with fully subsidized test offers in this study, but our recent fieldwork in the same district in Bihar (India), as well as in other areas in Bangladesh, suggest that it is very rare for a household to turn down a test when it is offered for free.

[5] Kremer and Miguel (2007) find that demand for deworming treatment drops by 58 percentage points (pp), when a small fee is introduced. Cohen and Dupas (2010) estimate that a 10% reduction in subsidy reduces take up of ITNs by 60 pp.

[6] Inflation can explain only a small part of the additional demand, as we explain in Section 3.1. We discuss implications of learning and wealth effect for additional demand in Appendix B.

seems to be less sensitive to price at middle price levels.[7]

Our study further investigates how households respond to the information on environmental quality that arsenic tests provide. We use the quasi-experimental variation caused by the spatially stochastic occurrence of arsenic to identify the behavioral responses of households. In the follow-up survey conducted three months after the first wave of test offers, about one-third of households whose wells had unsafe levels of arsenic switched to a safer well for drinking and cooking. This avoidance behavior is in line with previously reported switching rates, although at the lower end of the spectrum (Ahmed et al., 2006; Bennear et al., 2013; Chen et al., 2007; George et al., 2012b; Madajewicz et al., 2007; Opar et al., 2007; Pfaff et al., 2017). This could be in part because the proportion of wells tested remains relatively low when compared to free testing, which limits the switching options for households with an unsafe well. When compared with other studies, our response survey was conducted after a relatively short time period of three months, which might also explain it if the behavioral impact of information increases with time (Balasubramanya et al., 2014). On the other hand, the response to arsenic testing by switching in Bihar is considerably larger than the response (i.e., seeking treatment) in reaction to health information provided by a subsidized diagnostic test for malaria in Kenya (Cohen et al., 2015). We see no strong evidence that the price paid for testing affects the probability of switching to a safer water source, although the confidence intervals for this result are very wide.

In a novel finding, we find clear evidence of selective recall and concealment of test results. About half the households whose wells tested *unsafe* were unable to recall their well status correctly (with only one in six households unable to recall correctly in the case of safe wells). We also document that households actively concealed information on their well's arsenic level when tests revealed their well water to be high in arsenic by discarding arsenic-status placards attached to their wells. Stigma, concerns over reduced property value, or obstacles to switching might explain this choice. We present evidence that wealthier households are more likely to hide adverse information.

The remainder of the paper is structured as follows. Section 2 discusses the details of the experiment, data, and empirical specifications. Results are presented in Section 4, and Section 5 concludes.

2. Details on experiment, data and methodology

2.1. Study setting and sample

Our study is set in a region in the Indo-Gangetic plain in Bihar, India, where arsenic levels are elevated in a significant proportion of drinking water wells. Chakraborti et al. (2003) first documented that a large number of wells in the region showed elevated arsenic levels by extending their testing campaign upstream along the Ganges from the state of West Bengal. Arsenic testing is a new service in the study area: tests are not available in the private market (nor are they elsewhere in South Asia), and while Nickson et al. (2007) report that about 5000 wells have been previously tested in this region, it has not previously been covered by any government-sponsored blanket testing of wells.[8] Within the general study area, we selected the Bhojpur district to conduct our intervention. In this large district (1045 villages are recorded in the census), we selected a study area of four blocks (sub-districts) adjacent to the village where arsenic was first reported in Bihar (Chakraborti et al., 2003). Our final sample consists of 26 villages of moderate size (50-400 households), based on the probability of some arsenic occurrence, as indicated by distance from the river.[9] The household survey during round 2 identifies 4084 well-owner households in total. Our sample consists of households who own a private tube well. We did not collect a count of households who did not own a well, but a comparison of our data with the 2011 census suggests that at least three-quarters of household had their own tube well.[10] For simplicity, we refer well-owner households in our sample as ‘households’.

To elicit demand, we used a simple revealed preference approach, namely making take-it-or-leave-it offers of arsenic tests at a certain price to households in the sample villages. A take-it-or-leave-it offer elicits only a bound on each household's willingness to pay. For instance, if a household agrees to purchase a test at Rs. 30, we can only infer that its willingness to pay was at least Rs. 30. Similarly, rejection only suggests that willingness to pay was less than the asking price.

We randomly assigned each village to one of five price levels at which households were offered arsenic tests for purchase, rising from Rs. 10 to Rs. 50, in increments of ten. It was felt that offering different prices to households *within* a given

[7] To our knowledge, no previous study has estimated the demand curve for diagnostic testing of water source quality with respect to arsenic. A single study of cost-sharing arsenic tests by George et al. (2013) previously reported a comparable demand of 53% for a one-time offer at a single fixed price equivalent to USD 0.28 in Bangladesh, and showed that education and media campaigns increased purchases to over 90%.

[8] Nickson et al. (2007) report arsenic testing of about 5000 wells in six out of 14 sub-districts of our study district. The sub-districts were not identified in the study, and it is therefore not possible to precisely compare the number of wells tested to the number of local wells. However, the share of wells tested was certainly a small fraction of the wells used by 335,000 households, as reported in the 2011 census for the entire study district, when 26% of wells tested unsafe.

[9] The original study design included a sample of 25 villages, i.e., five villages in each of our five price groups. However, enumerators erroneously visited two villages of the same name during initial fieldwork. We included the additional village as the 26th for the rest of the program.

[10] We cross-checked the number of households recorded in our study against 2011 census data for 21 out of 26 villages that could be matched to the census. For these villages, the census shows 4497 households that report using tube well for drinking water, whereas we record 3322 attempted sales in the same 21 villages, that is, 74% of the census population. The discrepancy can be due to two reasons: first, households may be sharing tube wells when they do not own one themselves; and secondly, enumerators and census teams may have had a different understanding of some village boundaries.

village would be seen as violating fairness norms and would deter purchases.[11] We, therefore, chose not to randomize our prices within villages. The highest price (Rs. 50) was chosen based on initial local focus group discussions; it is slightly below the average daily per capita income of Rs. 58 in the Bhojpur district in 2011–12. Revenue from test sales was used to partially cover the enumerators' salaries and travel costs. The cost of the test kits alone was about USD 0.35 per test (about Rs. 21 at January 2014 exchange rates); the COGS for testing, including wages, quality control, and arsenic-status placards amounted to USD 2.26 per test (Rs. 136). Well ID tags intended purely for data collection added an additional USD 0.48 (Rs. 29). The highest price charged, therefore, more than covered the full cost of the test kits and about one-third of the entire COGS. We did not add a treatment arm that would have offered tests free of charge because of a strong expectation that take-up would be near-universal at zero cost. This expectation was based on prior experience in arsenic testing campaigns and was further confirmed when free tests were offered with near-complete take-up in four pilot villages visited for the design of our experiment. It is also in line with broader evidence from the lab (Shampanier et al., 2007) and field experiments (Cohen and Dupas, 2010; Kremer and Miguel, 2007).

2.2. Implementation – testing campaign and surveys

We used the Arsenic Econo-Quick field test kit, a cost-effective and time-saving alternative to laboratory-based testing. Previous laboratory inter-calibrations have shown that the kit correctly determines the status of 96% of wells with respect to the WHO guideline for arsenic in drinking water of 10 μg/l and 88% of wells relative to the higher threshold of 50 μg/l, which was still the Indian standard at the time (van Geen et al., 2014; George et al., 2012c).[12] Testers were locally recruited from among college graduates, and trained prior to the roll-out of the campaign. Testing then proceeded in two rounds. Round 1 of testing was conducted in 2012–13. Approximately three months after testing was completed, a follow-up survey was conducted to record whether households had switched to a new well. In this follow-up, we attempted to interview all households who had purchased a test in round 1 and recorded a switching status for about 90% of the sample. Round 2 was conducted about two years later in 2014–15. In this round, tests were offered again in the sample villages and close to all the households in those villages were surveyed. The timeline of fieldwork is provided in Table 1. For simplicity, we henceforth refer to round 1 of testing as having taken place in 2012, and round 2 in 2014 .

Round 1 of testing began with focus group meetings in each village. To increase awareness of the arsenic issue, a large poster was put on display, showing a satellite image of a pilot village along with color markers indicating the arsenic status of tested wells (Fig. 2). The poster served the additional purpose of making tangible the great spatial variation in arsenic contamination, and the resulting opportunities for well switching. Following the focus group meetings, testers began to offer tests door-to-door; where a sale was made, tests were conducted using a reliable field kit that requires approximately 15 minutes per test (van Geen et al., 2014). In order to minimize any effect of enumerators, village assignment to enumerators was randomized. The protocol foresaw that for all households approached with a test offer, GPS coordinates and basic data on the household would be collected. However, in contrast with what was intended, testers did not record data from many households that did not purchase a test and as our round-2 data suggests, some purchased tests also went unreported. We discuss the resulting challenges for estimation of round 1 demand and how we address the problem in detail in Section 2.5 and Appendix A.

During round 1 of test offers, a total of 1212 tests were sold across the 26 sample villages (Table 4, Column 4). At the time of testing, and during the pre-testing focus group discussions, two arsenic cutoffs were systematically conveyed to the households verbally, explaining the arsenic safe, moderate, and high values with a color code – blue, green, and red, respectively. The results of each test were posted on the pump-head of the well that was tested, with an easy-to-read metal placard ('arsenic-status placard'), color-coded red for unsafe wells (>50 μg/l arsenic), green for 'borderline safe' wells where arsenic is of some concern (> 10 – 50 μg/l), and blue for safe wells (≤10 μg/l) (Fig. 3).The cut-off values correspond with the WHO guideline and Indian national safety standard, respectively. The government of India, unlike the government of Bangladesh, has since matched its standard to the WHO guideline. The choice of arsenic-status placard color and graphic was based on a UNICEF design widely used in Bangladesh (van Geen et al., 2014). Uniquely numbered well ID tags (shown in Fig. 3) were also attached to each pump-head with thin metal wire in anticipation of a future response survey. Regrettably, fewer than 5% of tags placed in 2012 were still attached in 2014.[13] Well ID tags were the only identifier available to us to link well owner households across two survey rounds in 2012 and 2014, which is why we are unable to create a household panel across the two rounds.

Immediately after the first wave of arsenic testing was completed, village-level maps were exhibited in each village, showing the approximate geo-locations of safe, borderline safe, and unsafe wells, with the goal of illustrating, where relevant, that the proximity of safe wells would make well-switching feasible. Geo-locations were jittered on the maps to preserve anonymity. During home visits, households were alerted to the fact that switching from unsafe or borderline safe wells to neighboring safe wells would be an effective way to avoid arsenic exposure. The first phase of the project concluded

[11] This consideration obviated the use of alternate techniques for eliciting willingness to pay, such as the Becker-DeGroot-Marschak (BDM) mechanism and other auction-based methods. In any case, auctions would have been unlikely to be efficient mechanisms, given the potential buyers' uncertain and likely correlated beliefs as to the value of arsenic tests.

[12] For comparison, Rapid Diagnostic Test kits used in Cohen et al. (2015) to test for malaria show an 8% in-field false positive rate.

[13] Anecdotal evidence suggests that most of the well ID tags disappeared due to a lack of durability of the thin metal wire used to fix them to the well. These well ID tags did not provide any information about water quality to households.

Table 1
Fieldwork timeline.

August 2012	Arsenic testing in four pilot villages
November 2012 – February 2013	Round 1 of arsenic testing
February 2013 – May 2013	Follow-up survey of well switching (household panel)
November 2014 – January 2015	Round 2 of arsenic testing

Note: the table shows the time line of main events. Round 1 of testing started in Nov 2012. A follow up survey to collect household's response to the test outcomes was conducted about three months later. Since about 86% uniquely numbered well ID tags remain attached, this helped in creating a household-level panel to analyze switching behavior. However, only few well ID tags survived till round 2 in 2014-15. Hence, we do not have a household panel across the two rounds. We conducted a detailed socio-economic survey in the round 2.

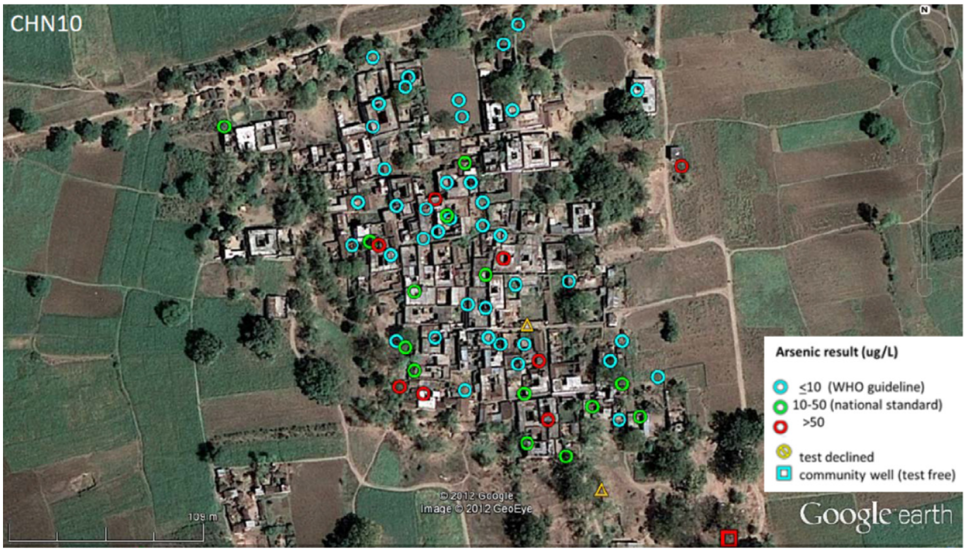

Fig. 1. Example of well arsenic distribution in a village in Bhojpur district, Bihar (India).
Note: a sample village map from the study is shown with the outcomes of arsenic testing after round 1. Red circles denote drinking water wells that are highly contaminated with arsenic; green circles show wells with intermediate arsenic levels; blue circles show wells that are low in arsenic and safe to drink from. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

Fig. 2. Satellite maps from nearby villages were shown in focus group meetings.
Note: village meetings and exhibition of posters showing safe and unsafe wells from nearby villages. The geo-location of wells were jittered because of privacy concerns. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

with a follow-up visit conducted approximately three months after testing was completed. Enumerators visited all households who had purchased the test and collected information on their current source of water for drinking and cooking purposes.

Fig. 3. Arsenic-status Placards.
Note: red (Arsenic high), green (Arsenic moderate) and blue (Arsenic low) placards were fixed on the tube wells after arsenic testing. The text in Hindi means "Arsenic-safe water" (on Blue and Green placards) and "Arsenic-contaminated water" (on Red placards). Visual display with a *cup in hand* are designed to communicate that high arsenic wells must be avoided for drinking water, while water from moderate arsenic wells should be used in a limited way. Enumerators also explained these labels verbally after each test. This choice of placard color and graphic was based on a UNICEF design widely used for arsenic testing in Bangladesh. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

In our sample, about half the wells are not visible from the outside. However, well sharing is readily possible since houses are close to each other, and people interact on a regular basis in a small village economy, even if ownership of these wells is well-defined. There is a small number of community/public wells in about half the villages (no more than one or two wells at a maximum), for example, wells within the premises of a temple or school. We tested all these wells for free, and if people switched to a safe community well in response to high arsenic outcome in their private wells, it was captured in our data collection. Since arsenic occurrence is spatially stochastic, it is unlikely that a household could successfully predict the status of its well by looking at nearby test results.

In a second round commencing in 2014, some two years after the initial visits, we offered the tests again in the same set of villages and at the original nominal price.[14] Across the 26 villages, a total of 4084 households were approached with the intention of making a sales offer (Table 4, Column 2). In round 2, data were collected systematically from every household where a respondent could be interviewed, including from households that did not wish to buy the tests. Each house was visited at least twice to ensure high coverage. After two visits, about 14% of households could not be surveyed because no adult member was present or willing to answer questions; sales offers could be completed in 3528 households. The enumerators reported that, to avoid embarrassment, some households who were unwilling to purchase tests at the asking price avoided being interviewed. For a conservative demand estimate, we, therefore, work throughout with the number of households approached for sales, rather than the number of households where a sales offer could be completed. A total of 719 tests were sold in round 2 Table 4 (Column 8). The household survey administered in round 2 gathered socio-economic and demographic information, along with GPS coordinates of the wells. However, the precision of GPS coordinates is insufficient to link households unambiguously across the two rounds of offers. The survey also collected information on the recall of tests offered and purchased in 2012. The recall data allow us to carry out balance checks and additional analysis with the socio-economic data collected in the round 2 survey, which would otherwise not be possible due to implementation issues during the round 1 offers. Finally, survey team collected information on 2012 arsenic-status placards still mounted on tube wells. In total, during the round 2 survey, we found 492 placards out of 1212 tests conducted in round 1, either still mounted on the well or kept in the house.

2.3. *Summary statistics*

Summary statistics from the 2014 survey show modestly well-off village communities (Table 2). Households are of moderate size (3.9 members on average). Most (89%) own at least one mobile phone, and most (70%) live in houses made from durable building materials ('pucca'). Ownership of bikes (68%) and cows (67%) is common, though fewer households own consumer durables or have access to sanitation, and very few own cars.

[14] Considering inflation in rural Bihar during this period, the lowest price of Rs. 10 and the highest price of Rs. 50 during round 2 would be equivalent to Rs. 8 and Rs. 41, respectively, in the round 1. As we argue in Appendix B, this may partly explain additional demand at the time of the repeat offer.

Table 2
Summary statistics and randomization balance.

	Number of well-owner households	Household members			Asset ownership									
		Adults	Infants	Children	Asset Index	Pucca	Has Latrine	Cow	Whitegood	Cell	TV	Bike	Motorbike	Car
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Panel A: Continuous price														
Price	−2.215*	0.00120	0.00401	0.000543	0.00193	4.29e-05	0.00729***	0.00117	−0.000568	0.00177	−0.000995	−0.00247	0.00197*	−0.000252
	(1.139)	(0.0197)	(0.00298)	(0.00632)	(0.00549)	(0.00285)	(0.00231)	(0.00188)	(0.00224)	(0.00143)	(0.00227)	(0.00199)	(0.00104)	(0.000424)
Constant	222.7***	3.861***	0.215***	0.549***	−0.0514	0.699***	0.132*	0.634***	0.230***	0.839***	0.230***	0.742***	0.161***	0.0353**
	(46.22)	(0.544)	(0.0783)	(0.187)	(0.166)	(0.106)	(0.0696)	(0.0657)	(0.0684)	(0.0496)	(0.0691)	(0.0432)	(0.0401)	(0.0137)
R-squared	0.128	0.000	0.004	0.000	0.001	0.000	0.044	0.001	0.000	0.006	0.001	0.005	0.004	0.000
Panel B: High price														
High Price (> = Rs.40)	−59.11**	0.273	0.108	0.0481	0.0344	0.00562	0.233***	− 0.0126	−0.0267	0.0412	−0.0401	−0.0779	0.0653**	−0.00722
	(26.75)	(0.567)	(0.0975)	(0.169)	(0.202)	(0.0840)	(0.0642)	(0.0491)	(0.0795)	(0.0435)	(0.0732)	(0.0749)	(0.0295)	(0.00999)
Constant	179.8***	3.814***	0.291***	0.550***	−0.00993	0.699***	0.258***	0.669***	0.223***	0.874***	0.215***	0.699***	0.194***	0.0307***
	(24.85)	(0.290)	(0.0475)	(0.0940)	(0.102)	(0.0639)	(0.0358)	(0.0374)	(0.0428)	(0.0337)	(0.0412)	(0.0313)	(0.0229)	(0.00672)
R-squared	0.110	0.003	0.002	0.001	0.000	0.000	0.051	0.000	0.001	0.003	0.002	0.006	0.005	0.000
Panel C: Price levels														
Price = Rs. 20	−6.800	0.678	0.0830	0.238	−0.104	−0.227	−0.0667	− 0.00546	0.0262	0.0124	0.0308	−0.0277	−0.0515	−0.0110
	(77.68)	(0.673)	(0.0992)	(0.233)	(0.240)	(0.142)	(0.0895)	(0.0903)	(0.109)	(0.0804)	(0.103)	(0.0564)	(0.0563)	(0.0189)
Price = Rs. 30	−33.40	−0.729	0.0618	−0.134	0.0444	−0.0372	0.0257	0.125	0.0112	0.0532	0.0214	−0.0469	0.00206	−0.0127
	(62.04)	(0.580)	(0.149)	(0.217)	(0.297)	(0.0994)	(0.119)	(0.0819)	(0.116)	(0.0707)	(0.114)	(0.106)	(0.0411)	(0.0175)
Price = Rs. 40	−61.60	0.268	0.141	0.0633	−0.0582	−0.142	0.166	0.00104	−0.0180	0.0623	−0.00814	−0.137	0.0297	−0.0276**
	(58.03)	(0.696)	(0.114)	(0.254)	(0.334)	(0.0989)	(0.111)	(0.0817)	(0.141)	(0.0612)	(0.137)	(0.135)	(0.0379)	(0.0141)
Price = Rs. 50	−82.60	0.439	0.176	0.157	0.0818	−0.0304	0.270**	0.0387	−0.00802	0.0576	−0.0392	−0.0583	0.0644	0.000127
	(60.42)	(1.023)	(0.160)	(0.300)	(0.218)	(0.104)	(0.114)	(0.0970)	(0.106)	(0.0668)	(0.0812)	(0.0766)	(0.0461)	(0.0221)
Mean at Price = Rs. 10 (Constant)	192.8***	3.741	0.242	0.492	0.018	0.795	0.278	0.638	0.209	0.855	0.198	0.722	0.214	0.038
Mean across price groups	157.1	3.893	0.322	0.564	0.000	0.700	0.326	0.665	0.215	0.885	0.204	0.676	0.213	0.0286
R-squared	0.132	0.040	0.004	0.019	0.004	0.040	0.059	0.011	0.001	0.007	0.003	0.009	0.009	0.003
Joint significance														
Wald chi2(df)	4.404	4.766	2.592	3.060	0.776	3.929	17.17	4.613	0.130	1.458	0.761	1.446	4.883	9.929
Prob > chi2	0.354	0.312	0.628	0.548	0.942	0.416	0.00179	0.329	0.998	0.834	0.944	0.836	0.300	0.0416
Observations	26	3526	3528	3522	3229	3758	3528	3527	3528	3528	3528	3528	3528	3527

Note: the table shows overall mean values of key demographic and asset variables observed in 2014-2015 survey (round 2), alongside regression results showing correlation with (Panel A) continuous price variable, (Panel B) high price indicator variable, and (Panel C) differences in means across price groups. 'Pucca' denotes concrete houses. Asset index is created with house characteristics and asset ownership information using standard principal components approach (Filmer and Pritchett, 2001). A test for joint significance of the price dummies is reported in the bottom rows. Cluster bootstrap standard errors in parentheses (400 replications). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3
External validity: characteristics of sample villages compared to census block, district, and state means.

	Housing characteristics		Asset ownership					Household characteristics		
	'Pucca' (1)	Latrine (2)	Cell phone (3)	Bike (4)	Motorbike (5)	Car (6)	TV (7)	Scheduled caste (8)	Literate (9)	Employed (10)
Sample villages	0.659	0.203	0.617	0.592	0.119	0.0174	0.224	0.168	0.611	0.328
Panel A										
Census blocks where villages are situated	0.598	0.258	0.594	0.525	0.113	0.0182	0.224	0.154	0.589	0.298
Difference	−0.061	0.0547*	−0.0226	−0.0671*	−0.00658	0.00082	−0.000193	−0.0138	−0.0218	−0.0304
Panel B										
Bhojpur district	0.627	0.224	0.598	0.509	0.101	0.0184	0.182	0.162	0.583	0.31
Difference	−0.0315	0.0205	−0.0187	−0.0831**	−0.0184	0.000976	−0.0426	−0.00583	−0.0276	−0.0188
Panel C										
Bihar	0.461	0.19	0.517	0.496	0.0773	0.0161	0.128	0.179	0.505	0.343
Difference	−0.197***	−0.0132	−0.0993*	−0.0957**	−0.0421***	−0.00136	−0.0962***	0.0116	−0.106***	0.0141

Note: the table shows mean values of key demographic and asset variables observed in the 2011 Census, for 21 out of 26 sample villages that could be matched with the census, the four census blocks that nest these villages (Panel A), and the district (Panel B) and state (Panel C) where they are all located. 'Pucca' denotes concrete houses. Mean values are shown for each group, alongside the difference between the mean for the respective group and the mean for our sample villages. Significance of differences obtained from robust standard errors (omitted for readability); *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2 also shows a randomization check on observables. We calculate a normalized asset index with house characteristics and assets information, using a standard principal components approach (Filmer and Pritchett, 2001); estimated coefficients are provided in Column 5. As Table 2 shows, price category dummies are jointly significant at the 90% level for two of the eleven variables tested. The two instances where there are significant differences (ownership of cars and access to sanitation) appear isolated and suggest opposite signs in a relationship between price and ownership. There is, therefore, no indication that the price groups in question are systematically any more or less wealthy than the other groups. Note that, as shown in Column 1, the total number of well-owning households varies significantly across price groups, with larger villages in the low-price groups. However, Table 2 also demonstrates that other demographic characteristics and asset ownership were similar across villages in different price groups. We also find no correlation between mean asset index and village size in additional tests.

During the follow-up on switching after round 1, our field team could not contact about 14% of the households that had purchased the test in round 1. Appendix Table C1 tests for attrition balance in the follow up survey. The difference between the follow-up and attritted sample is statistically insignificant in general, although the direction of difference is consistent and indicates that households that could not be reached in the follow-up round may be economically weaker and may have made a relatively recent investment in a well.

To give a sense of the external validity of our results, Table 3 compares household wealth proxies in the 2011 census for our sample villages, the four blocks that nest them, the Bhojpur district, and the state of Bihar. As is evident, households in our sample villages are similarly well-off to the mean household in the blocks (Panel A) and the Bhojpur district (Panel B). They are, however, better off than the average household in Bihar, with a far higher share of houses made from durable materials, greater literacy, and ownership of household assets up to 10 pp higher for many categories (Panel C). While we show below (Table 6) that purchase decision at high price levels does not correlate with assets, we might expect demand in our sample villages to be representative of Bhojpur district, but perhaps higher than in Bihar at large.

2.4. Empirical specification

We have two sources of exogenous variation in this study: (1) experimental variation in prices, and (2) quasi-experimental variation in arsenic occurrence. The village-level price variation is used to estimate the demand, and the household-level arsenic occurrence is utilized to analyze the behavioral response to the information revealed by tests. Throughout this paper, we analyze data using Ordinary Least Squares (OLS).

We estimate the demand for arsenic testing with the following three specifications, using (1) a continuous price variable (linear demand model); (2) an indicator variable for high price; and (3) a set of indicator variables for each price level, respectively (Eqs. (1)–(3)).

$$Purchase_{iv} = \beta_0 + \beta_1 price_v + \epsilon_{iv} \tag{1}$$

$$Purchase_{iv} = \beta_0 + \beta_1 \mathbb{1}(price_v \geq 40) + \epsilon_{iv} \tag{2}$$

$$Purchase_{iv} = \beta_0 + \alpha_p + \epsilon_{iv} \tag{3}$$

Here, $Purchase_{iv}$ is a binary variable showing whether household i in village v purchased the test, when offered at a price p_v ($p \in P\{Rs.10, Rs.20, Rs.30, Rs.40, Rs.50\}$). *price* denotes a continuous price variable, while $\mathbb{1}(price_v \geq 40)$ and $\boldsymbol{\alpha_p}$ represents a high price dummy and a set of price-level dummy variables, respectively. We include a dummy indicator for 'high price' based on top two price levels, and use the alternate definition (i.e., based on top three price levels) as a robustness check. The coefficient on the price variable is our estimate of price sensitivity of demand. ϵ_{iv} is the error term.

Next, we estimate a model of avoidance behavior, where the binary outcome variable $Switched_{iv}$ shows whether a household i in village v switched to a safe well or not. $TestOutcome_{iv}$ shows the arsenic status of the well owned by the respondent households. The coefficient on the $TestOutcome_{iv}$ provides an estimate of the effect of information provided by the diagnostic test on the probability of switching.

$$Switched_{iv} = \beta_0 + \beta_1 \mathbb{1}(TestOutcome_{iv} = HighArsenic) + \epsilon_{iv} \tag{4}$$

Using a similar specification with a price and asset index interaction term, we test whether behavioral response depends on the price paid to obtain the information, that is, whether switching is correlated to the price paid by households.

Our final investigation concerns concealment, and selective recall of test outcomes, whereby households failed to retain the physical marker displaying arsenic test outcome or failed to recall the test result correctly. Since we cannot link households across the first and second survey rounds, we pool the two cross sections. We construct indicator variables representing each of the three possible test outcomes (i.e. placard colors) by pooling information from the two rounds, in the following fashion. This indicator variable is defined by recorded test outcomes in round 1 and recorded/recalled round 1 test outcomes during the round 2 survey. For round 1, the indicators reflect the color of the arsenic-status placard (Red/Green/Blue) affixed to the well by enumerators after arsenic testing in round 1. For round 2, it represents the color of arsenic-status placards available physically on the well or in the house, or the household's recall of placard color when the placard could not be located during the round 2 survey. Specifically, we create three sub-samples with round 2 data consisting of observations on: (a) an arsenic-status placard still mounted on the well, (b) a placard available in the house, or (c) a household's recall of placard color, and pool each subsample with round 1 data. We then regress the binary indicator of arsenic-status placard color (Red, Green, or Blue) on an indicator for observations made in round 2 for each sub-sample. Finally, we combine all sub-samples to conduct a similar analysis. In simpler terms, this regression is equivalent to a t-test on whether there is an equal proportion of tests with each of the three possible outcomes in two groups, namely (1) as tested in round 1, and (2) as reflected in physical evidence or as reported by the households in round 2.

$$TestOutcome_i = \beta_0 + \beta_1 Round2_i + \epsilon_i \tag{5}$$

The above specification describes three separate regressions where the outcome is a dummy variable for TestOutcome = High (i.e. red), TestOutcome = Moderate (i.e. green), and TestOutcome = Safe (i.e. blue), respectively. β_1 denotes the change in the proportion of a particular test outcome from round 1 to round 2. In a similar specification, we use an interaction of $Round2_i$ with asset ownership to test whether concealment and selective recall of test outcome are correlated with asset ownership.

In all regressions, we report cluster bootstrapped standard errors to account for randomization at the village level. For estimated coefficients in the demand equations, we also calculate wild bootstrap-t p-values as a robustness check (Cameron et al., 2008).

2.5. Bias in the round 1 demand estimation due to incomplete data

During the first round of sales offers made in 2012, enumerators did not systematically collect data from all households. During the first round of sales offers made in 2012, enumerators did not systematically collect data from all households (Appendix A). This is evident from a comparison of Columns 2 and 3 in Table 4. First, enumerators did not offer tests in some areas where people showed strong reservations about being charged for tests during focus group meetings. Secondly, the enumerators could not identify the boundaries of a few villages and may have omitted parts of these villages during the round 1 testing campaign. Finally, in a few villages, a number of tests sold were not recorded in round 1, as recovered arsenic-status placards suggest. All these factors could bias the demand estimate. If the number of offers is under-reported for any reason, the demand will be overestimated. On the other hand, when we use the full sample of households covered in 2014 survey as the offer pool in round 1, the aggregate demand estimate would potentially be lower than the true demand. More importantly, the estimated slope of the demand curve may be more sensitive to a bias which varies with price.

We use two different approaches to estimate the round 1 demand. First, we use recorded sales in 2012 over the full sample of households covered in the 2014 survey (approach 1). Secondly, we use the recall of round 1 offers and purchases, as collected during the round 2 survey (approach 2). Each set of estimates is likely to have flaws.

Comparing the two approaches (Table 4, Column 11 and 12) shows that the estimates obtained are broadly aligned in the aggregate demand (30% and 27%, respectively). As a corollary, there is a good match between the ratio of *recalled* 2012 sales to *recorded* 2012 sales (0.65) on the one hand, and the ratio between *recalled* 2012 offers and *recorded* 2014 sample size on the other (0.60).[15]

[15] It would be a potential concern if our demand estimates with approach 2 are biased by a differing impact of adverse test outcomes on test purchase recalls. However, note that we find little correlation between offered price and high arsenic outcomes (Column 2, Table C5). Moreover, a lower recall of high-arsenic wells affects only a small share of the total number of wells, and is also almost fully compensated by a higher recall of moderate-arsenic wells (Table 8).

However, it is important to note that the demand *slope* may still be sensitive to any correlation between the probability of recall or response with the price. Since the ratio of recorded to recalled demand diverges at higher prices (Column 7), we cannot rule out that there is such a bias in purchase recall that changes monotonically with price. This would lead the recall-based demand curve to be steeper than the true demand. Consistent with this notion, Columns 11 and 12 show that the demand calculated with recall data at two higher price levels is lower than the corresponding demand calculated with approach 1. Thus, our recall-based estimate for the round 1 demand slope may be biased toward showing higher price sensitivity.

This brings us back to approach 1, which is unlikely to be contaminated by concerns about recall/response bias. We, therefore, use approach 1 as the preferred approach to estimating the round 1 demand curve, as well as the cumulative demand after round 2. In the next section, Fig. 5 provides a comparison of both approaches, showing that approach 1 provides a more conservative estimate of demand slope. Note that, while we prefer approach 1 to estimate round 1 demand, the recall-based demand estimate still shows a reasonable level of internal consistency, which allows us to use the more detailed household-level data collected during the round 2 survey for additional analysis.

We present additional analysis and robustness checks concerning the data and implementation issues in Appendix A.

3. Results

3.1. Demand for well arsenic testing

Demand for fee-based arsenic tests in the study area is substantial. Overall, a total of 1857 tests, corresponding to 47% of approached households, were sold at randomly assigned prices across the 26 sample villages over the entire duration of the program (Table 4, Column 15).[16] An example of test results in one village is provided in Fig. 1; a map displaying the proportion of safe, unsafe, and untested wells in each village is shown in Fig. 4. In total, using the national and WHO thresholds of 50 and 10 μg/l, respectively, 50% of wells tested 'safe' ('blue'), 31% tested 'borderline safe', and 19% tested 'unsafe' ('red'). As expected, test results varied over small distances, and there is a wide spread in the shares of unsafe wells across villages, ranging from 2% to 77% (Appendix D). The expected health burden resulting from this distribution of arsenic contamination is significant. The 60th percentile of the arsenic distribution in our sample corresponds to 10 μg/l. In a decade-long cohort study in Bangladesh, 21% of all-cause deaths were attributed to chronic exposure by drinking water at arsenic levels above 10 μg/l in (Argos et al., 2010).

As noted earlier, demand estimation for the round 1 testing is complicated by incomplete data collection. In the following, we work with both approaches described in Section 2.5, namely, round 1 demand estimated with (1) sales in 2012 and sample size in 2014; and (2) with the recall data systematically collected during the second test wave to determine 2012 demand. Demand in the round 1 of sales alone was 30% across price groups in our preferred estimate based on recorded sales (Table 4, Column 11). Demand at the time of the second offer was 18%, after adjusting for repeat purchases (Column 13). While round 1 demand estimated with the recall data is about 3 pp lower (Column 12), it yields about two-fold steeper demand curve due to likely recall/response bias at higher price levels (Column 7).

At the lowest price of Rs. 10 (USD 0.15 at market rates at the time of the repeat offer), 38% of households purchased the test after one offer, and 68% after two offers (Table 4, Columns 11 and 15). While our experiment did not include an arm with a zero price offer, uptake of free tests can be assumed to be nearly 100% given households' tendency to conceal test does not affect take up of free tests (as discussed in Section 2.1). Thus, while there is significant demand at Rs. 10, charging this small amount, rather than offering the test for free, reduces coverage after two sales offers by about one third. Demand further drops precipitously at higher prices, and at Rs. 50, it reduces to less than one-quarter of households after one offer, and less than one third after two offers.

Table 5 presents estimated coefficients for linear demand. The estimated slope in round 1 suggests a 3 pp decline in demand with a Rs. 10 increase in price, but is statistically insignificant (Column 1). The linear demand slope estimated with recalled purchases is steeper (Column 2). Estimated coefficients on the high-price indicator also suggest that demand drops as price increases.[17] Column 3 shows the linear-demand estimates for round 2 which suggest a 5 pp decrease in demand with a Rs. 10 increase in price. Estimates for total coverage after two rounds are shown in Columns 4 and 5 using recorded and recalled purchases, respectively. With our preferred approach using recorded purchases in round 1, the slope estimate in Column 4 suggests that total coverage drops by 7 pp for a Rs. 10 increase in price.

We further estimate a non-linear demand curve with indicator variables for each of the five price levels offered (Fig. 5, Table C2). This analysis is constrained by limited power due to a small number of villages under each price bin. Estimated coefficients have the expected negative signs, but we are unable to reject equivalence in all cases. Fig. 5 also shows recall-based demand curves for round 1 and the corresponding total coverage after both rounds. Demand curves estimated with

[16] To estimate total coverage after two offers, we add demand in rounds 1 and 2, correcting for repeat purchases. We define round 2 purchases to have been repeat purchases in 74 instances, where households recall having bought the test in 2012 and purchased another test in 2014. Households had been advised that, since arsenic levels in ground water are stable over time, wells need not be tested repeatedly. In case of repeat purchase, only the round 2 purchase is taken into account to calculate total coverage after two rounds.

[17] Our findings are robust as to coding the binary high price variable with the top three prices levels instead of two (Table C3).

Table 4
Test offers, sales, and demand.

Price (Rs.)	*Full Sample 2014*	2012 offers and sales					2014 offers and sales			Demand estimates					
		Recorded offers	Recorded sales	Recalled offers	Recalled purchase	Ratio of recalled sales and recorded sales	Sales	Sales among those who recall being offered a test in 2012	Sales among those who recall buying a test in 2012	2012 demand (recorded)	2012 demand (recall)	2014 demand	2014 demand given 2012 offer (recall)	Total coverage [Cumulative of 2014 demand and 2012 demand (recorded)]	Total coverage [Cumulative of 2014 demand and 2012 demand (recall)]
p	*N*	o1	s1	o2	s2	s2/s1	s3	s4	s5	s1/N	s2/N	s3/N	s4/o2	(s3+s1)/N	[s3+N*(s2-s5)/o2)]/N
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
10	960	431	361	615	249	0.69	288	187	11	0.38	0.40	0.30	0.30	0.68	0.69
20	1105	423	310	804	206	0.66	183	135	1	0.28	0.26	0.17	0.17	0.45	0.42
30	815	352	218	460	125	0.57	117	74	17	0.27	0.27	0.14	0.16	0.41	0.38
40	653	327	196	441	92	0.47	86	72	41	0.30	0.21	0.13	0.16	0.43	0.25
50	551	289	127	350	52	0.41	45	34	4	0.23	0.15	0.08	0.10	0.31	0.22
All	4084	1822	1212	2670	724	0.60	719	502	74	0.30	0.27	0.18	0.19	0.47	0.42

Note: the table summarizes the number of offers and sales in both rounds, and calculated demand in 26 villages. Column 1 shows the offer price, which was kept constant for each village in both rounds (nominally). Column 2 shows the number of households estimated in the round 2 survey in each price group. Column 3–4 and Column 5–6 show number of offers and sales by price-group, based on recorded data or recall, respectively. Column 7 shows the ratio of recalled vs. recorded sales. Columns 8-10 show sales among households in general and among households who could recall being offered or purchasing the test in 2012. Column 10 shows 74 'repeat' purchases, i.e. 74 well-owner households purchased a test in 2014 even though they recall purchasing the test in 2012. We report round 1 demand using both approaches – recorded sales in 2012 over sample size in 2014 (Column 11) and recalled sales and offer from 2012 (Column 12). Round 2 demand is shown in Column 13 and 14. Column 15 and 16 show total coverage after two rounds. Column 16 requires scaling the recall-based demand to sample size N. In order to calculate total coverage, repeat purchases are adjusted as 'no purchase' in the recall data to avoid double-counting.

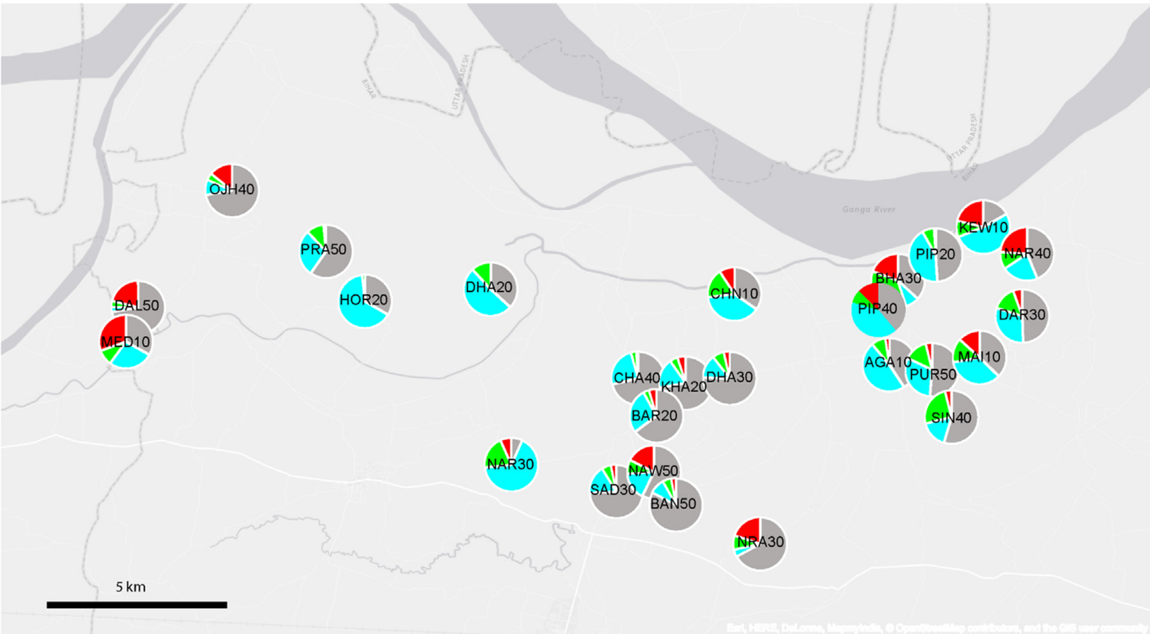

Fig. 4. Map showing village locations with the arsenic test outcomes. (See online appendix for village-level maps)
Note: the map shows the location of villages, take-up and outcome of the arsenic testing in subject area. Red (Arsenic high), Green (Arsenic moderate) and Blue (Arsenic safe) colors show the outcome of arsenic testing in round 1. Grey color shows the proportion of untested wells. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

round 1 purchase recall are steeper, but are also subject to concerns about recall/response bias, as we have discussed earlier. Demand seems to be more sensitive to price at lower price levels and mostly flattens out at higher prices. Results are not markedly different if four villages that observed significant implementation and data collection issues in round 1 are dropped from the analysis (Appendix A).

Our demand results, with the exception of round 1 demand, are broadly in line with the recent studies on preventive health products, such as ITNs or rubber shoes, in developing countries (Cohen and Dupas, 2010; Dupas, 2014b; Kremer and Miguel, 2007; Meredith et al., 2013). Arsenic tests are arguably less well-known to consumers than products studied elsewhere. Perhaps the most natural comparison in terms of the nature of products offered is to Berry et al. (2012), who study willingness to pay for water filters to remove pathogens in northern Ghana. Berry et al. report that, while 95% of respondents had non-zero willingness to pay (an analog of near-universal take-up at zero cost), charging a price equivalent to 116% of daily income (or 30% of the filter's cost) reduced demand to 21%.[18] In our experiment at a price of Rs. 50 and after two sales offers: we observe a demand of 31% at a price equivalent to 111% of average daily income and 30% of the full cost of goods and services.

Our purpose in re-offering the arsenic test was to assess whether additional demand (i.e. from households who did not purchase in round 1) could be elicited. We repeated the offer *at the same nominal price charged initially*, as opposed to repeating it at a *uniform* price as in Dupas (2014b). This allowed us to study the (reduced-form) effect of making a repeat offer at different price levels, a question of immediate policy interest. We found that repeating the offer after a two-year delay did indeed generate substantial additional demand. Thus, purchases at the time of the second offer raised total coverage by some 17 pp, from 30% to 47% (Table 4, Columns 11 and 15). Round 2 demand is also more price-sensitive than the demand at the first offer, estimated with approach 1 (Fig. 5). However, we observed an effect of repeating the sales offer on coverage at any price level, with increases ranging from 80% of the original sales at Rs. 10 to 35% at Rs. 50 (using our preferred estimate). The per capita real income in Bihar rose at a rate of about 10% per year between 2012 and 2014, and thus the 2014 prices were lower in real terms. However, real price difference alone does not seem sufficient to explain additional demand. For example, if inflation reduced the real price of a Rs. 30 test to Rs. 24 after two years, one would expect three percent additional demand for a Rs. 6 drop in price (considering the higher slope value estimated with recall-based demand), whereas we found about additional 14% demand for Rs. 30 price group in round 2. We provide a detailed discussion of the choice of keeping nominal price constant and two potential channels explaining additional demand in Appendix B.

[18] Demand figures from Dupas (2014a). Figures are not directly reported in Berry et al. (2012). Share of income is based on USD 4.20 (GHS 3) price and 2010 (current) per capita GDP of USD 1323.

Table 5
Estimated demand.

	Round 1 demand				Round 2 demand		Total coverage after two rounds			
	Recorded purchase-based	[WB p-val]	Recalled purchase-based	[WB p-val]	demand	[WB p-val]	Round 1 and recorded sales	[WB p-val]	Round 2 and recalled sales	[WB p-val]
	(1)		(2)		(3)		(4)		(5)	
Panel A: Continuous price										
price	−0.00277		−0.00551*		−0.00485***		−0.00712***		−0.0115***	
	(0.00224)	[0.255]	(0.00305)	[0.135]	(0.00155)	[0.030]	(0.00196)	[0.005]	(0.00166)	[0.005]
Constant	0.371***		0.418***		0.307***		0.660***		0.727***	
	(0.0816)		(0.116)		(0.0559)		(0.0700)		(0.0602)	
R-squared	0.007		0.028		0.029		0.037		0.098	
Panel B: High price dummy										
Price > = Rs.40	−0.0404		−0.127*		−0.0954**		−0.127*		−0.263***	
	(0.0523)	[0.425]	(0.0676)	[0.065]	(0.0388)	[0.020]	(0.0721)	[0.091]	(0.0446)	[0.005]
Constant	0.309***		0.309***		0.204***		0.506***		0.497***	
	(0.0384)		(0.0552)		(0.0326)		(0.0513)		(0.0435)	
R-squared	0.002		0.017		0.013		0.014		0.059	
Observations	4084		2666		4084 0.050		4084		4084 0.112	
Mean across Price groups	0.297		0.271		0.176		0.468		0.419	

Note: the table shows estimated demand coefficients. We use two different specification of price (Panel A) continuous price variable and (Panel B) high price dummy. The demand for round1 is estimated with two approaches – (1) using recorded purchase in 2012 over 2014 sample, and (2) using recall data on purchase/offer in 2012 (as collected in 2014). Total coverage after round 2 is also calculated in two ways, using either estimate of the round 1 demand. Note that, there are 74 repeat purchases in our data, which are accounted only once before calculating total coverage. Cluster bootstrap standard errors (based on 400 replications) in parentheses. Wild bootstrap p-values [WB p-val] are shown next to the estimated coefficient (Cameron et al., 2008).
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

3.1.1. No buyer selection at different price levels

We test whether wealthier households are more likely to purchase the test at higher prices by regressing purchase decision on a set of interactions of price and asset index. Table 6 shows that, independently of the asking price, wealthier households were more likely to buy. However, the interaction terms between the continuous price variable and asset index are statistically insignificant and small in magnitude (Column 1): a two standard-deviation increase in the asset index attenuates the main effect of price on demand by only about one tenth. We find consistent results when using a high price dummy (Column 2) or with the non-linear specification using indicator variables for each price level (Column 3). Hence, purchase decisions at a higher price did not correlate with wealth. In all three specifications, the coefficient on the interaction term is not only not significant, but also small in magnitude. For instance, in Column 1, even at 95% of the asset index distribution, the magnitude of the estimated interaction term would be less than 10% of the price effect. Table C4 complements this analysis by showing the correlation between purchase decisions and household characteristics. The absence of a wealth pattern suggests that either purchasing decisions were driven by a different valuation of the product among similar households, or that marginal utility of consumption differed in ways that do not correlate with characteristics we observe.

3.1.2. No residential sorting

We test whether households can predict arsenic contamination, and, potentially, sort accordingly in choosing their residence. As noted, the distribution of arsenic in groundwater wells is hard to predict; it would be surprising if we were to observe sorting. Appendix Table C5 confirms this notion, in keeping with findings in Madajewicz et al. (2007). There is no relationship between well characteristics (age, depth, and cost) and the probability of high contamination, that is, households do not appear to specify well design to effectively avoid arsenic (Column 1). Nor is there a distinct relationship between asset ownership and arsenic status of wells that would suggest residential sorting (Column 3 and 4). We also show that there is little correlation between test price and well quality (Column 2).[19]

[19] Given the small number of high-arsenic wells, tests are run separately for each asset category to avoid over-fitting (Column 4). Due to multiple hypothesis testing, the standard errors reported in Appendix Table C5 are too small. We omit any adjustment because the absence of sorting emerges even when precision is overstated.

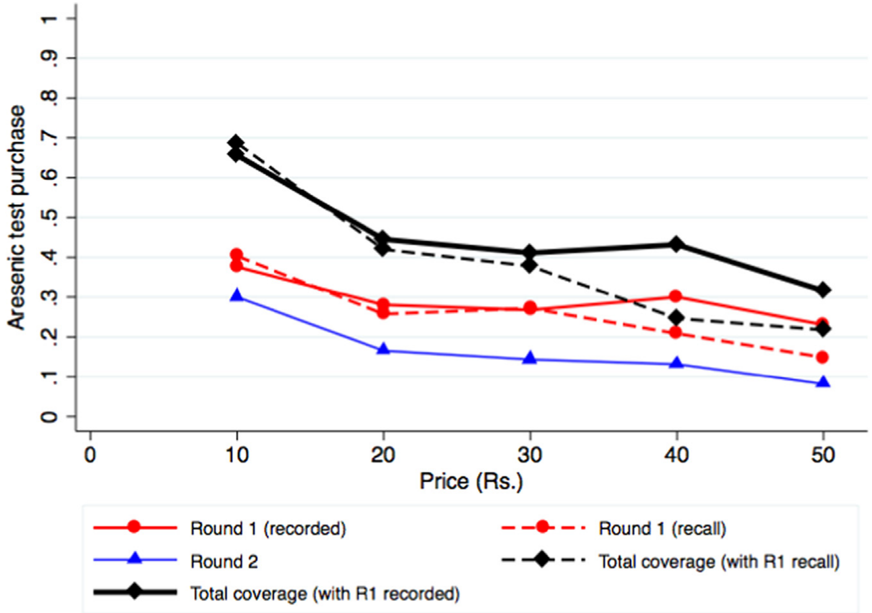

Fig. 5. Demand curves after one and two sales offers.
Note: the plot shows coefficients estimated for non-linear demand specification for two rounds as well as the total coverage after both rounds (see Table C2 for more details). Red line denotes the round 1 demand, blue denotes the round 2 demand and black line denotes total coverage after two rounds. Solid and dotted lines denote recorded and recalled purchases, respectively. The gap between two versions of total coverage seems to be wider than the gap between the two versions of round 1 demand due to correction required for 'repeat' purchases. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

3.2. Behavioral response to arsenic content information: well switching

We next consider how households use the information revealed by arsenic testing, leveraging the quasi-experimental variation induced in the type of information revealed by the spatially stochastic arsenic occurrence. Particular importance attaches to whether households switch from highly contaminated wells to safe water sources. Within the context of the wider literature on preventive health products, this can be viewed as equivalent to behavioral issues surrounding the use of information. Thus, it is the act of switching to a safe water source that brings about health benefits after the purchase of a test – and switching imposes further inconvenience costs. Similarly, after the purchase of an ITN or a drinking water filter, it is the act of sleeping under the net or filtering water that generates health benefits, and each may be associated with inconvenience to a degree specific to the particular context.

Among households which purchased the test in 2012, high-arsenic households reported 30.5% higher switching to a safer drinking water well, when compared with 0.3% baseline switching among households whose well turned out to be safe.[20] Table 7 estimates the behavioral response to the information provided by arsenic testing in terms of switching from high-arsenic wells (red) to other safe (blue) or moderately contaminated (green) wells. Column 1 shows that 24% of households whose wells tested high or moderate in arsenic switched to a safe well; 28% of well owners switched when we only consider high-arsenic wells. The switching rate from moderate arsenic to safe wells is thus lower than the switching rate from high-arsenic to safe wells, suggesting that the behavioral response to information depends on the level of contamination, as observed in Madajewicz et al. (2007). Columns 3 and 4 show estimates for switching to a well which is either safe or contains only a moderate level of arsenic .

Overall, a switching rate 30% is on the lower side, but not an atypical response. A number of similar studies in Bangladesh have reported switching rates of 26–39% (Ahmed et al., 2006; Bennear et al., 2013; Chen et al., 2007), although others find higher rates of between one-half and two-thirds of affected households (George et al., 2012b; Madajewicz et al., 2007; Opar et al., 2007). One notable difference is that Madajewicz et al. (2007) and George et al. (2012b) document switching rate of 50 to 70% in response surveys conducted after one year. We conducted the response survey on switching after a relatively shorter period of three months, and switching rate may increase further if the behavioral response to arsenic information increases with time (Balasubramanya et al., 2014). In line with prior evidence (Chen et al., 2007; Opar et al., 2007), we find that distance to safer wells is an important predictor of switching (Fig. 7). We calculate the distance to the nearest safe well using the geo-locations recorded at the time of sales in round 1. The level of response to information could be related to the limited number of wells identified as safe, or because of lower take-up of the for-fee service, as opposed to blanket testing.[21]

[20] Only two of 633 households with a safe well switched to another safe well. Since our three-months follow up survey covered only those households who had purchased the test in round 1, we do not have information on switching by the households who purchased it in round 2 or did not purchase at all.

[21] This also highlights the potential for a positive externality where arsenic tests are accessible to all well owners.

Table 6
Do purchase decisions at high price levels correlate with wealth?.

	Test purchased		
	(1)	(2)	(3)
Asset index	0.0456* (0.0250)	0.0550*** (0.0193)	0.0509*** (0.0195)
Price	−0.0109*** (0.00177)		
Price × Asset index	0.000642 (0.000668)		
High Price (> = Rs.40)		−0.247*** (0.0608)	
High Price (> = Rs.40) × Asset index		0.0193 (0.0270)	
Price = Rs. 20			−0.215** (0.0970)
Price = Rs. 30			−0.292*** (0.0918)
Price = Rs. 40			−0.378*** (0.0745)
Price = Rs. 50			−0.444*** (0.0769)
(Price = Rs.20) × Asset index			0.00204 (0.0856)
(Price = Rs.30) × Asset index			0.0105 (0.0413)
(Price = Rs.40) × Asset index			0.0392 (0.0329)
(Price = Rs.50) × Asset index			0.00658 (0.0253)
Constant	0.691*** (0.0620)	0.473*** (0.0480)	0.635*** (0.0527)
Observations	3229	3229	3229
R-squared	0.104	0.067	0.112
Mean at price = Rs. 10	0.636	0.636	0.636
Mean across all prices	0.402	0.402	0.402

Note: the table tests whether purchase at higher price levels are correlated with household's wealth. Sample includes all the households who participated in the round 2 survey. The dependent variable 'Test Purchased' indicates whether a household has purchased the test in either round. Different specifications include continuous price variable (Column 1), high price dummy variable (Column 2), and price indicator variables (Column 3), and their interaction with asset index. Alternate specification for high price dummy (i.e Price > = Rs.30) provides similar result (not shown). Cluster bootstrap standard errors (obtained from 400 replications) in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

3.3. *Price paid for information and behavioral response*

We further test whether the propensity to switch depends on the purchase price (Fig. 6, Table C6); that is, whether the behavioral response to environmental quality information varies with the price paid to obtain the information.

The estimated coefficients shown in Fig. 6 do not indicate an association between the price paid and the probability of switching to a safer well. However, the confidence intervals on the estimated coefficients are wide in most cases and are large enough to include high negative and positive values (Table C6). For example, a 95% confidence interval on the high price dummy (price > = *Rs*.30) is almost twice as large than the mean level of switching across prices. This limits our ability to draw any strong conclusion.

With the above caveat of large confidential intervals, we can only tentatively infer that our data do not show evidence of large screening or sunk cost effects. Both effects would tend to increase usage with price, and imply that highly subsidized provision might lead to 'overinclusion' of those who do not sufficiently value the information provided.[22] We also note that

[22] In our setting, the respective arguments are as follows: 'those who decided to buy at high price care more about health from the outset, and will,

Table 7
Behavioral response to arsenic test outcome.

	Switched to a safe well		Switched to a safe or moderately contaminated well	
	(1)	(2)	(3)	(4)
Test outcome = High arsenic		0.276*** (0.0621)		0.305*** (0.0624)
Test outcome = High or moderate arsenic	0.242*** (0.0435)		0.259*** (0.0425)	
Safe well (Constant)	0.00316* (0.00186)	0.00316 (0.00195)	0.00316* (0.00186)	0.00316* (0.00185)
Observations	1037	844	1037	844
R-squared	0.158	0.214	0.171	0.239

Note: the table shows the probability that households whose wells had unsafe arsenic levels ('red') switched to safer wells. Arsenic test results from 2012 data (round 1); self-reported switching data from 2013 follow-up survey. Column (1) considers switching only to wells with safe ('blue') levels of arsenic; Column (2) and (3) considers switching to safe or moderately contaminated ('green') wells. Cluster bootstrap standard errors (obtained from 400 replications) in parentheses. ***$p < 0.01$, ** $p < 0.05$, *$p < 0.1$.

in our setting, higher price could pose an obstacle to switching, because with lower demand, the status of fewer wells is revealed, resulting into fewer opportunities to switch to known safe wells. This effect would favor less switching at higher prices, in contrast to screening or sunk cost effects. While tentative, our result is consistent with recent findings that have suggested that, for preventive health care products, there is little empirical evidence of overinclusion in subsidized provision (Cohen and Dupas, 2010; Dupas, 2014a).

3.4. Concealing and selective recall of high arsenic result

We find strong evidence of selective recall, and find that households not only avoided reporting adverse arsenic test outcomes but also took direct action to remove markers of unwelcome results. When visited at the time of the second sales offer, households who purchased a test when the first sales offer was made two years earlier were asked "Do you know the status of this well with respect to arsenic?". About 26% of households responded that their water was not fully safe (and about 15% stated that they could not recall). However, the actual test outcome distribution in round 1 tests showed that the proportion of highly and moderately contaminated wells was about 21% and 18.5%, respectively.

Table 8 offers a test for selective recall that builds upon this observation. It compares the proportion of test outcomes in each category of arsenic contamination levels (Red/high, Green/moderate, and Blue/safe) observed in the round 1 tests recorded in 2012 to the proportion of corresponding test outcomes *recalled* in 2014. We adduce the information on arsenic status of a well in three different ways: (1) those households where the arsenic-status placard was still affixed to the well; (2) those where the placard had been removed from the well, but was still kept in the house; and (3) those where the placard was neither on the well nor kept in house, but the respondent reported being able to remember the arsenic contamination status.[23]

The data show that the proportion of respondents who purchased a test in round 1 and believed their wells to be unsafe when visited during the round 2 survey was consistently some 9 to 11 pp lower than the true proportion of red tests recorded in round 1 (Columns 1, 4, 7, and 10). It is particularly striking that such a discrepancy exists even among households where the arsenic-status placard was still attached to the well: since it is inconceivable that red tags are more likely to be accidentally lost than others, this is clear evidence of intent either to hide the well's status, or to avoid being reminded of it (Column 1). The magnitude of the effect is very substantial: 20% of wells tested 'red' in 2012; a decrease of the share of 'red' wells by about 9–11 pp, therefore, implies that about half the households with wells that were high in arsenic intentionally sought to hide the test outcome. In the case of placard-color recall, estimates suggest that one in six households with safe well also could not recall the placard color, whereas about half of households with high arsenic fail to recall (Columns 7 and 9).[24]

We also note that respondents who did not produce a placard tended to preferentially indicate that wells were tested 'green' – suggesting that households prefer to claim a moderate arsenic level in their highly contaminated wells (Column 8). Conversely, as Appendix Table C7 shows, wells in households that opted to repeat the arsenic test in 2014 were more likely

(*footnote continued*)
therefore, be more likely to switch wells'; and 'those who buy at high prices have invested more in the test, and will hence more highly value the information it yields'.

[23] The survey specifically asked "Do you remember the color of the placard that was provided?", which was followed by another question on the placard color.

[24] In Bangladesh, the self-reported recall rate for well safety is found to have a relatively smaller difference when unsafe wells are compared with safe wells. Balasubramanya et al. (2014) document 2% higher and 4% lower recall rate for unsafe wells after two and five years of testing, respectively.

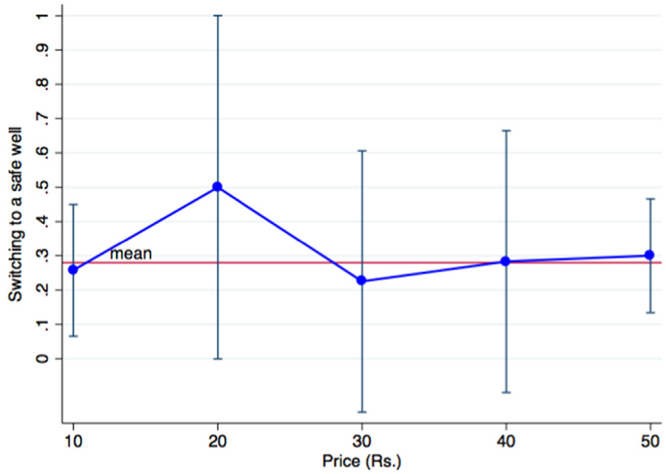

Fig. 6. Effect of price on behavioral response to information.
Note: the graph shows how the behavioral response (i.e. switching probability) varies with the price of the arsenic test, condition on test purchase. The sample consists of all households whose well turned out to be high in arsenic in round 1. Outcome variable denotes whether the household switched to a safe well after known the test result. Pattern remains the same when we include households who switched from a high arsenic well to a relatively moderate arsenic well. 95% confidence intervals with cluster bootstrap standard errors (obtained from 400 replications) are shown. Detailed regression results are provided in Table C6.

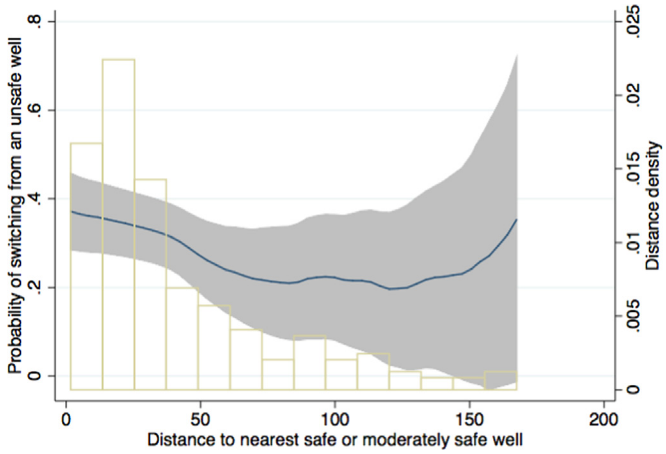

Fig. 7. Switching conditional on distance to blue/green.
Note: the graph shows the probability that household whose wells tested 'red' (high arsenic) in 2012 switched to a safer ('blue' or 'green') well, conditional on distance (in meters) to the nearest safer well. Distance is calculated using GPS locations of wells collected at the time of testing. Local polynomial fit with confidence interval; histogram of distances overlaid. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

to have tested 'green' than those only tested once. It is possible that some households opted to purchase another test because they could not recall the result of the earlier test. However, more specifically, the higher proportion of repeat purchases among 'green' wells that tested borderline safe may suggest that some households who initially received 'mixed news' sought to resolve any uncertainty, and, hence, were more likely to purchase the test again than those who received clear 'good' (i.e. blue) or 'bad' news (i.e. red).

These findings are consistent with general theoretical and experimental evidence of 'self-serving bias' and 'over-confidence' (see, e.g., (Eil and Rao, 2011)). More practically, we note that efforts to hide unsafe well status could be related to low well-switching rates in various ways. It could be that well owners hide bad news because there is (for unrelated reasons) a high private or social cost to taking action to remedy the situation, as evidenced by the relatively low switching rates reported above. It is also possible that both the reluctance to share and the propensity to hide bad news speak to a social stigma or material loss (e.g. in house value, for the United States, Boyle et al. (2010) found a temporary 1% reduction in residential sales values associated with a 10 μg/l increment in arsenic levels) associated with owning an unsafe well. We note that there is some indication that wealthier households may be more likely to hide adverse test results, potentially because of greater concerns over stigma or material loss.

Table 8
Selective recall of arsenic test outcomes.

Placard color	Round 2 sample: Placard on the well			Round 2 sample: Placard in the house			Round 2 sample: Placard color recall			Round 2 sample: All combined		
	Red (1)	Green (2)	Blue (3)	Red (4)	Green (5)	Blue (6)	Red (7)	Green (8)	Blue (9)	Red (10)	Green (11)	Blue (12)
Difference in proportion (between round 1 and the survey in round 2)	−0.0942*** (0.0239)	0.0584 (0.0355)	0.0358 (0.0402)	−0.0925** (0.0431)	0.155*** (0.0503)	−0.0621 (0.0715)	−0.102*** (0.0376)	0.208*** (0.0351)	−0.106* (0.0541)	−0.0955*** (0.0256)	0.118*** (0.0330)	−0.0221 (0.0420)
Actual proportion (round 1 data)	0.21	0.18	0.61	0.21	0.18	0.61	0.21	0.18	0.61	0.21	0.18	0.61
Recorded proportion (round 2 survey after 2 years)	0.12	0.24	0.64	0.12	0.34	0.54	0.11	0.39	0.50	0.11	0.30	0.58
R-squared	0.010	0.004	0.001	0.006	0.016	0.002	0.006	0.025	0.004	0.014	0.018	0.000
N(round 1)		1208			1208			1208			1208	
N(round 2 survey after 2 years)		321			171			140			631	
N(total)		1529			1379			1348			1840	

Note: the table compares the proportion of 'red' (unsafe), 'green' (moderately contaminated) and 'blue' (safe) wells in the recorded outcome of tests conducted in 2012 (as measured), and in household recall or retained placards obtained in the 2014 survey. Top row headings denote subsamples from the round 2 survey– (1) "fixed on well" – the placard still fixed on the well (Columns 1-3), (2) "kept in house"- removed from the well but still kept in the house (Columns 4-6), and (3) "recall of placard color"- the proportion of red, green and blue recall (Columns 7–9), respectively. Columns (10–12) pool information on well status from all test outcome recall and retained placards in round 2. The coefficient on 'Difference in proportion' reflects the difference in shares of each test result category in round 2, when we compare corresponding subsamples from round 2 with the actual measurements in round 1. We estimate the 'Difference in proportion' by regressing test outcome dummy indicator (pooled from round 1 and round 2) on an indicator variable for round 2, for each sub-sample category. "Actual proportion" displayed are the actual measured test outcomes in the round 1 (i.e. constant in the regression). "Recorded proportion" indicate observed or recalled test outcome during round 2 within each subsample. The sample sizes in respective columns reflect the count of all tests recorded in 2012 (i.e. N(round 1)) and the number of households for which information in a given category was available in 2014 (i.e. N(round 2)). Out of 1208 placards mounted on wells in 2012 (data on four placards is missing), we find 321 placards still fixed to the well while 171 placards kept in house (but not on the well) after two years. Cluster bootstrap standard errors (obtained from 400 replications) in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 9
Selective recall and household assets.

	Placard color red			
	Round 2 sample: Placard on the well (1)	Round 2 sample: Placard kept in house (2)	Round 2 sample: Placard color recall (3)	Round 2 sample: All combined (4)
Second round	−0.0831***	−0.0688	−0.0652	−0.0760***
	(0.0285)	(0.0507)	(0.0411)	(0.0256)
Household owns consumer durables	0.0423	0.0423	0.0423	0.0423
	(0.0402)	(0.0405)	(0.0406)	(0.0397)
Second round X Household owns consumer durables	−0.0571	−0.0661	−0.125**	−0.0728*
	(0.0495)	(0.0662)	(0.0634)	(0.0407)
R-squared	0.012	0.007	0.009	0.016
N(round 1)	1179	1179	1179	1179
N(round 2)	318	171	140	629
N(total)	1497	1350	1319	1808

Note: the table shows differences in the share of 'red' wells in 2012 tests and 2014 recall/retained placard as shown in the Table 8, but conditional on ownership of (any) consumer durables. The coefficient on 'Household owns consumer durables' is the same across all four samples by construction: it is only the composition of the 2014 sub-sample that changes, not the composition of the 2012 test sample. Corresponding sample sizes are shown at the bottom of the table. Cluster bootstrap standard errors (obtained from 400 replications) in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

To investigate this, we compare test results and recall as above for high arsenic outcome, but distinguish between households that owned and did not own consumer durables (the one asset ownership indicator collected consistently in both survey rounds) (Table 9). As is evident, while all households under-report, households that own durables are about twice as likely to do so; the difference is significant for the larger samples.

We add two caveats regarding our evidence on concealment and selective recall of adverse outcomes. First, these estimates in Tables 8 and 9 represent concealment and selective recall of adverse test outcomes by households who first revealed their preference for knowing the arsenic status of their well, since we cannot analyze households who did not purchase the test. Secondly, while we cannot correct for attrition during the round 2 survey and due to the imperfect recall of test purchase itself, attrition would pose little threat to our results qualitatively: attrition would bias the observed proportion of adverse outcome downward if it is correlated with adverse test results. However, such a correlation is in itself evidence of selective recall.

4. Conclusion and policy discussion

We use experimental evidence from Bihar, India, to quantify the demand for and use of environmental information relevant to health. The data show substantial demand for testing wells for arsenic, but also considerable sensitivity to price. Compared to the near universal adoption found under a free provision, two-thirds of households purchased tests at the lowest price, and about one-third at the highest price, over the duration of the project. We also find that a repeat offer made within two years of the original offer was met with significant demand, raising total coverage by 17pp, from 30% to 47%. The considerably higher additional demand observed is remarkable because the opportunities for learning are circumscribed by the fact that arsenic tests are an experienced good only in a limited sense. Thus, once some consumers buy tests in the round 1, others may observe that neighboring wells test positive for arsenic, and may learn about opportunities to switch; however, because the health impacts of arsenic are slow in onset, health benefits from switching are not immediately observable.

Our demand estimates compare well with results shown by George et al. (2013), who estimated demand for arsenic tests in Bangladesh at a single price point of USD 0.28 in 2011 – the equivalent of about Rs. 10 in 2014 in our setting. George et al. find a 53% uptake in the control group, where no dedicated awareness campaign was conducted, and a 93% uptake in each of two treatment arms with an awareness campaign. Our estimate at Rs. 10 is between these two values after two offers, but far below what would be expected after a single offer. This is perhaps intuitive: arsenic tests were not widely known in our intervention area, while George et al. (2013) worked in Bangladesh, where government-sponsored blanket testing and many other interventions have significantly raised awareness of arsenic.

Our results confirm that subsidies remain critical in ensuring high coverage of environmental quality information relevant to human health. Cost-shared provision might still have a useful role to play in providing an ongoing testing service in the absence of, or between, public testing campaigns. In particular, one could imagine a business model in which independent testers generate their own wages, while NGOs conduct awareness campaigns, provide test kits, train testers, and implement quality control (for instance, GIS tracking and re-testing of a subsample of wells). However, market demand was not quite sufficient to cover wages. In 2012, expected daily revenue was about Rs. 200 (revenue per offer made was highest in the Rs. 30-50 price range, at about Rs. 8; on average, testers visited about 25 households per day). By way of contrast, under local labor market conditions, testers might have expected a daily wage in the range of Rs. 300–400.

Through a follow-up survey conducted after the first wave of sales, we assessed how households respond to the environmental quality information furnished through well testing. About one-third of households with unsafe wells switched to less perilous water sources. This is in the lower range of switching rates found in other studies of arsenic testing. Preferences for sharing within caste groups may have limited opportunities to draw water from safer sources, which is an important consideration for future arsenic testing campaigns in Bihar. Among households in our survey, 90% reported that they prefer to exchange water within their own caste or group of relatives. Similarly, in Uttar Pradesh, a state adjacent to Bihar, caste in particular has been found to be a major factor in impeding water trade within a village (Anderson, 2011). We also note that the margin of effort in switching after the information is revealed by arsenic testing may be significantly higher than it is in using many health products. For example, in Bangladesh, Madajewicz et al. (2007) estimate that switching to a safer well costs four additional minutes for a round trip, and the time spent daily would be higher when several trips are required in a day.

We further explore other important and policy-relevant aspects of the provision of environmental quality information. First, we test whether the probability of switching depends on the price paid for the test. Our results provide weak evidence on an absence of correlation between price paid and switching, which would imply that in our setting, willingness to pay for information on environmental quality had little impact on the behavioral response to such information. We note, however, that limited power does not allow us to precisely estimate the presence of selection effect, which significantly weakens this conclusion.

Secondly, by comparing the share of wells with safe and unsafe arsenic levels between test results collected in 2012 and results recalled in 2014, we show that households avoid reporting adverse test results, and, indeed, recall test outcomes strategically, or even remove arsenic-status placards indicating arsenic contamination. This may speak to discomfort around knowledge of well status in the context of low switching rates, stigma, or concerns over property value. The reaction is policy-relevant, in particular when allowing for the possibility that the *ex ante* decision to purchase a test might be affected by any motivation to avoid bad news. In many settings, local environmental quality information remains private and strategic revealing by households may defeat mitigation efforts and elevate the damage to others who cannot readily access this information.

Appendix A. Robustness check

In this appendix, we discuss robustness of our demand estimates to restricting the sample to villages where implementation issues in data collection and actual coverage in round 1 were less pronounced. This appendix is an extension of our discussion in Section 2.5; our analysis here is based on the data on geo-location of sales collected in round 1 and the location of arsenic-status placards recovered during the round 2.

The series of Google Earth maps included in Appendix D, all on the same scale, show well locations recorded while

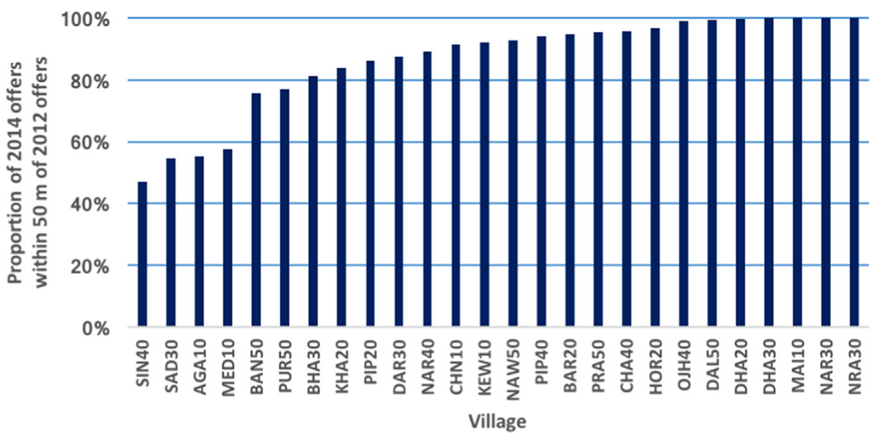

Fig. A 1. Exclusion of village areas during the round 1.
Note: the plot shows the proportion of 2014 offers (second round) within 50 m of a 2012 offer (first round) out of the total number of 2014 offers. A lower proportion would suggest that a higher number of households in specific areas of a village were either not covered under the first round or their data was not recorded. Average proportion (across all 26 villages) is 0.83. A similar pattern is observed if we vary the 50 m cutoff to 25 m or 100 m.

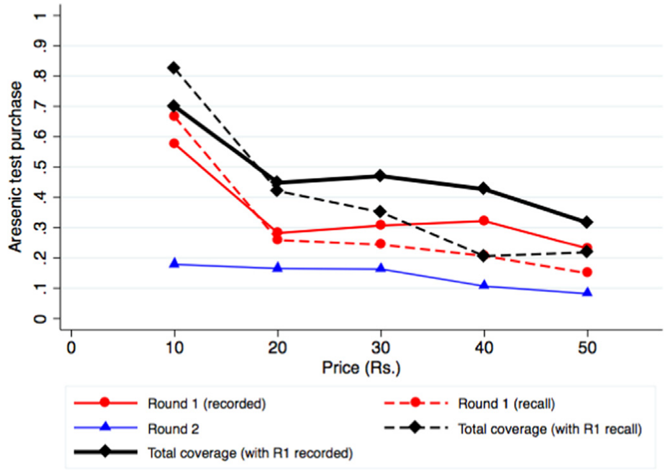

Fig. A 2. Demand estimates (22 villages).
Note: the plot shows demand estimates obtained using data from 22 villages, which excludes 4 villages where data limitations are most severe. The plot is estimated in the same way as Fig. 5. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

making the first and second round of test offers in each of the 26 villages. With the exception of a limited number of outliers, probably due to poor satellite coverage or trees and buildings degrading the signal, the maps show that the wells are concentrated in villages surrounded by open fields. The locations of wells recorded during the round 2 are shown in two colors to distinguish those located within 50 m of a well recorded during round 1. The maps suggest that a significant portion of four villages (coded as AGA10, MED10, SAD30, and SIN40) mapped by the second round of offers was either not covered or the sales and offer data were not recorded during the first round of offers. We analyze this apparent pattern more systematically by calculating the proportion of second-round offers within 50 m from an offer in round 1 for each village (Fig. A1). There is a clear discontinuity around 60% in the proportion coverage that separates the four partially-covered villages from the remaining 22, which confirms our observation from visual inspection of the maps. This pattern is robust to using alternate cutoff distances of 25 m or 100 m.

Next, we note that some arsenic-status placards were recovered during round 2 in areas that, according to the maps of round 1 data, were not covered by test offers during round 1. We look at locations of placards recovered on wells (or in houses) at a distance of 50 m or more from the nearest recorded sales in 2012. In total, 47 (9.6 percent) out of all 492 placards observed in 2014 were found over 50 m from a 2013 offer in all 26 villages. But about 87% (i.e. 41 out of 47) of these 47 placards come from only three of the four partially covered villages (no such placards were recovered in SIN40). For

Table A1
Estimated demand: Robustness check by excluding villages partially-covered in round 1.

	Round 1 demand		Round 2 demand	Total coverage after two rounds	
	Recorded purchase-based (1)	Recalled purchase-based (2)	(3)	(Round 1 purchase as recorded) (4)	(Round 1 purchase as recalled) (5)
Price	−0.00463*	−0.00821**	−0.00259**	−0.00645**	−0.0121***
	(0.00242)	(0.00375)	(0.00128)	(0.00253)	(0.00253)
Constant	0.455***	0.514***	0.218***	0.644***	0.740***
	(0.0966)	(0.146)	(0.0481)	(0.0984)	(0.0918)
Observations	3,139	2,064	3,139	3,139	3,139
R-squared	0.017	0.057	0.009	0.028	0.105
Mean at Price = Rs. 10	0.576	0.665	0.178	0.700	0.824
Mean across Price groups	0.319	0.276	0.142	0.455	0.384

Note: the table shows demand slope estimates obtained using data from 22 villages, which excludes 4 villages with severe data limitations. Empirical specification is the same as in Table 5. Demand for 2012 is estimated in two ways – (1) using recorded sales in 2012 over 2014 sample, and (2) using recall data on sales/offer in 2012 (as collected in 2014). Total coverage after round 2 is also calculated in two ways, using either estimate of the first round demand. Cluster bootstrap standard errors (based on 400 replications) in parentheses. *** $p < 0.01$, ** $p < 0.05$, *$p < 0.1$.

Table A2
Estimated demand: Robustness check with enumerator fixed effects.

	Round 1 demand		Round 2 demand	Total coverage after two rounds	
	Recorded purchase-based (1)	Recalled purchase-based (2)	(3)	(Round 1 purchase as recorded) (4)	(Round 1 purchase as recalled) (5)
Price	−0.00260	−0.00606*	−0.00466**	−0.00682**	−0.0124***
	(0.00256)	(0.00365)	(0.00197)	(0.00265)	(0.00189)
Constant	0.367***	0.433***	0.301***	0.652***	0.753***
	(0.0832)	(0.126)	(0.0669)	(0.0864)	(0.0640)
Observations	4084	2667	4084	4084	4084
R-squared	0.016	0.047	0.040	0.046	0.107

Note: the table shows demand slope estimates obtained after controlling for enumerator fixed effects. Empirical specification is the same as in Table 5. Demand for 2012 is estimated in two ways – (1) using recorded sales in 2012 over 2014 sample, and (2) using recall data on sales/offer in 2012 (as collected in 2014). Total coverage after round 2 is also calculated in two ways, using either estimate of round 1 demand. Cluster bootstrap standard errors (based on 400 replications) in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

comparison, our round 1 data indicates that a total of 113 tests were sold in these three villages in 2012. Furthermore, we also observe that these three villages were assigned to a particular enumerator team. The balance of six such arsenic-status placards came from an additional three villages, while no such cases were found in remaining 20 villages.

Based on the above discussion, we conclude that partial coverage of village area, as well as limited data collection both, may have contributed towards the observed difference between the number of offers in 2012 and the sample size recorded in 2014 (as evident from a comparison of Columns 2 and 3 in Table 4). It is impossible to accurately determine the extent to which each factor contributed in the aggregate and in each village; however, we can leverage the fact that we know that four out of 26 villages are most affected by partial coverage and unrecorded sales to conduct a robustness check.

In Fig. A2, we show demand results obtained by restricting the sample to the 22 villages without obvious coverage issues (using the same specification as in Fig. 5). Demand curves show a similar downward pattern and the total coverage calculated from both rounds barely shifts when 22 are considered instead of the full sample of 26 villages. Table A1 show the estimated linear demand slopes. The estimated slope coefficients are larger in magnitude for round 1 demand (both measures) but are almost the same for total coverage after two rounds. This remains true when we also drop two additional villages where fewer than 80% of round 2 observations were within 50 m of a round 1 offer (BAN50 and PUR50) (not shown). Finally, we test the robustness of the estimated demand slope by including enumerator fixed effects and find similar estimates as in Table 5 (Table A2). The main demand estimates of our study are therefore robust to removing villages with higher concerns about implementation issues in round 1.

Appendix B: why is there substantial demand at the time of the second offer?

We find that repeating the offer after a two-year delay generates substantial additional demand and raised total coverage by about 18 pp, from 29.7% to 47.3% (Table 4, Column 13). Second-round remand is more price-sensitive when compared with the more conservative estimate of first round demand (Fig. 5). We observe an effect of repeating the sales offer on coverage at any price level, with increases ranging from 80% of the original sales at Rs. 10 to 35% at Rs. 50. To study the (reduced form) effect of making a repeat offer, we kept nominal price constant within a village. This, in turn, limits our ability to directly test for learning as a specific mechanism driving demand at the time of the second offer. The reason we cannot assess learning as in Dupas (2014b) is as follows. Our product is distinct from the ITNs offered in Dupas (2014b) in that there is no reason for households to repeat arsenic tests, whereas there is reason to purchase ITNs again after some time. Still, if we had made the second sales offer at a uniform price, we might have tested for learning by using first-round price to instrument for first-round demand, and then studying the effect of first-round demand on second-round demand through peer learning. This is not possible, however, when price levels are the same in the first and second round: as an instrument, price would clearly violate the exclusion restriction.

From a policy perspective, the effect of making a repeat offer is remarkable: price matters greatly for demand, but at any price level considered here, repeating the offer meaningfully increases coverage (and from a business perspective, sales). Irrespective of the channels - learning, income growth, or marketing intensity - this simple finding underscores the need for a more careful assessment of experimental evidence generated with offers available only for a short period.

Because we lack a household panel, and because there may be some error in recall of first-round tests, we cannot completely rule out the concern that some of the demand at the second offer may be driven by households that might not have been approached during the first offer phase in 2012. However, the observable evidence offers significant reassurance.

Table B1
Household characteristics of round 1 and 2 buyers.

	Panel A: as observed at time of purchase		
	2014 buyers (1)	2012 buyers (2)	2014 vs. 2012 (1)–(2)
HH has consumer durables	0.225 (0.0404)	0.226 (0.0276)	−0.00135 (0.0392)
	Panel B: as observed in 2014		
	2014 buyers (1)	2012 recall (2)	2014 vs. 2012 recall (1) – (2)
Household characteristics			
Number of HH members	4.919 (0.367)	4.311 (0.325)	0.608 (0.382)
Infant living in HH	0.302 (0.0459)	0.223 (0.0246)	0.0798** (0.0370)
Child living in HH	0.488 (0.0585)	0.438 (0.0618)	0.0497 (0.0657)
Housing characteristics			
House pucca	0.701 (0.0556)	0.756 (0.0504)	−0.0553 (0.0391)
Has latrine	0.330 (0.0551)	0.408 (0.0496)	−0.0778 (0.0553)
Asset ownership			
HH has consumer durables	0.225 (0.0404)	0.301 (0.0563)	−0.0766* (0.0405)
Has cell phone	0.912 (0.0230)	0.861 (0.0578)	0.0507 (0.0460)
Has TV	0.208 (0.0372)	0.298 (0.0573)	−0.0905** (0.0424)
Has bicycle	0.783 (0.0187)	0.811 (0.0402)	−0.0285 (0.0382)
Has motorbike	0.248 (0.0254)	0.261 (0.0243)	−0.0131 (0.0260)
Has cow	0.680 (0.0417)	0.680 (0.0319)	6.24e-05 (0.0353)
Caste			
Scheduled caste or tribe	0.0163 (0.00852)	0.0386 (0.0240)	−0.0223 (0.0226)
Other backward caste	0.227 (0.0518)	0.127 (0.0298)	0.0995** (0.0411)
Kshatriya	0.0767 (0.0309)	0.124 (0.0455)	−0.0473 (0.0371)
Brahmin	0.251 (0.0658)	0.388 (0.0646)	−0.137*** (0.0510)
Baniya	0.297 (0.0670)	0.203 (0.0446)	0.0940* (0.0537)

Note: the table shows characteristics of households that bought tests in 2014 (Column 1) and 2012 (Column 2), and the change between the two phases (Column 3). Panel A shows ownership data as observed at the time of purchase; Panel B shows data as observed in 2014 – that is, 2014 values for those who buy in 2014 in Column (1), and 2014 values for those who recall having purchased in 2012 in Column (2). Cluster bootstrap standard errors (obtained from 400 replications) in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B2
Do first-round test results relate to second-round demand?.

	Demand in round 2				
	(1)	(2)	(3)	(4)	(5)
Share of wells in village tested arsenic high (red) in first round	0.0384 (0.112) [0.0301]	0.0699 (0.125) [0.0384]	0.0437 (0.107) [0.0301]	0.0933 (0.114) [0.0326]	0.117 (0.130) [0.0404]
Controls					
Price	Yes	Yes	Yes	Yes	Yes
First-round demand	No	No	Linear	Quadratic	Quadratic
Wealth proxies	No	Yes	No	No	Yes
N	4084	3002	4084	4084	3002
R-squared	0.037	0.060	0.051	0.059	0.082

Note: the table summarizes the correlation between arsenic test outcomes in the first phase and the demand in second phase. In each column, the dependent variable is demand for well tests in the second phase of offers, and the coefficient of interest is the share of wells that tested 'red' (high arsenic) among wells tested in the first offer phase. All models include price controls; Columns 3-5 control for first-round demand, and Column 5 controls for wealth proxies. We consider Column 4 to show the preferred specification. Cluster bootstrap standard errors (400 replications) in parentheses, classical standard errors in square brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

About 70% of the new purchases in 2014 are made by households who recall being offered the test in 2012, but did not purchase (Table 4, Columns 8 and 9). Perhaps most compellingly, the pattern of 2014 demand is very similar among those who recall having been made an earlier offer and the overall sample (Columns 13 and 14).

It is intriguing to ask why there is a high level of demand when a repeat offer is made within the relatively short timeframe of two years. Although our data do not allow us to conclusively assess this question, we present some suggestive evidence in this Appendix. (i) Strong state-level growth in nominal income between survey rounds suggests that changes in wealth between the first and second offer may have played a role; our survey data on asset ownership are consistent with this mechanism, but not conclusive. The absence of a wealth pattern in demand is at odds with this explanation (see Section 3.1.1). (ii) Learning may have led households to adjust their valuation of arsenic testing. The product's characteristics were not familiar to potential customers at the time of the first offer, and the initial wave of tests may have allowed households to change their beliefs about the possibility of contamination, and opportunities to switch, although the health benefits of switching cannot be observed within two years. We obtain the 'expected' sign in a test with a credibly causal interpretation, but the results are not significant (i.e. a positive but insignificant effect of 'arsenic unsafe' outcome in the first phase on the demand for arsenic testing during the second phase). (iii) In the absence of conclusive evidence on wealth or learning effects, one could speculate about a direct effect of repeating the offer – what one might call a 'marketing' or 'nudge' effect. We consider it a priority for further work to assess the importance of such an effect.

This appendix summarizes evidence on what might explain demand at the time of the repeat offer. On balance, the evidence is inconclusive. Patterns in wealth proxies are consistent with a contribution of growing income and wealth. We note, however, that this is at odds with the absence of a correlation of wealth proxies with sales price among buyers shown above. A test for learning that allows for a sound causal interpretation is consistent in sign, but not significant.

B.1. Wealth effects

There is mixed evidence on increased wealth as a driver of repeat offer demand. As reported above, we find that observable wealth does not correlate systematically with willingness to pay. Indeed, one of the two wealth proxies that does correlate – ownership of a latrine – can be read as a marker of difference in concern over health that might affect valuation of the arsenic test as much as it may speak to lower marginal utility of consumption.

Still, there are some good reasons to ask whether rising wealth may have, to some degree, contributed to generating additional demand.

The most important piece of *prima facie* evidence is the rapid economic growth Bihar experienced between sales rounds. Per capita real income rose precipitously, at a rate of about 10% per year between 2012 and 2014.[25] In line with such a favorable development, ownership of consumer durables among households who purchased tests in the first round of offers (the one asset category we can reliably compare among both survey rounds, and the one group of consumers sampled in a consistent way) rose by 5 pp from a baseline value of 23% between 2012 and 2014 (result not shown). Because the tests were offered at the same *nominal* price in both phases, inflation further reinforced this effect. In total, nominal per capita income grew by some 38% between the two offers.

[25] State GDP growth for India from https://data.gov.in/catalog/major-socio-economic-indicators-states-india

Secondly, patterns in asset ownership among buyer groups and across time are consistent with a wealth effect – although they do not offer a very powerful test. Our data allow in principle for two tests to reject wealth effects (at the mean). Most obviously, we can compare wealth among the two groups of buyers *at the time of purchase*, that is, in 2012 and 2014, respectively. This comparison could furnish some evidence against wealth effects if it were to emerge that second-round buyers were less well-off at the time of purchase than first-round buyers were at the time their wells were tested (with the assumption that the two groups initially had the same valuation of the tests). We can only draw this comparison on the ownership of (any) consumer durables; questions used to collect ownership information for all other categories of assets differed too much between the 2012 and 2014 surveys. For consumer durables, there is no significant difference between buyer groups, and the coefficient is centered near zero (Panel A in Table B1). This finding is consistent with wealth effects (new buyers catching up in wealth to original buyers), but also does not exclude a contribution of learning.

Beyond the ownership of consumer durables, we are constrained to comparing wealth as observed in the year 2014: among households that bought in 2012 and households that bought in 2014. This comparison could also reject wealth effects, namely if second-round buyers were weakly better off in 2014 than first-round buyers (and we were willing to assume that growth in wealth among the two groups was such that the ranking had not been reversed since 2012 – which would then imply, less appealingly, that the wealthier group initially had a lower valuation of the tests). Our data suggest quite clearly that the opposite was the case: first-round buyers were better off than second-round buyers when surveyed in 2014 (Table B1). Difference in ownership of durables such as TV and consumer durables are significant, second-round buyers have significantly less education than first-round buyers, and there are notable differences in caste composition[26].

B.2. Learning

Arsenic tests in themselves are distinctly a non-experience good: a one-off application which does not directly affect the consumer. It is therefore most plausible to suggest that learning might be chiefly driven by increased awareness of the probability of arsenic contamination, and of opportunities to switch to safe wells.

We test in the following way for evidence of learning after the first wave of tests. Because the distribution of arsenic in ground water varies substantially and unpredictably over small distances, variation in the results of first-round tests is exogenous. We posit that different distributions of first-round results at the village level may induce differential effects on second-round demand. In particular, we speculate that, when a high share of wells tested 'unsafe' during the first wave, concern in the village community over arsenic contamination might have been raised, translating into learning, namely greater awareness of the health risks associated with arsenic, and the benefits of testing and well-switching. Empirically, the relationship between second-phase purchases and the share of wells tested unsafe' in the first phase has the expected sign, across a range of specifications (Table B2). However, results are not significant with cluster bootstrap standard errors. Furthermore, we have considerably low statistical power to detect any learning effect in Table B2 because there are only 26 villages in our sample.

Appendix C

Table C1
Attrition balance in the follow up survey after round 1.

	Full sample			Follow up sample			Attritted sample				
	N	mean	SD	N	mean	SD	N	mean	SD	Difference	P-Value
Price paid (Rs.)	1212	25.20	13.41	1032	25.36	13.35	180	24.28	13.74	1.081	0.319
Well age (Years)	1210	8.222	7.470	1031	8.320	7.641	179	7.659	6.392	0.661	0.275
Well depth (Ft.)	1210	82.16	24.62	1031	82.49	24.60	179	80.26	24.71	2.226	0.264
Household owns consumer durables	1185	0.226	0.418	1012	0.227	0.419	173	0.220	0.415	0.008	0.825

Note: the table shows comparison of household samples in the follow-up survey on switching. Among the households who purchased the test in round 1, enumerators could contact about 86% households for follow up on switching after three months.

[26] We note that, strictly speaking, we are comparing between one group observed pre-treatment (2014 buyers) and one group observed post-treatment (2012 buyers). However, since the health effects of arsenic are long-term, one would not expect a strong treatment effect a mere two years after the test, even conditional on households effectively avoiding exposure. We acknowledge that in principle, arsenic testing could have had effects upon wealth through conduits other than health – for instance, a change in the value of houses with wells tested safe/unsafe, or a change in social status with implications for future wealth.

Table C2
Estimated non-linear demand.

	Round 1 demand				Round 2 demand		Total coverage			
	Recorded purchase-based	[WB p-val]	Recalled purchase-based	[WB p-val]	demand	[WB p-val]	Round 2 and recorded sales	[WB p-val]	Round 2 and recalled sales	[WB p-val]
	(1)		(2)		(3)		(4)		(5)	
Mean at Price = Rs. 10 (Constant)	0.376***		0.403***		0.300***		0.656***		0.686***	
	(0.100)	[0.085]	(0.148)	[0.110]	(0.0713)	[0.000]	(0.0683)	[0.000]	(0.0680)	[0.000]
Price = Rs. 20	−0.0955		−0.146		−0.134*		−0.211*		−0.267***	
	(0.126)	[0.465]	(0.175)	[0.435]	(0.0728)	[0.120]	(0.113)	[0.155]	(0.0700)	[0.005]
Price = Rs. 30	−0.109		−0.132		−0.156*		−0.245**		−0.309***	
	(0.112)	[0.350]	(0.165)	[0.485]	(0.0926)	[0.080]	(0.109)	[0.100]	(0.0742)	[0.005]
Price = Rs. 40	−0.0759		−0.195		−0.168**		−0.224**		−0.440***	
	(0.109)	[0.470]	(0.155)	[0.405]	(0.0793)	[0.050]	(0.0939)	[0.025]	(0.0709)	[0.005]
Price = Rs. 50	−0.146		−0.255		−0.218***		−0.340***		−0.469***	
	(0.114)	[0.280]	(0.163)	[0.255]	(0.0740)	[0.015]	(0.0934)	[0.005]	(0.0709)	[0.005]
Observations	4084		2666		4084		4084		4084	
R-squared	0.011		0.034		0.037		0.050		0.112	

Note: the table shows estimated demand coefficients with a flexible model using indicator variables for price levels. The demand for round 1 is estimated in two approaches – (1) using recorded purchase in 2012 over 2014 sample, and (2) using recall data on purchase/offer in 2012 (as collected in 2014). Total coverage after round 2 is also calculated in two ways, using either estimate of the round 1 demand. Note that, there are 74 repeat purchases in our data, which are accounted only once before calculating total coverage. Cluster bootstrap standard errors (based on 400 replications) in parentheses. Wild bootstrap p-values [WB p-val] are shown next to the estimated coefficient (Cameron et al., 2008). ***p < 0.01, ** p < 0.05, * p < 0.1.

Table C3
Robustness check: Demand at high price.

	Round 1 demand				Round 2 demand		Total coverage			
	Recorded purchase-based	[WB p-val]	Recalled purchase-based	[WB p-val]	demand	[WB p-val]	Round 2 and recorded sales	[WB p-val]	Round 2 and recalled sales	[WB p-val]
	(1)		(2)		(3)		(4)		(5)	
Price >= Rs. 30	−0.0570		−0.106		−0.105**		−0.152**		−0.252***	
	(0.0667)	[0.400]	(0.0781)	[0.180]	(0.0488)	[0.018]	(0.0736)	[0.043]	(0.0565)	[0.005]
Constant	0.325***		0.321***		0.228***		0.543***		0.544***	
	(0.0589)		(0.0709)		(0.0420)		(0.0606)		(0.0528)	
Observations	4084		2666		4084		4084		4084	
R-squared	0.004		0.014		0.019		0.023		0.065	

Note: the table shows robustness check for high price coefficient. The high price dummy is defined as 1 for top three price levels (Rs. 30, 40 and 50), and 0 otherwise. Alternate specification is shown in Table 5. The demand for round 1 is estimated in two approaches– (1) using recorded purchase in 2012 over 2014 sample, and (2) using recall data on purchase/offer in 2012 (as collected in 2014). Total coverage after round 2 is also calculated in two ways, using either estimate of the round 1 demand. Note that, there are 74 repeat purchases in our data, which are accounted only once before calculating total coverage. Cluster bootstrap standard errors (based on 400 replications) in parentheses. Wild bootstrap p-values [WB p-val] are shown next to the estimated coefficient (Cameron et al., 2008). ***p < 0.01, ** p < 0.05, * p < 0.1.

Table C4
Purchase decision and household characteristics.

	Household purchased the test											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Price	− 0.0105***	− 0.0105***	− 0.00879***	− 0.0105***	− 0.00895***	− 0.0104***	− 0.00820***	− 0.0104***	− 0.0101***	− 0.0104***	− 0.0110***	− 0.0116***
	(0.00208)	(0.00154)	(0.00221)	(0.00161)	(0.00346)	(0.00162)	(0.00186)	(0.00170)	(0.00155)	(0.00170)	(0.00170)	(0.00229)
Children in HH	0.0994											
	(0.0740)											
Children in HHX Price	0.00171											
	(0.00207)											
Infants in HH		0.174**										
		(0.0695)										
Infants in HH X Price		0.000110										
		(0.00204)										
House pucca			0.0874									
			(0.0684)									
House pucca X Price			−0.00159									
			(0.00194)									
Has latrine				0.184**								
				(0.0720)								
Has latrine X Price				−0.00137								
				(0.00193)								
Has cell					0.0818							
					(0.114)							
Has cell X Price					−0.00131							
					(0.00316)							
Has TV						0.0229						
						(0.0665)						
Has TV X Price						0.00244						
						(0.00215)						
Has cow							0.106**					
							(0.0412)					
Has cow X Price							−0.00284**					
							(0.00120)					
Has consumer durables								0.0290				
								(0.0642)				

Table C4 (*continued*)

	Household purchased the test											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Has consumer durables X Price								0.00209				
								(0.00203)				
Schedules caste or tribe									0.291			
									(0.191)			
Schedules caste or tribe X Price									−0.00912			
									(0.00967)			
Other backward caste										−0.0505		
										(0.0754)		
Other backward caste X Price										−0.00127		
										(0.00223)		
Household head illiterate											−0.145*	
											(0.0755)	
Household head illiterate X Price											0.00168	
											(0.00267)	
At least one adult with primary education												0.0961
												(0.0763)
At least one adult with primary education X Price												0.000602
												(0.00217)
Constant	0.612***	0.629***	0.552***	0.621***	0.584***	0.646***	0.585***	0.646***	0.656***	0.682***	0.708***	0.662***
	(0.0788)	(0.0591)	(0.0808)	(0.0533)	(0.132)	(0.0573)	(0.0637)	(0.0609)	(0.0578)	(0.0661)	(0.0563)	(0.0735)
Observations	3522	3528	3758	3528	3528	3528	3527	3528	3341	3341	3294	3318
R-squared	0.098	0.098	0.082	0.096	0.078	0.083	0.079	0.083	0.081	0.084	0.090	0.097
mean	0.386	0.387	0.346	0.387	0.387	0.387	0.387	0.387	0.391	0.391	0.401	0.401

Note: the table shows relationship between household characteristics and the decision to purchase the test, with a specification similar to Table 6. Socio-economic data come from the second-round survey. Outcome variable denotes an indicator for any purchase by the household in either round (first round purchases are based on recall). Cluster bootstrap standard errors (obtained from 400 replications) in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C5
Sorting on arsenic contamination.

	Dependent variable: High arsenic well				
	(1)	(2)	(3)	(4)	(5)
Well age	−0.00234 (0.00323)				
Well depth	0.00114 (0.00127)				
Well cost	1.48e-06 (9.79e-06)				
Price		0.0051 (0.00386)			
High price (> = Rs.40)			0.1012 (0.0908)		
Asset Index				0.0212 (0.0309)	
					Coefficients from univariate regressions
Car					0.172 (0.140)
Cell					−0.0148 (0.0881)
Several Cells					−0.0558 (0.0800)
TV					−0.00610 (0.0615)
Bike					0.0626* (0.0325)
Motorbike					−0.0285 (0.0413)
Cow					0.102** (0.0438)
Several Cows					0.0529 (0.0514)
Whitegoods					0.0377 (0.0679)
Pucca					−0.0255 (0.0609)
Latrice					0.0981 (0.0689)
Number of HH members					−0.00480 (0.00936)
Infants					0.0125 (0.0212)
Children					−0.00866 (0.0219)
Observations	677	719	719	676	719
R-squared	0.007	0.022	0.008	0.002	n/a

Note: the table shows correlations among wells tested in 2014, between the probability of a well having high arsenic status (at least 50 μg/l) with characteristics of the well (Column 1), price (Column 2 and 3), asset index (Column 4) and the household asset ownership (Column 5). To avoid evident overfitting problems, regression coefficients show in Column 5 were obtained by performing univariate regressions for each characteristic. Cluster bootstrap standard errors (obtained from 400 replications) in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C6
Effect of price paid on behavioral response to information.

	Switched from high arsenic to safe well		Switched from high arsenic to safe or moderately contaminated well	
	(1)		(2)	
		95% CI		*95% CI*
Panel A: Linear Specification				
Price	0.000425	[−0.00638, 0.00723]	0.00110	[−0.00600, 0.00820]
	(0.00347)		(0.00362)	
Constant	0.267**	[0.03964, 0.49436]	0.276**	[0.03572, 0.49828]
	(0.116)		(0.118)	
R-squared	0.001		0.001	
Panel B: High price dummy				
Price > = Rs. 30	−0.0242	[−0.28096, 0.23256]	0.000578	[−0.25814, 0.25930]
	(0.131)		(0.132)	
Constant	0.295***	[0.10135, 0.48865]	0.308***	[0.11612, 0.49988]
	(0.0988)		(0.0979)	
R-squared	0.001		0.001	
Price > = Rs. 40	0.0191	[−0.23570, 0.27390]	0.0260	[−0.23468, 0.28668]
	(0.130)		(0.133)	
Constant	0.271***	[0.10126, 0.44074]	0.297***	[0.13510, 0.45890]
	(0.0866)		(0.0826)	
R-squared	0.001		0.001	
Panel C: Breakdown by price levels				
Price=Rs. 20	0.242	[−0.30092, 0.78492]	0.227	[−0.31592, 0.76992]
	(0.277)		(0.277)	
Price=Rs. 30	−0.0326	[−0.47360, 0.40840]	0.00227	[−0.41913, 0.42367]
	(0.225)		(0.215)	
Price=Rs. 40	0.0254	[−0.39012, 0.44092]	0.0292	[−0.41376, 0.47216]
	(0.212)		(0.226)	
Price=Rs. 50	0.0424	[−0.21632, 0.30112]	0.0773	[−0.15006, 0.30466]
	(0.132)		(0.116)	
Constant (mean at Price=Rs. 10)	0.258***	[0.03964, 0.49436]	0.273***	[0.18860, 0.34540]
	(0.0971)		(0.0971)	
R-squared	0.018		0.014	
Joint significance				
Wald Chi2	0.096		1.13	
Prob > Chi2	0.916		0.889	
Mean across price groups	0.280		0.308	
Observations	211		211	

Note: the table shows the correlation between behavioral response (i.e. switching) and price paid for arsenic testing. Panel A include continuous price variable, and Panel B include high price dummy variables. Panel C shows regression coefficient for price group level dummy variables. Arsenic test results from 2012–13 data (round 1); self-reported switching data from 2013 follow-up survey conducted three months later. Column (1) considers switching only to wells with safe ('blue') levels of arsenic; Column (2) considers switching to safe or moderately contaminated ('green') wells. 95% confidence intervals are shown next to each estimate. Cluster bootstrap standard errors (obtained from 400 replications) in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C7
Decision to re-test depends on contamination status.

	Well contamination status		
	Red (1)	Green (2)	Blue (3)
Test purchased in both 2012 and 2014	−0.0411 (0.0582)	0.172*** (0.0598)	−0.130* (0.0792)
Share among wells tested once only	*0.257*	*0.274*	*0.468*
Observations	719	719	719
R-squared	0.001	0.013	0.006

Note: the table compares the proportion of 'red' (unsafe), 'green' (moderately contaminated) and 'blue' (safe) wells in the recorded results of tests conducted in 2014, among households that recalled preciously purchasing a test, and households that recalled a prior offer, but no purchase. Arsenic levels are stable over time, so test results obtained in 2012 can be assumed to have been identical to those measured in 2014. Cluster bootstrap standard errors (obtained from 400 replications) in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix D. Supplementary data

Supplementary data associated with this article can be found in the online version at http://dx.doi.org/10.1016/j.jeem.2017.08.002.

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