The value of clean water: Experimental evidence from rural India*

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Abstract

Over 2 billion people lack clean drinking water, but existing solutions suffer from high costs (piped water) or persistently low demand (point-of-use chlorine). We use a cluster-randomized field experiment to test an alternative approach: decentralized treatment and home delivery of clean water to the rural poor. At low prices, there is over 90 percent take-up, sustained throughout the experiment. Water delivery is cost-effective, and also privately profitable at high prices. We use the experiment to recover revealed-preference measures of household valuation, finding substantial willingness-to-pay and even higher willingness-to-accept. The latter exceeds the full variable costs of providing water for free.

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1 Introduction

More than 2 billion people still do not have clean drinking water at home. The costs of this deprivation are staggering. Drinking contaminated water causes approximately 2 billion cases of diarrhea and half a million deaths among children under 5 annually – making it the 5th-leading driver of child mortality worldwide (WHO, UNICEF, World Bank, 2022).[1] Yet despite this acute public health crisis, clean water access is increasing so slowly that it has not kept pace with population growth: the WHO estimates that the number of people without clean water in low-income countries *increased* by 197 million between 2000 and 2022 (WHO, UNICEF, 2024). Sustainable Development Goal 6—clean water and sanitation—is far from being met, and the UN argues that achieving these 2030 targets will require the pace of drinking water improvement to accelerate six-fold, even as climate change is exacerbating water scarcity, leaving billions of people newly vulnerable to water-borne disease (United Nations, 2022; World Bank, 2016).

These bald facts make clear that existing solutions to the clean water access problem are falling short. Prior work inside and outside of economics has focused on two main clean water access approaches. The first is directly supplying piped water to the home (e.g., Devoto et al. (2012))—which is how drinking water is delivered to the vast majority of households in high-income countries.[2] Piped drinking water is not a new technology — the United Kingdom has required that *all* new houses have clean piped water since the passage of the Public Health Act of 1875. Yet, nearly 150 years on, less than 30% of the overall population in low-income countries—and only 14% of the rural population—has access to clean water at home, reflecting high capital costs and practical implementation challenges (WHO, UNICEF, 2024).[3]

Furthermore, money has not proved to be the only barrier to clean water access via pipes. The experience of developing countries suggests that even with water infrastructure in place, keeping water clean is challenging. Lab testing reveals that piped water in low-income countries is often just as contaminated as untreated surface water (World Bank, 2017). Indeed, the US Center for Disease Control and Prevention identified only 58 countries and sub-national territories worldwide where tap water is safe for drinking. This list excludes

[1] For a sense of scale, the losses in disability-adjusted life years (DALYs) from unclean drinking water well exceed those from the first year of the global COVID pandemic on an annual basis.

[2] As of 2022, 94.3% of the population in these countries had "safely-managed" drinking water, defined as having an improved water source located on the premises, with water that is free from contamination is available when needed (WHO, UNICEF, 2024).

[3] 63% of historical spending on water has been in the form of capital expenditures (World Bank, 2024). Reaching the Sustainable Development Goals of universal clean water and sanitation access by 2030 is expected to require increasing annual spending by between $131 and $141 billion (World Bank, 2024).

most of Asia (including India and China), much of Central and South America, and all of Sub-Saharan Africa (Centers for Disease Control and Prevention, 2023).[4]

In response to this disappointing reality, a second approach has gained traction among researchers: point-of-use treatment using chlorine tablets or solution. A rich body of evidence suggests that chlorine-based water treatment at home is both effective and cheap. Kremer et al. (2023) carry out a meta-analysis of 15 field experiments providing drinking water to the rural poor, revealing that point-of-use treatment is highly cost-effective as a means of reducing infant mortality.[5] However, this same rich evidence also reveals that chlorine is remarkably unpopular with users. The take up of chlorine-based water treatments is very low, with the meta-analysis reporting an average compliance in experiments of just 46% (Kremer et al., 2023). Crucially, low take-up of chlorine is not due to monetary costs: even at *zero price*, chlorine usage has been far too low to achieve anywhere close to universal access. Moreover, many households who do obtain chlorine do not ultimately use it to treat their drinking water.[6]

These facts pose two challenges for researchers. The first is to identify new options that do better at enabling universal access. The second is to better understand household preferences and quantify how much they value clean water itself. This paper seeks to contribute on both these fronts. We conducted a cluster-randomized field trial, in partnership with a small private company serving poor households in rural Odisha, to study a novel solution to the drinking water problem: decentralized water treatment combined with home delivery in sealed, reusable containers. This approach has the potential to overcome several problems faced by prior approaches. It has dramatically lower capital and maintenance costs than piped water, which means it can even be profitably supplied by private providers like our implementation partner. It alleviates the non-monetary costs of chlorine solutions and tablets, such as unpleasant taste and the inconvenience and cognitive burden of treating water at home, both of which may explain the relative unpopularity of point-of-use treat-

[4]Prior work found that in India's National Capital Region, one of the most developed parts of the country, 60% of piped water samples were unfit to drink (Jalan and Somanathan, 2008).

[5]All but one of the studies examined in Kremer et al. (2023) involves point-of-use treatment. The exception is a study on protecting water from springs at the point of collection (Kremer et al., 2011a). Berry, Fischer, and Guiteras (2020) study in-home water filtration rather than chlorine.

[6]These results are remarkably consistent across contexts. Low initial take-up at low or zero prices has been documented in Bangladesh (Luoto et al., 2011), Malawi (Dupas et al., forthcoming), and Kenya (Null et al., 2018), while meaningful gaps between initial take-up and follow-up chlorine usage has been shown in Bangladesh (Luoto et al., 2011), Kenya (Dupas et al., 2016), and Zambia (Ashraf, Berry, and Shapiro, 2010a). In our own data, only 3.6% of representative control-group households report using chlorine to treat water in an average survey round, even though chlorine is widely available and very cheap.

ment.[7] Lastly, home delivery removes any time costs and contamination risks associated with households collecting and transporting their own water (e.g., Kremer et al. (2011a)).

Remarkably, notwithstanding decades of work on treatment and filtration technologies in developing countries, virtually no evidence exists on interventions that directly supply clean water — as distinct from treatment technology — to the rural poor. This is particularly surprising because households in low- and middle-income countries have increasingly turned to non-tap sources of clean water often supplied by the private sector. Between 2005 and 2015, usage of privately supplied clean water in Brazil, China, India, Indonesia, Mexico, and Thailand increased by 175% (Cohen and Ray, 2018).

Our experiment randomly assigned 60,000 households in 120 villages in Odisha, India, into treatment arms intended to study household demand under different market and non-market approaches to providing home delivery of clean drinking water. Odisha, one of the poorest states in India and with low levels of safe drinking water access, is an ideal setting for this experiment. Section 2 provides more detail on our study setting and on how water was treated and delivered to the home.

The experiment, which we describe in detail in Section 3, had three treatment arms: (i) a 'Prices' regime where water was sold at varying prices through randomized household-specific discounts, (ii) a 'Free Ration' regime where households could order a limited amount of water per month for free, and (iii) an 'Exchangeable Entitlement' regime, where households could either order clean water for free or, by forgoing an order, receive cash rebates of varying amounts for every unused unit of their entitlement. Control households could also buy water, at the prevailing (high) price that was in force prior to the experiment. We analyze this experiment using rich administrative data on water orders and a detailed household survey, which we describe in Section 4.

Our first main finding is that home delivery of clean water works well to expand clean water access. We present this and our other results in Section 5. We find that take-up of clean water at low prices is nearly universal and is sustained over the experiment duration. In stark contrast to the low take-up of point-of-use chlorine water treatments, we find that approximately 90% of households order water when it is free. We see no evidence of a "zero price effect" (Cohen and Dupas, 2010): take-up is similarly high at low prices — at our lowest price of INR 0.14 / litre, take-up is about 89%. Home delivery of clean water thus overcomes take-up ceilings documented in other water-treatment approaches (Dupas et al., forthcoming).

[7] Taste aversion to water treated with chlorine tablets or solution has been documented across the world, sometimes even leading households to choose contaminated over treated water (Jeuland et al., 2016; Crider et al., 2017; Puget et al., 2010; Smith et al., 2021; Dupas et al., 2016).

The variation in water use across treatment arms we observe comes largely from differences in take-up and not from differences in the amounts ordered conditional on take-up. Across all treatment arms and all prices, we find that *if* a household chooses to order water at all, they do so in substantial quantities, sufficient to fulfill all or most of their drinking water needs, based on conventional benchmarks of 1.5–2 litres per day per person. Put differently, the patterns of demand we observe are consistent with informed consumers seeking to obtain reliable access to clean drinking water, rather than obtaining utility from standalone containers of water. For households choosing to order, demand is also stable over time, unlike prior evidence on chlorine-based water treatment (e.g., Ashraf, Berry, and Shapiro (2010a); Berry, Fischer, and Guiteras (2020); Dupas et al. (2016)). Interestingly, demand is not unbounded even when water is free. Our ration was generous enough to exceed the likely drinking water needs of most households. We find that although households do order somewhat more water when it is free than when it is highly priced, the ration does not bind. This result suggests limited waste and perhaps a role for free distribution.[8]

Water demand in this setting may reflect both time and health benefits. Using self-reported outcome data, we find evidence of reduced time-use and improved health outcomes from clean water, the latter measured with more noise.[9]

Our second main finding is that households value clean water highly. Because some households in our intervention directly trade off money and clean water, we are able to derive two incentivized valuation metrics from our experiment: a willingness-to-pay (WTP) measure from the prices arm, and a (bounded) measure of willingness-to-accept (WTA) derived from a household's choice to forgo cash in lieu of water in the exchangeable entitlement arm. Measuring both is substantively important for several reasons. First, theory predicts that the WTA may significantly exceed the WTP for goods with limited substitutes that are highly valued (Hanemann, 1991). This describes our setting, and indeed many cases of environmental quality, quite well. Second, revealed preference measures of WTP may be biased by various development market-failures including liquidity constraints, as highlighted by Greenstone and Jack (2015). Lastly, when policymakers decide how to allocate limited funds, both measures of valuation are important and the WTA can motivate increased spending on environmental and health goods without the need to appeal to social externalities.

We estimate a population average WTP from our experimentally measured demand curve for clean water of INR 132 per month for clean water access ($20 annually at INR 80

[8]Although our population is deprived of potable water, they do have plentiful access to water for other purposes. This result likely reflects this fact.

[9]Intent to treat point estimates suggest water collection times reduce by 15 to 39 percent, illness episodes by 14 to 22 percent, and missing work by 20-45 percent. Instrumental variable estimates of health improvements are larger and more precise.

per USD). This is about 1.5 percent of median household monthly expenditure. The best comparable estimate to our number comes from seminal work by Kremer et al. (2011a) who use travel cost methods to indirectly infer an annual WTP of USD 4.44 (adjusted to 2023 dollars) amongst rural Kenyan households for year long access to a protected (clean) spring. Our estimate is over 4.5 times as high. The WTP we measure for home-delivered clean water is also dramatically higher than chlorine-based point-of-use treatment. Adjusted to 2023 dollars, the population average WTP for chlorin (home-use chlorine solution) reported in Ashraf, Berry, and Shapiro (2010b) is about USD 0.11 (about 14 times lower) for an amount of water equal to the monthly consumption in our sample. Indeed, the WTP from other studies of chlorin for home use (e.g., Kremer et al. (2011b)) is even smaller. These extraordinarily low valuations reflect the substantial gap between the monetary costs of point-of-use chlorine and its take-up and a key implication of our experiment is that this does not represent how much households value water.[10]

We estimate a lower-bound for WTA that is even larger, at INR 420 per month (approximately $60 annually), or 4.7% of median expenditures. Our WTA estimates are high enough to exceed the full variable costs of providing water to households for free. This cost is also lower than existing cash transfers, in theory creating the policy space for a swap that mirrors the one of the treatment arms that households respond to in our experiment.

Although our treatment effects are encouraging, any approach to providing drinking water must be amenable to large-scale implementation. We suggest that direct delivery of decentralized treated water passes this test. First, it appears technically feasible to sustain. We do not need to theorize about this given the empirical fact that our partner firm has worked for over a decade in a few hundred villages in Odisha. In addition, we gather detailed data on fixed and variable implementation costs which suggest that private water sales at a relatively high price can be (just) profitable. This may explain the growth of private sector non-tap alternatives serving people in developing countries (Cohen and Ray, 2018).

Unfortunately, the private market cannot solve the universal drinking water access problem. Our cost data and our experimental demand curve demonstrate that providing clean water at high prices can be privately profitable. Indeed, private provision of clean water is proliferating across low-and-middle-income countries, with decentralized water kiosk and "water on wheels" delivery models like those used by our partner becoming increasingly com-

[10]Our directly revealed estimates are also large compared to other indirectly-inferred WTP measures of environmental quality in developing countries. For example, Baylis et al. (2024) use experimental variation in demand for masks to back out a WTP measure and find that low-income residents of Delhi would pay just USD 1.33 (2023 dollars) annually for a 10 unit decrease in air pollution, about 15 times lower than our WTP estimate and 45 times lower than our lower-bound on WTA. Our estimates are comparable to WTP for clean air in much-richer China (Ito and Zhang, 2020).

mon (Advani et al., 2011; Brown et al., 2011; Daly et al., 2021). However, our experiment also reveals that the prices required to turn a profit result in extremely low take-up. This means that achieving *universal* clean water access would require the government to play a role in subsidizing costs. Water in our treatment is significantly more expensive than providing home-use chlorine to households, inclusive of implementation costs, but we carry out a back-of-the-envelope calculation using benefit estimates from Kremer et al. (2023) and estimate that the cost per DALY of decentralized home delivery of clean water ranges between $68 and $165, implying that 100 percent subsidies would easily clear conventional cost-effectiveness benchmarks while approaching near universal take-up. We discuss these issues further in Section 6.

This paper makes two key contributions to the literature in both development and environmental economics. First, we provide revealed-preference experimental estimates of how much households value clean drinking water. Importantly, we recover direct valuations of clean water, thus extending prior work that has either inferred this quantity by placing a financial estimate on non-monetary costs such as time (Kremer et al., 2011a), or used take-it-or-leave-it or Becker, DeGroot, and Marschak (1964) approaches to estimating demand for clean water *production technologies* such as chlorine solution (Ashraf, Berry, and Shapiro, 2010a) or water filters (Berry, Fischer, and Guiteras, 2020). This distinction appears meaningful, because our estimates of WTP are much higher than past work has found. More broadly, we also add to the literature on valuing environmental quality in both low-and-middle-income countries (e.g., Greenstone and Jack (2015); Ito and Zhang (2020); Baylis et al. (2024)) and in high-income countries (e.g., Chay and Greenstone (2005); Deschênes, Greenstone, and Shapiro (2017); Keiser and Shapiro (2019)).

Our paper also provides perhaps the first experimental evidence on willingness-to-accept measures of valuation for clean water or environmental quality more generally, extending a prior literature that has been reliant on contingent valuation (Horowitz and McConnell, 2002; Tunçel and Hammitt, 2014). Because the good we provide (tasteless and potable water) has very poor substitutes in our setting, this paper also provides empirical evidence confirming classic theoretical work predicting that the willingness-to-accept in such cases may far exceed willingness-to-pay (Hanemann, 1991).

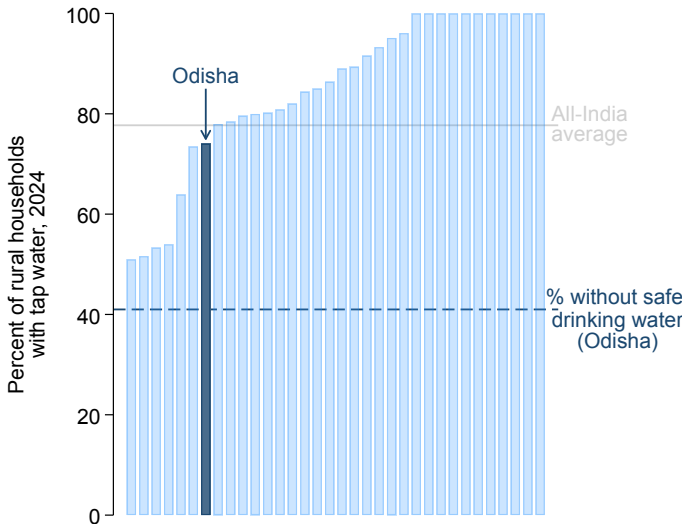
Our second contribution is evaluating a novel, increasingly widespread, and effective method for extending clean water access in the developing world. Prior research on drinking water in low-income countries has been overwhelmingly focused on point-of-use treatment (Ashraf, Berry, and Shapiro (2010a); Kremer et al. (2011b); Luoto et al. (2011); Dupas et al. (2016); Null et al. (2018); Berry, Fischer, and Guiteras (2020); Dupas et al. (forthcoming), culminating in the Kremer et al. (2023) meta-analysis), with a smaller body of work on piped

water (Devoto et al., 2012). Given the magnitude of the clean water access problem, and the limited prospects for this problem being solved by either point-of-use treatment or piped water alone, demonstrating the efficacy of additional options is paramount.

2 Background

The setting for this paper is the Indian state of Odisha. Odisha is one of the poorer regions in the country, with its HDI ranking 29th out of 36 Indian states and union territories in 2017–2018 (MOSPI (2021)). As with many parts of the developing world, Odisha faces a severe drinking water access problem, especially in rural areas. Figure 1 plots the share of rural households with tap water connections across Indian states and union territories per the Department of Drinking Water and Sanitation. Odisha ranks 28th out of 34, with 26% of households still lacking tap water as of 2024 (Jal Jeevan Mission (2024)). Moreover, a 2023 survey in 9,856 villages revealed that 41% of the population lacked access to safe drinking water (Atmashakti Trust (2023)). Perhaps unsurprisingly, drinking water became the subject of grassroots protests (Express News Service (2024b)) and was a heated election campaign topic (Express News Service (2024a)) in Odisha during India's 2024 general election.

Figure 1: Clean water access in Odisha



Notes: This figure plots the share of rural households with a tap water connection across Indian states and union territories in 2024, using data from the Department of Drinking Water and Sanitation (Jal Jeevan Mission, 2024). The gray line plots the all-India average. The dashed navy line shows the share of households in Odisha found to lack safe drinking water in an independent survey of nearly 10,000 villages in 2023 (Atmashakti Trust, 2023).

Study population We conducted our study in 160 villages spanning 6 of Odisha's districts; we map the study location in Appendix Figure B.1.Our study population was relatively poor, with the representative control group reporting an average monthly household expenditure at endline of INR 12,494, or approximately INR 2,500 in monthly per-capita expenditure – meaningfully lower than the contemporaneous all-rural-India average of 3,773 (MOSPI, 2024). The median monthly expenditure was INR 9,000.[11] The median household in our sample had 5 members, of whom 3 are children. Our sample also had limited (but highly-skewed) savings, with the control reporting a median (mean) bank balance of only INR 2,000 (INR 22,791). The median household reported being able to make a purchase of no more than INR 500 ($\approx$ 6 USD) tomorrow without borrowing, suggesting liquidity is limited.[12]

Clean water access Although our population was poorer than the national average, our households were not constrained for water. Odisha has an abundance of groundwater at shallow depths and receives substantial rainfall. Averaged across all our surveys over the course of the experiment (August 2022 – August 2023), 76% of the representative control group reported using ground water for drinking, and 34% reports using piped water. Less than 1% reported using surface water or bottled water.[13] That said, households do report expending time for water collection, with the average household spending 32 minutes a day obtaining drinking water.

Unfortunately, household access to *potable* drinking water could have nevertheless been quite poor, as river, ground, and surface water contamination are common in Odisha (Odagiri et al., 2016; Senapati, 2021; Biswas, 2022). Piped water is also not guaranteed to be safe to drink, with prior research having found *E. coli* in piped water in rural areas of the state (Reese, 2017).[14] Rural piped water in our setting was delivered from storage tanks filled with ground or surface water without central treatment plants, potentially increasing the chance of contamination. As indicative evidence, we collected 17 water samples from different sources (open well, tube well, tap, and the drinking water in our intervention) from

[11] As described in Section 4 below, we conducted repeated monthly surveys with a randomly-selected subset of experiment households, starting in the first month of the intervention and ending either 5 or 7 months after implementation began in each village. The endline contained more detail on households so we use the representative control in the endline survey to provide these details.

[12] A potential explanation for limited liquidity is that common income sources, such as agricultural income, remittances, and government transfers are irregular. In such settings, expenditure can be a more reliable measure of a household's economic status than monthly income. With this caveat, in response to a question on binned monthly income, the median control response was in the range INR 12,000 to INR 16,000.

[13] Households could use multiple sources, so the total exceeds 100%.

[14] This corroborates other findings from developing countries broadly (World Bank, 2017) and from urban India (Jalan and Somanathan, 2008) that show high degrees of contamination in piped water.

our study villages and had them tested by an Odisha state water testing laboratory (see Appendix Figure A.2). Though this sample is too small to be representative, we detected problems with pH, *E.Coli*, fecal coliform, and salmonella in at least one sample from every source *except* the treated water of our intervention. We found *E. Coli* and fecal coliform in all tap water samples.[15] There have been several operational challenges documented with the rural piped water network in Odisha that lead to a risk of contamination. These include very little systematic testing of water quality, no residual chlorine in most samples, and village level committees that face difficult management and maintenance challenges (Jal Jeevan Mission, 2022).

Many households appeared to be aware that their water was not safe, and took steps to treat at least some of it. Aggregated over monthly surveys during the experiment, 13% of households in the representative control group reported using chlorine in at least one survey round, 19% of households reported boiling water at least once, and 39% reported straining at least once. Nevertheless, regular water treatment was somewhat more limited, with only 3.2% of households reporting using chlorine, 9.6% reporting boiling, and 19.5% reporting straining in the average survey round.

Implementation partner In 2022, we began a collaboration with Spring Health India Pvt. Ltd—a private company that sells clean water to rural households in Odisha. Spring Health was founded in 2011, and in 2022 operated in 230 villages in 7 districts of the state. We partnered with Spring Health to conduct a field experiment, discussed in more detail in the next section.

The clean water sold by Spring Health originally comes from local ground water or surface water, and is treated in a plant powered using decentralized solar electricity. In most cases, there is one treatment facility per village. Spring Health trains a local entrepreneur who operates and maintains the treatment facility and is normally also the owner of the well providing the input water. We discuss the various elements and costs of this business model in more detail in Section 6 as part of a cost-benefit analysis of our intervention.

Spring Health treats its water using an electro-chlorination process using solar panels to power the treatment plant. This procedure is intended to remove coliform and other organic contaminants, but does not address contamination due to arsenic or other heavy metals.[16]

[15] Unlike prior work on water in South Asia, which is concerned with heavy metals (e.g. Buchmann et al. (2022)), this is not a major problem in Odisha. India's Central Ground Water Board has detected arsenic contamination in only one district in Odisha, lying outside our study regions (Ministry of Jal Shakti (2022)). In our own water tests, no samples contained problematic levels of arsenic, cadmium, or lead.

[16] As discussed above, arsenic contamination has not been detected in our study districts.

Throughout this paper, unless explicitly specified, we use 'treated water' or 'clean water' to indicate water that has been treated to remove coliform, or that has passed a coliform test.

In Spring Health's status quo business model, any household in a village served by Spring Health could pay for clean water deliveries to their home. Treated water is packaged and sealed in reusable bottles and delivered by Spring Health delivery staff to the doorstep of enrolled households.[17] Deliveries are made multiple times each week. Households could place orders as they like during the week, with payments made against orders fulfilled. Not all households paid at the time of delivery, with regular consumers often paying at the end of the month. Spring Health pays out a monthly stipend to both the operator of the treatment plant and to its delivery staff.

A key benefit of this model is that it eliminates some of the non-monetary costs associated with point-of-use treatment or source improvement. First, although treatment is decentralized and hence far cheaper than piped water networks, it occurs outside the household. This reduces the inconvenience costs of having to remember to treat and store water at home. Second, it ensures that water is appropriately treated and does not leave an unpleasant residual taste.[18] Third, the home delivery model eliminates time costs for households who may otherwise have had to travel to collect water, while also removing the risk of contamination in this process. An important remaining risk involves contamination at home, underscoring the continued importance of hygiene behaviours and habits such as boiling.

3 Experimental design

3.1 Research objectives

The design of our experiment is motivated by two main goals. First, we aim to measure whether—and how much—households value clean water. Empirical evidence on whether households value clean water itself, as distinct from water treatment methods, is critically important. If households do not value clean water, whether due to undervaluation of health, lack of information, or other behavioral factors (Dupas and Miguel, 2017; Kremer, Rao, and Schilbach, 2019), this would explain limited take-up of point-of-use treatment and have important implications for any other approach, including piped water infrastructure. Conversely, if households do value clean water, uncovering solutions which deliver this product

[17] 86% of our households live in 10-litre-bottle villages, and the remainder in 20-litre-bottle villages.

[18] The use of ClO_2 gas for treatment avoids the well-known unpleasant taste from using chlorine tablets or solutions, which use stable salts such as $NaOCL$, resulting in free chlorine and chloramine compounds containing nitrogen (Crider et al., 2018). In the taste tests we conduct, all participants ranked water treated with chlorine solution (per the package's specifications) worse than both bottled water from the market (Bisleri brand) and Spring Health water (see Appendix Figure A.1).

outside of previously-evaluated approaches may have high returns. Moreover, because clean water is often publicly provided, two measures of valuation are of interest: both the amount a household would be willing to pay to obtain clean water (WTP) and the amount a household would have to be paid to forego clean water access (WTA). As pointed out in theoretical work by Hanemann (1991), WTP and WTA need not be the same in settings where substitutes are limited, even in the absence of market failures. In a developing-country context, market failures may also exist, driving a wedge between these metrics. Both quantities are therefore valuable to policymakers. Thus, our experiment is designed to capture both the WTP and WTA for clean water.

Second, we aim to test the viability of home delivery of clean water as a solution to the access problem. Specifically, we seek to measure whether households order clean water, how much households order, how these orders change over time, and how this varies with the price of water. Combining this information with data on costs also reveals the extent to which it is privately profitable to deliver clean water. As secondary outcomes, we are also interested in the extent to which home delivery of clean water affects drinking water sources, water collection time, water treatment, and a variety of health outcomes. Our experiment is thus designed to facilitate estimation of these features of water demand and subsequent benefits of clean water access.

3.2 Sampling

Our experiment took the form of a cluster-randomized field trial with several treatment arms. We selected 160 villages where Spring Health had an existing presence as the site of the experiment. All villages in the experiment had been served by Spring Health for at least 24 months prior to the beginning of the study.

Figure 2 depicts the experimental design. We randomly assigned 120 villages in the sample to one of three treatment arms (with 40 villages per arm), holding 40 villages back to serve as a buffer for necessary replacements / "pure control" group where no experiment activities or survey data collection took place. In treatment villages, we randomized every household in the village to either a (sub-)treatment arm, in the form of special offers from Spring Health (*nominally* 39 households per village, see randomization below), or to control (all other households). In 107 of the 120 main experiment villages, water is sold in 10 litre bottles for INR 1.4 per litre. In the remaining 13 villages, Spring Health sold water in 20 litre bottles for INR 1.25 per litre. 4 of these villages were randomly assigned to the price arm; 6 to the free ration arm; and 3 to the exchangeable entitlement arm.

Attrition Early in the experiment, our partner faced significant operational challenges and staff shortfalls following the loss of a valuable revenue stream through the sale of carbon credits. Water delivery and other implementation activities such as completing household payments were disrupted in some villages, and, as a result our experiment could only be implemented in 99 villages (36 in the discount group, 27 in the exchangeable entitlement group, and 36 in the free ration group), rather than the original 120. Appendix Figure C.1 shows that our treatment arms are pair-wise balanced, even after this attrition. Moreover, we include village fixed effects in all regressions, ensuring that our identification comes from *within-village* comparisons of households randomly assigned to treatment or control conditions.

3.3 Treatment arms

Priced water In each village in the 'prices' arm, we randomly assigned 13 households to receive a 10% discount offer for the duration of the experiment, 13 to receive a 50% discount offer, and 13 to receive a 90% discount offer. All remaining households received no discount, but were able to continue to order Spring Health water at the full market price.

Free water ration In each village in the 'free rations' arm, we randomly assigned 39 households to receive a free and unconditional ration of up to 400 litres of water per month.[19] To receive any water, households needed to place orders with Spring Health, just like paying customers. Households who exhausted their ration could order additional water at full price. Households could opt not to use some or all of their quota, since water deliveries were only made when requested. There was no penalty or benefit for households who chose not to avail the full ration. The remaining households received no ration (equivalent to a free quota of 0 litres per month), but could continue to order Spring Health water at the full market price.

Exchangeable water entitlements In each 'exchangeable entitlement' village, we randomly assigned 38 households to receive an offer of a 400 litre entitlement, just as in the free ration condition. However, in this arm, households could redeem unused water within their entitlement amount for cash. For every unclaimed bottle of water, households were randomly entitled to receive payments equal to 10% of the market price (9 households), 50% of the market price (10 households), 90% of the market price (9 households), or 100% of the market price (10 households).

[19]This was benchmarked to be well above the mean consumption for households observed in both pre-experiment administrative data and in our pilot.

The reimbursement rates were set exactly equal to the prices in the prices arm, creating similar monetary incentives differentiated only by whether ordering an additional bottle involved paying out vs forgoing cash. At no point did this condition involve any physical exchange of water for cash. As in the other arms, households who did not receive a non-zero exchangeable quota remained eligible to buy Spring Health water at full price.

Since the entitlement was monthly, each household's refund was calculated at the end of every month. Transfers were made either to mobile money accounts or bank accounts using details provided by households. While households were intended to be paid at a monthly frequency, in practice payments were made less often in part because refund amounts for a single month were often very small. Appendix Figure C.3 demonstrates that households do not change water ordering behavior in response to payments, suggesting that payment timing does not impact how households order water in this arm.

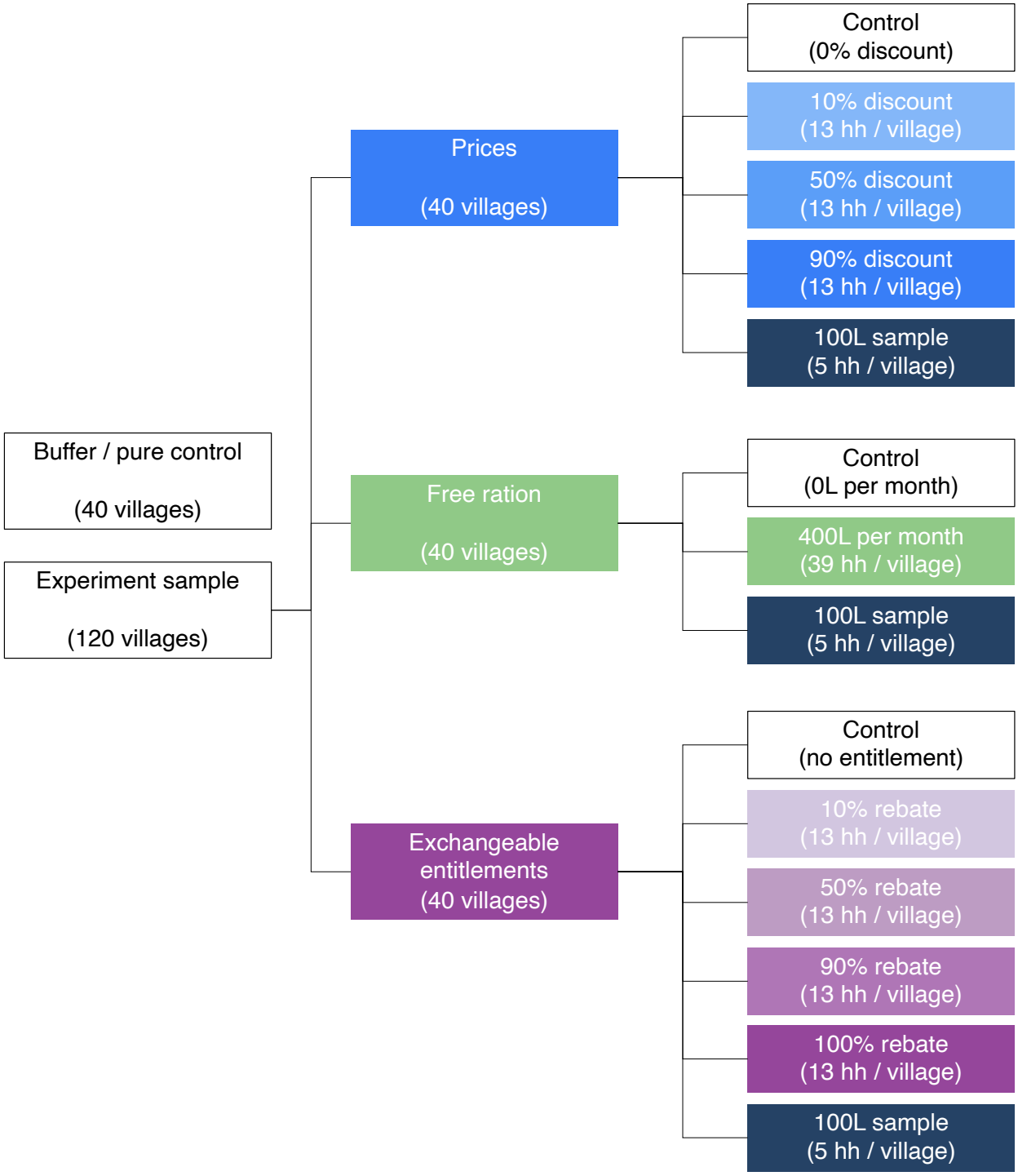
All treatment villages: Free one-time 100 litre sample In addition to the treatments described above, we randomly assigned 5 households in each treatment village to receive a one-time offer of 100 litres of free water. Households who chose not to take this offer received nothing in return. These households could otherwise order water at the market price. This condition provides a test of the extent to which experience with clean water impacts demand.

Buffer / pure control (40 villages) We assigned a final 40 villages to a buffer / pure control group, from which we drew replacement villages in the event of main-sample attrition. The remaining villages in this arm were completely 'business-as-usual'. Throughout the experiment, all households in these villages were able to order Spring Health water just as they had been doing before, but no surveys were conducted in these villages. Our only source of data on these villages comes from Spring Health's administrative data, generated through the normal course of business. We compare households in these villages to control households in treated villages in Appendix E.

3.4 Randomization

We intended to include all households in each treatment village in our study, and therefore conducted an *in-situ* randomization procedure, in which we distributed scratch cards drawn from a shuffled deck to every home in the village. We prepared decks based on the most recent census figures on village population, and each village's deck included 39 treatment cards. Since the number of households in most villages had changed since the census and since not every household had an available adult to receive a card during distribution, decks were not exhausted. Thus, in some villages the actual number of treatment cards distributed

Figure 2: Experimental design diagram



Notes: This figure shows the experiment design (pre-attrition). We randomly assigned 40 villages each to a price arm, an exchangeable entitlement arm, a free ration arm, and a buffer / pure control group (no intervention or surveys). In treatment villages, we randomly assigned 39 households to sub-treatments, 5 to a one-time free sample condition, and the remainder to control. In the price arm, treatment households could order clean water with discounts of 10, 50, or 90 percent. In the exchangeable entitlement arm, treatment households could order up to 400 litres of water per month for free, and received 10, 50, 90, or 100% rebates for un-ordered water. Treatment households in the free ration arm could order up to 400 litres of water per month for free, but received no rebates.

was less than 39. The average number of realized treatment households per village was 36.25. Appendix Figure B.2 shows the scratch card design. All scratch cards looked identical, but had a unique ID number that the research team could use to map scratch cards to treatment conditions. This mapping was unknown to the field staff; offers were only revealed when a household scratched their card in front of the enumerator. The household address and a mobile phone number was noted down when the card was distributed. This helped ensure that cards could not be used by anyone other than the recipient households.

3.5 Treatment duration

The experiment began in May 2022 and concluded in August 2023. For logistical reasons, villages were randomly assigned to 8 phases, and the offers were rolled out in a staggered manner, with village randomization stratified by phase. All treatment households were initially told that their offers would last for 5 months, based on available funding at the beginning of the experiment. However, we obtained additional funding during the course of the experiment, and were thus able to extend the offer for 2 months in all except the first two waves, which contained 35 villages. Appendix Figure B.3 shows the implementation timeline, including the scratch card distribution, water distribution, and survey data collection (described in more detail in Section 4 below).

3.6 Experiment integrity

Balance Appendix Figure C.1 provides pair-wise balance tests between each treatment arm and the control group on a series of household characteristics. Due to logistical constraints, we were unable to conduct surveys prior to the start of water distribution. We therefore test for balance using data from the endline survey on variables that were impossible or very unlikely to change as a result of our experiment: household size, presence of children in the household, the education of the household head, years the household head has lived in the village, and ownership of expensive appliances. We fail to reject balance on all variables and across all pair-wise comparisons. Because this balance test is conducted on data collected at endline, it accounts for attrition by construction.

Compliance Appendix Figure C.2 shows that while most sample villages received water deliveries for 100% of experiment months, in some cases our implementation partner made no deliveries due to staffing shortfalls. In such months water, orders are zero for all households by definition. In Section 5, we present the effects of our treatment in months where delivery

occurred, because we are interested in the effects of our treatment arms on outcomes, and neither treatment nor control conditions exist in a month where water is not available.

Pre-analysis plan This study was pre-registered through the AEA RCT Registry as AEARCTR-0010545.[20] We report the minor deviations from the PAP in detail in Appendix G.

4 Data collection and outcome variables

4.1 Record of scratch-card distribution

During our visits to all households in each treatment village to distribute scratch cards, we generated a "listing" dataset, which includes the village name, the name of the household head, whether the household was a Spring Health customer prior to the experiment, contact and address details, the offer type, and the scratch card ID, which allows us to confirm the link between a household and its treatment offer. We use these data to define the universe of households in each of our treatment villages.

4.2 Administrative data

Our main outcomes of interest concern clean water demand. We obtained administrative data on water orders from Spring Health. For every household that orders clean water—including both households who received a treatment offer and those who did not—we observe daily information on the number of bottles of water they ordered from Spring Health, and at what price. For exchangeable entitlement households, we use these data to calculate how much money they are owed at the end of each month.

Because Spring Health's administrative data are complete (i.e., they contain entries for every single bottle of water ordered in each village), and our listing dataset enumerates every household in each village, we can also infer that households who do not appear in the Spring Health administrative dataset must consume 0 litres. This yields a household-by-date panel of Spring Health water orders.

[20]The registry entry is available from https://www.socialscienceregistry.org/trials/10545.

4.3 Survey data

We administered a series of surveys with a randomly-selected subsample of 13 households in each treatment village, stratified across sub-treatments to maximize statistical power.[21] We visited each survey household for four short "high-frequency" checks and a longer endline. The first high-frequency survey normally occurred in the first or second months of the treatment start date, while the endline normally occurred in the last month.[22] Where possible we repeatedly surveyed the same households in each survey, except in cases where a household dropped out. In such cases they were replaced by selecting a backup household at random from the corresponding (sub-) treatment arm.

In each survey, we asked households about health outcomes, missed work, drinking water choices, water treatment choices in the week before the survey. We also gather data on health expenses. In the first survey we additionally collected basic household demographic information, including total number of household members, number of adults/children, monthly income and occupation. In the endline survey, we also collect more data on households including information on household savings, expenditures, liquidity, and asset ownership.

4.4 Water testing

As discussed in the background section, we conduct two types of water tests on small samples. First, we collected 19 water samples from households in our study villages (8 from open wells, 6 from tube wells, 4 from taps, and 1 Spring Health) and had water quality (including pH, heavy metals, and biological contaminants) analyzed by a government laboratory. Appendix Figure A.2 shows the results. We find substantial biological contamination among all but the Spring Health sample, and no evidence of heavy metal issues.

Second, we conducted 9 water taste tests with members of our survey enumeration team. In these tests, subjects were given a comparison cup of water (unbeknownst to the subjects, this was Bisleri, a leading bottled water brand), and then asked to compare four additional samples (provided blind, and in a randomized order) to this original sample. These samples were: tap water plus added chlorine (per the instructions on the chlorine treatment packet), Bisleri plus added chlorine, the treated water in our treatment, and treatment water plus

[21] In price discount villages, we selected 4 control households and 3 households in each discount level for surveys; in pure quota villages we surveyed 6 control households and 7 quota households; and in exchangeable quota villages, we surveyed 3 control households, 2 10% exchange households, 3 50% exchange households, 2 90% exchange households, and 3 100% exchange households. We did not survey in pure control villages.

[22] An exception are the 35 villages in the first two phases where the offer duration was shorter. Here the last survey occurred three months after the offers ended. Appendix Figure B.3 shows survey timings by calendar month for all phases.

added chlorine. Appendix Figure A.1 shows the results. Our taste testers uniformly rate chlorinated sources the lowest, and rank Spring Health the highest on average.

5 Analysis and results

In this section, we describe the specifications we use to analyze data from the experiment and summarise the main results. We begin by describing patterns of demand for clean water under our different treatment arms. Next, we turn to our results on the valuation of water. Finally, we provide survey evidence on the effects of water offers on time use, water treatment behaviors, and health.

5.1 Demand for clean water

We use administrative data on water orders to estimate demand under all of our treatment arms. Households see non-zero marginal costs under both the prices and exchangeable entitlements arms. In the first case, ordering water requires households to pay a per-unit price, and in the second, ordering water requires households to forgo an equivalent per unit cash transfer. In the free ration arm, the marginal cost is zero.

The top panel of Figure 3 shows the demand curve, plotting average water orders at each price level for the priced water arm in blue, and at each refund level for the exchangeable entitlement arm in purple. The green point at a zero price reports the average water orders for households in the free ration arm.[23] The bottom panel of Figure 3 separates net demand into changes on the extensive margin on the left (probability of a household ordering *any* water in a month) and the intensive margin on the right (conditional on ordering, how *much* do households order).

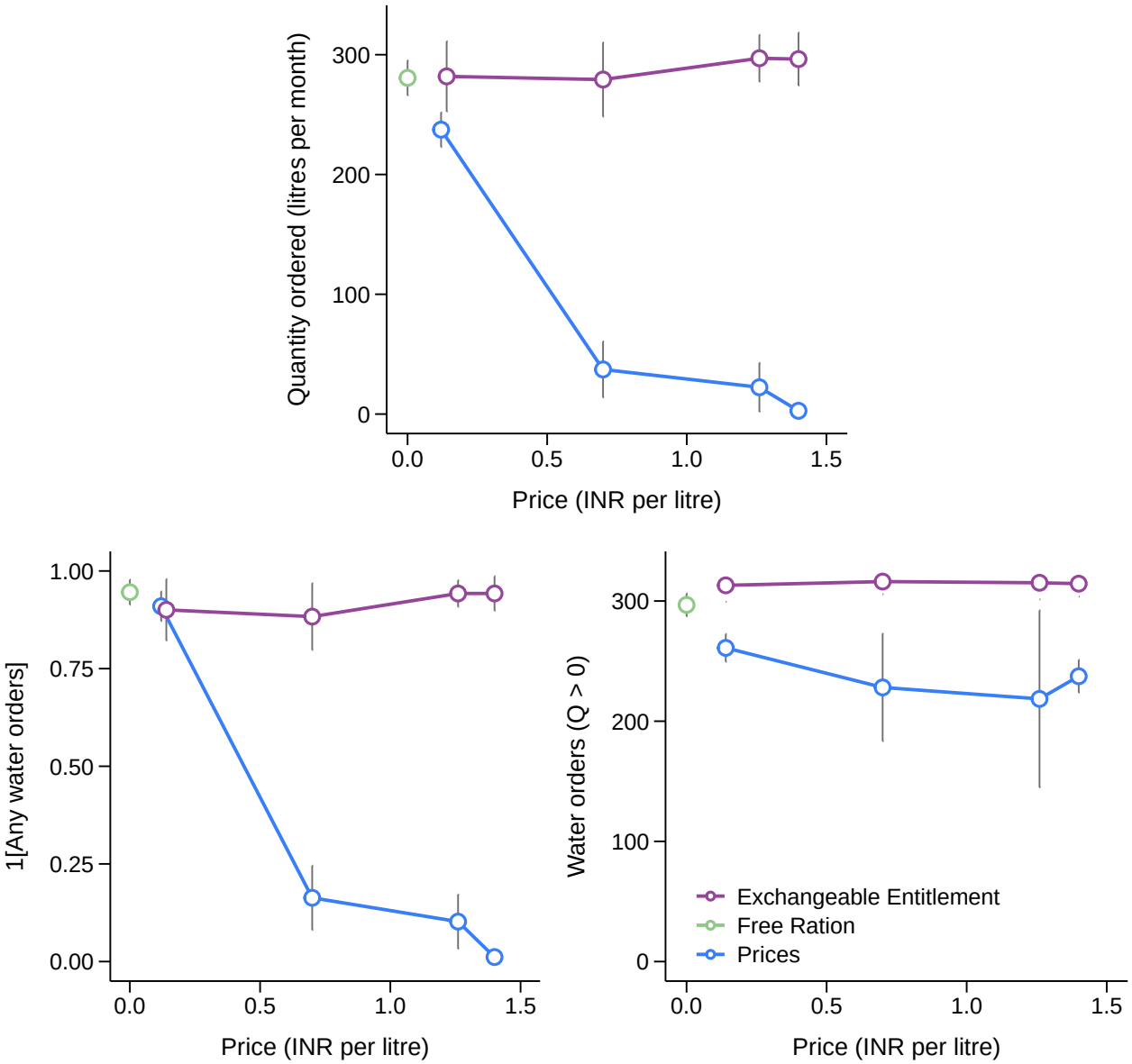
We also describe changes in demand relative to the status-quo levels in the control by estimating a set of simple regression models, described in Equation (1), and report the results in Table 1.[24]

$$\begin{aligned} Y_{it} = {} & \eta_1 \cdot 10\% \text{ discount}_i + \eta_2 \cdot 50\% \text{ discount}_i + \eta_3 \cdot 90\% \text{ discount}_i \\ & + \eta_4 \cdot 10 \text{ \% exchange}_i + \eta_5 \cdot 50 \text{ \% exchange}_i + \eta_6 \cdot 90 \text{ \% exchange}_i \\ & + \eta_7 \cdot 100 \text{ \% exchange}_i + \eta_8 \cdot \text{Free ration}_i + \eta_9 \cdot \text{One free}_i + \gamma_v + \theta_t + \varepsilon_{it} \end{aligned} \tag{1}$$

[23] For graphical clarity in Figure 3, we do not use data from the 20-litre bottle villages where prices are slightly lower. These villages are included in Table 1.

[24] In estimating Equation (1), we restrict the estimation sample to exclude months where water delivery was disrupted and households could not place orders. See Appendix C for more details on these interruptions.

Figure 3: Demand for clean water



Notes: This figure presents the demand curve for clean water. We plot monthly water orders against the price of water for households in the price arm and control (i.e., full price) in blue and against the refund amount for the exchangeable entitlement arm in purple. The green point at 0 price shows mean orders in the free ration arm. The top panel plots average water orders at different prices (or refund rates). The bottom-left panel plots the probability of ordering any water. The bottom-right panel plots quantity ordered conditional on ordering water. We show 95% confidence intervals, derived from standard errors clustered at the village level, in light gray. We jigger the INR 0.14 price point slightly to the left for visual clarity. In the experiment, both the price arm and the exchangeable entitlement arm included an identical INR 0.14 incentive level.

where the outcome Y_{it} is either an indicator equal to 1 if the household i ordered any water during the course of the month-of-sample t, or the total monthly water orders for household i in month-of-sample t in litres, Q_{it}. The treatment indicators are each listed, γ_v and θ_t are village and month-of-sample fixed effects, and ε_{it} is an error term. Identification comes from a comparison of treated consumers to untreated customers *within* villages.

Table 1: Intent-to-treat effects of clean water offers on water orders

	Any orders		Orders in litres	
	(1)	(2)	(3)	(4)
Prices (Discounts)	0.38		95.93	
	(0.02)		(7.68)	
Exchangeable Entitlement	0.90		290.79	
	(0.02)		(11.12)	
Free Ration	0.89	0.89	269.93	269.93
	(0.02)	(0.02)	(6.96)	(6.96)
Onetime 100L	0.14	0.14	13.13	13.14
	(0.01)	(0.01)	(1.98)	(1.98)
10% Discount		0.09		19.99
		(0.03)		(9.31)
50% Discount		0.15		34.08
		(0.04)		(10.84)
90% Discount		0.88		232.12
		(0.02)		(7.22)
10% Rebate		0.89		285.86
		(0.03)		(14.12)
50% Rebate		0.87		282.69
		(0.04)		(14.56)
90% Rebate		0.93		297.99
		(0.02)		(10.18)
100% Rebate		0.93		297.16
		(0.02)		(11.22)
N	239,173	239,173	239,173	239,173
Control means	0.012	0.012	2.818	2.818

Notes: This table preseents intent-to-treat effects of water offers on water orders at the monthly level, estimated using Equation (1) or a pooled version thereof. We restrict the sample to village-months where Spring Health delivered water. In Columns (1) and (2), the outcome is a binary indicator for the household having ever bought water during the month. In Columns (3) and (4), the outcome is total water orders in liters per month. All regressions include village fixed effects. Standard errors are clustered by village.

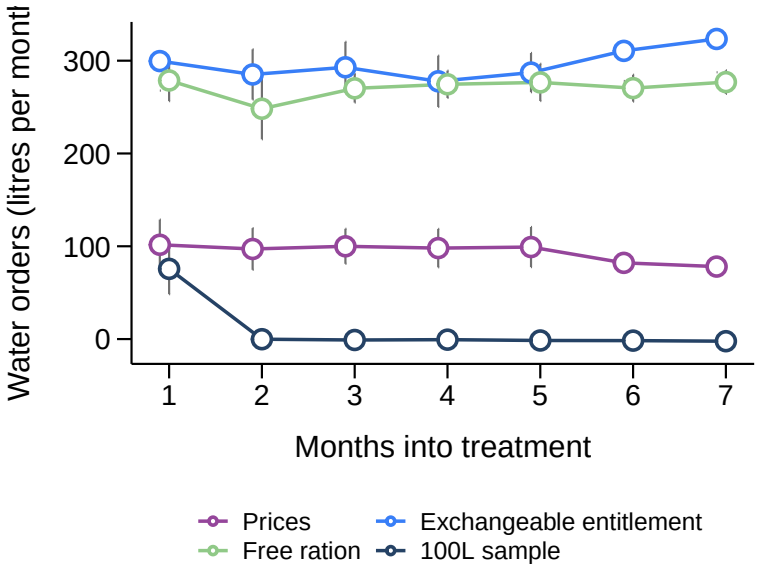
Finally, we estimate a dynamic specification to study the effects of our treatments on water orders over time, with results reported in Figure 4. We pool sub-treatment arms for

clarity and report coefficients for a fully disaggregated variant in Appendix Figure D.1.

$$
\begin{aligned}
Y_{it} = \sum_{r=1}^{7} \beta_1^r \cdot \text{Any discount}_i \times \mathbf{1}[\text{offer month} = r]_{it} \\
+ \beta_2^r \cdot \text{Any exchange}_i \times \mathbf{1}[\text{offer month} = r]_{it} + \beta_3^r \cdot \text{Free ration}_i \times \mathbf{1}[\text{offer month} = r]_{it} \\
+ \beta_4^r \cdot \text{One free}_i \times \mathbf{1}[\text{offer month} = r]_{it} + \gamma_v + \delta_t + \varepsilon_{it} \qquad (2)
\end{aligned}
$$

where $\mathbf{1}[\text{offer month} = r]_{it}$ is an indicator for being $r \in \{1, 7\}$ months into the treatment offer (such that 1 is the first month of the offer), and all other terms and sample restrictions are as above.

Figure 4: Water orders over time



Notes: This figure plots the effect of our treatments on water orders as a function of time since treatment started (with the first month offers were active set to 1), estimated using Equation (2). We pool over all price and exchangeable entitlement sub-treatments. Standard errors are clustered at the village level. 35 villages — the first enrolled in the experiment — had only 5 months of treatment, while the remainder had 7. The sample is restricted to months when deliveries occurred. Price and exchangeable entitlement points are jiggered to the left for visual clarity.

Access to clean water The intervention leads to very high clean water take-up. The bottom left panel of Figure 3 shows that the monthly probability of ordering any clean water rises as price falls, reaching approximately 90% at a price of 0.14 INR per litre (90% discounts). The monthly order probability at low but positive prices is therefore substantively and statistically identical to the share of households availing of the free ration. As shown in Column (2) of Table 1, moving households from the original market price to a 90% discount dramatically increases the probability of a monthly orders by 88 pp (s.e. 0.02). Importantly, we see no evidence of sharp changes in demand around a price point of zero (a so-called

"zero price effect"). These patterns are in stark contrast to prior work on chlorine-based water treatments, where take-up *even at zero price* is relatively low (Kremer et al., 2023), and where demand falls off precipitously as prices increase from zero (Dupas and Miguel, 2017).

Notwithstanding the variation in order probability with price, an interesting feature of demand is that *conditional* on consumption, quantity demanded is inelastic. The bottom-right panel of Figure 3 shows demand along the intensive margin. In the prices arm, conditional on ordering a non-zero amount of water, there is no statistically significant variation in orders with price. On the intensive margin, all treatment arms order substantial amount of clean water. Even at the highest price in the sample (1.4 INR per litre, 10× the lowest price), mean orders for households who consume more than zero is about 237 litres per month. The top panel of Figure 3 shows a full demand curve, combining the extensive and intensive margins, which shows quantity decreasing as price increases. As the two figures in the bottom panel show, this is driven entirely by the extensive margin.

What rationalizes the water order volumes we observe in our sample? Our results are consistent with households ordering about as much water as they need to fulfill (most of) their drinking water needs. The average household size in our experiment is 5. Consuming 1.5 (2) litres per person per day would lead the average household to use 225 (300) litres of water per month. Under all treatment arms and all prices, we see mean consumption falling within this range.[25]

These results are intriguing, because they are consistent with what we might expect to see if households are informed about the benefits of drinking clean water. It is unlikely to be very helpful from a health point of view to replace only a small share of potentially contaminated water with water from a clean source. Thus, an alternative framing of household decision-making in this setting is that households are interested in purchasing "access to clean drinking water at home," as opposed to treating each bottle as a distinct good.

Two facts are consistent with this framing. First, Appendix Table D.2 shows that our treatment offers increase the number of water sources households report using, increase the share of households drinking any clean water, and increase the share of households who report drinking *only* clean water. Second, households who are provided a 400 litre quota of free water do not use the ration in full. Table 1 shows that take-up under the free ration is 90%, and this treatment raises average consumption by 270 litres per month per household.[26] These results likely reflect the fact that although our households have limited access to clean

[25]The European Food Safety Association recommends 2 litres of water a day for men, 1.6 litres for women, and about 1 litre for 2-3 year-olds (EFSA Panel on Dietetic Products, Nutrition, and Allergies, 2010).

[26]This treatment effect translates to a population average of about 280 litres per month, or approximately 300 litres per month conditional on a non-zero order.

drinking water, they do not suffer from water shortages *per se*. In our survey data, in both treatment and control arms, households report they ran out of water (across all uses) in the prior week only 2% of the time.

The non-binding nature of the ration also suggests that households are not re-selling clean water to other households. We asked households about re-selling in the endline survey, and not a single household reported doing so. This is perhaps unsurprising in the context of clean water, where it is difficult to signal quality if it is not delivered by a trusted source in a sealed container, and where water is heavy and difficult to transport.[27]

Finally, it is notable that demand is sustained over time. Figure 4 plots dynamic effects of water orders for each treatment arm, estimated using Equation (2). Water orders in our main treatments are stable during the full experimental period. These results demonstrate that households are consistently willing to spend (or give up) money for water. This again contrasts with prior experiences using other approaches to water treatment (e.g., Ashraf, Berry, and Shapiro (2010a); Dupas et al. (2016); Berry, Fischer, and Guiteras (2020)), where many households initially took up either chlorine or water filtration but were not found to be using it in a follow-up measurement. Households in our main treatment arms exhibit similar demand for water in both the first and final month of our experimental offers, with no evidence of learning or "experience good" effects.[28]

5.2 The value of clean drinking water

Our experiment recovers two measures of valuation: (i) a willingness-to-pay (WTP) estimate from the demand curve in the prices arm, and (ii) a bound on a willingness-to-accept (WTA) estimate from the exchangeable entitlement arm.

Willingness-to-pay A measure of the population average WTP is the area under the demand curve in the top panel of Figure 3, which is also then the consumer surplus at zero price. To estimate surplus from drinking water, we calculate the area under the prices (blue) demand curve from Figure 3 using the trapezoidal method.[29] This yields an average WTP of about INR 132 for monthly clean water access — approximately 280 litres per month, where consumption at the 0 price point is given by the average water orders in the free ration

[27] In theory, a re-seller could hand over a Spring Health bottle to someone else without breaking the seal. In practice, this would render sales unprofitable, since our partner levied a substantial charge of 400 INR if water containers were not returned by the purchasing consumer.

[28] A direct test of experience good effects comes from households given a one-time free allocation of 100 litres. These consumers use this water in the first month, but revert to behaving like the control afterwards.

[29] This exercise is more flexible than making common functional form assumptions such as a logit demand system, which do not necessarily match the empirical evidence as well.

treatment arm. At approximately 1.5% of median consumption, this number is relatively high for a very poor population.

Willingness-to-accept In the exchangeable entitlement arm, households have an entitlement of 400 litres of water. They may place orders for water for free, but since unused water earns a rebate, this arm provides household the option of relinquishing the entitlement to water in exchange for a varying monetary incentive. Table 1, Column (4), shows that assignment to all rebate levels in this arm raises consumption by 280 to 300 litres per month. At the highest rebate rate (1.4 INR cash for every bottle not ordered) households continue to order a substantial amount of water (roughly 300 litres on average), and in doing so forgo on average INR 420 ($\sim$ USD 5.25) per month to 'cover their drinking water needs'.[30] This amount is a lower bound on WTA. If cash rebates had been large enough to induce households to decline water for cash, we would identify WTA precisely, but we do not observe this in our experiment. As we discuss above, demand – and therefore valuation – is sustained over time.

This is a substantial sum of money, even as a lower bound. INR 420 is about 4.7% of median monthly expenditure in the control group (INR 9000), 77% of average expenditure on tobacco and alcohol (INR 598), 2.3 times monthly expenditure on milk (INR 186), and 84% of spending on mobile bills (INR 498). It is also sufficient to cover the variable costs of providing water for free (see Section 6 for a discussion of the costs of supply). In Section 6, we argue that these measures of WTA have direct policy implications.

Comparing WTP and WTA Divergences between measures of WTP and WTA are common (Horowitz and McConnell, 2002; Tunçel and Hammitt, 2014), but our experimental design and results help rule out certain explanations. First, households in both the price and exchangeable entitlement arm face incentivized choices, ruling out issues with stated preference approaches. Second, our measures of WTP and WTA are derived in a long-running field experiment, which avoids many lab framing issues (e.g., those discussed in (Plott and Zeiler, 2005)). Third, differences in opportunity costs cannot explain the wedge because the marginal incentives in the price and entitlement arms are identical. Fourth, selection concerns are mitigated by randomized assignment, as the balance tests in Appendix Figure C.1 help confirm. Fifth, in both of these arms, households must call the company and ask for a delivery to get water. Households therefore do not have a physical stock of water that they are being asked to return for cash, so differences are unlikely to be due to

[30]This shorthand is convenient since household orders suggest they do not value the 400th unit of water much but do value the 100th unit, the former being well above the probable drinking water needs of most households and the latter well below.

loss aversion (e.g., Ericson and Fuster (2014)). Sixth, this gap is not driven by household mistrust in the exchangeable entitlement rebates. If households in this arm did not expect to be paid, we would expect that the pattern of household orders shortly *after* the first payment was deposited would differ from those observed shortly before. Appendix Figure C.3 shows that this is not the case. Finally, the gap between WTP and WTA is unlikely to be driven by information frictions, as Figure 4 shows that demand is stable over time, and that experience with clean water (in the one-time offer group) does not lead to sustained orders absent discounts.

What, then, explains the difference between WTP and WTA in our setting? Two possibilities seem plausible. First, as is common in developing-country contexts, our sample households report limited ability to undertake new expenditures at short notice, suggesting that liquidity or credit constraints could play a role (e.g., Berkouwer and Dean (2022)). Figure 5 suggests that higher income households order more water. Theory suggests another explanation: for goods lacking easy substitutes, WTA can be far higher than WTP (Hanemann, 1991; Amiran and Hagen, 2003; Hanemann, 2003).[31] This describes our setting well: given that *clean* (and perhaps also tasteless) water is the commodity of interest, households have only the imperfect substitute of point-of-use treatment, which – by revealed preference – is relatively unpopular.

Households value clean water highly Both our WTP and WTA estimates reveal that households would be willing to exchange substantial sums of money for clean water access, with an annual WTP of approximately $20, and an annual WTA of approximately $60. To contextualize our measures, this WTP (WTA) for clean water access is approximately 4.5 times (14 times) larger than the WTP for clean spring water estimated from time use in Kremer et al. (2011a).It is also dramatically higher than chlorine based point of use treatment. Adjusted to 2023 dollars, the population average WTP for chlorin (home use chlorine solution), from the demand curve reported in Ashraf, Berry, and Shapiro (2010b), is about USD 0.11 or 14 times lower that our WTP for an amount of water equal to the monthly consumption in our sample. As a comparison with other environmental quality valuations in the developing world, the WTP of households for clean water in our experiment is about 15 times higher (45 times for WTA) than the WTP of low-income Delhi residents to reduce PM2.5 by 10 micrograms per cubic meter from Baylis et al. (2024). It is slightly less than double the estimate in Ito and Zhang (2020) for how much richer Chinese households would be willing to pay to eliminate pollution generated by the Huai River heating policy. We therefore conclude that household valuation of clean water is quite high.

[31]Support for this hypothesis has been found in the lab (Shogren et al., 1994; Shogren and Hayes, 1997).

5.3 Impacts on water collection, water purification, and health

In this section, we use survey data to evaluate the extent to which clean water confers measurable benefits. We focus on three categories of outcomes. First, we measure time spent collecting water. Beyond any health benefits, the water we deliver in our intervention might lead to substantial time savings, if it were to displace trips to wells to collect groundwater.[32]

Second, we measure behaviors households undertake in order to make their water safe to drink. Theoretically one benefit of purchasing clean drinking water is that home treatment could be reduced, saving time and money, especially for boiling where fuel costs may be significant. On the other hand, given that in-home contamination remains a plausible concern, and that mixed drinking sources may be used, home treatment is probably a very useful habit to retain.

Finally, we measure health outcomes, including illness in the household, health expenses, missing work due to illness, and a series of symptoms using households' own reports. Self reported outcomes are particularly useful in this context, because perceived benefits are what matters for households' valuation of, and demand for, clean water. The health risks of drinking contaminated water are not in doubt. However, the benefits of clean drinking water may be attenuated if households practice poor sanitation behaviours at home. This matters both for valuation and for policy. If consumers are unaware that their actions are reducing or eliminating potential returns to clean water, they may value the water less highly; in turn, if improving clean water access alone is insufficient to deliver health benefits, governments may need to undertake complementary interventions.

We use data from our repeated surveys to estimate panel regressions of outcomes on our treatments. In the interests of parsimony and precision, in our main specification, we pool across sub-treatment arms.[33] Our main specification is thus:

$$\begin{aligned} Y_{it} = \eta_1 \cdot \text{Any discount}_i + \eta_2 \cdot \text{Any exchange}_i \\ + \eta_3 \cdot \text{Free ration}_i + \gamma_v + \delta_t + \varepsilon_{im} \end{aligned} \tag{3}$$

where Y_{it} is an outcome for household i surveyed in month-of-sample t, Any discount_i, Any exchange_i, and Free ration_i are treatment indicators, γ_v and δ_t are village and month-of-sample fixed effects, respectively, and ε_{it} is an error term, clustered by village.

[32] Though home delivery of clean water would not deliver time savings over piped water, only 24% of the control group reports getting their drinking water only from taps.

[33] We present intent-to-treat effects on health broken out by sub-treatment in Appendix Table D.1.

Water collection We first consider changes in time spent collecting water. The first column of Table 2 reports the results. Control households spend an average of 32 minutes per day collecting water. All three offer types lead to meaningful reductions in water collection time, with time savings of 4.8 minutes (15%, $p < 0.01$) in the price arm, 9.0 minutes (28%, $p < 0.01$) in the exchangeable entitlement arm, and 12.6 minutes (39%, $p < 0.01$) in the free ration arm.[34]

Water purification Next, we measure the effect of our offers on actions households undertake to make their water safe to drink. Column 2-4 reports effects on the probability that households report boiling, chlorinating, or straining water in the past week.[35] We find no evidence that households change their self-reported treatment behavior, and can reject relatively modest effects in all treatment arms. Columns 5 and 6 report changes in the collection time and costs of the main fuel used to boil water, conditional on reporting any boiling in that week. The point estimates suggest small-to-moderate reductions in both across all treatment arms, but they are imprecisely estimated.

Table 2: Intent-to-treat effects of water offers on water collection and purification

	Collection time (1)	Chlorinates (2)	Strains (3)	Boils (4)	Fuel time (5)	Fuel cost (6)
Prices (Discounts)	-4.77	0.00	0.02	0.00	-1.99	-5.54
	(1.81)	(0.01)	(0.03)	(0.02)	(10.95)	(10.55)
Exchangeable entitlement	-8.95	-0.01	-0.06	-0.01	-4.31	-6.07
	(2.12)	(0.01)	(0.04)	(0.04)	(5.81)	(18.88)
Free ration	-12.55	-0.01	-0.04	-0.01	-18.32	-9.95
	(2.54)	(0.01)	(0.02)	(0.02)	(14.36)	(12.63)
N	1,523	1,535	1,535	1,535	335	335
Control Means	32.339	0.032	0.195	0.096	60.267	31.614

Notes: This table presents intent-to-treat effects of water offers on water collection and purification, estimated using Equation (3). Column (1) is water collection time in minutes per day. Columns (2), (3), and (4) are binary indicators for treating water with chlorine, straining, or boiling, respectively. Column (5) is the amount of time spent collecting fuel for boiling in minutes per day, and Column (6) is amount of money spent on fuel for boiling in rupees, both for only the households who report boiling water. We restrict the sample to village-months where Spring Health delivered water. All regressions include village and month-of-sample fixed effects. Standard errors are clustered by village.

[34]Though we did not collect data on the identity of the household member who collects the water, it is extremely common in developing countries for this task to be borne overwhelmingly by women, so reductions in time spent collecting water may have gendered benefits (UNICEF (2017)).

[35]Note that the probability of reporting treatment in a given week is lower than the share of households that *ever* report using a given treatment technology. In the case of chlorine, for instance, while 13% of control households ever report treating their water with chlorine, the share reporting chlorine usage in the average survey round is only 3.2%.

Health: Intent-to-treat effects Table 3 reports intent-to-treat effects on self-reported health outcomes. The first three columns show impacts on overall health: Column 1 presents treatment effects on the number of household members reported being sick in the past week; Column 2 presents treatment effects on weekly health expenditures; and Column 3 reports treatment effects on whether any household member had to miss work due to sickness in the past week. Average illness in this population is high, with 52% of control households reporting that someone one in the family was ill in the last week; mean health spending in the control group of INR 240 (10% of average expenditure), and 20% of control households reporting that a household member needed to miss work due to illness in the past week.

We see meaningful improvements in these measures of health, though our estimates are not always precise. The point estimates of measures of reported sickness correspond to reductions of 21% (FDR-adjusted $p = 0.02$), 27% (FDR-adjusted $p = 0.15$), and 14% (FDR-adjusted $p = 0.29$) in the price arm, exchangeable entitlement arm, and free ration arm, relative to control. The estimated effects on weekly health expenses are all very noisy, though point estimates are negative and imply meaningful reductions in monthly spending – between 17% and 61% of our estimated WTA.[36] Finally, we estimate substantial declines in missed work, though again, these are only different from zero at conventional levels in the prices arm: 20% (FDR-adjusted $p = 0.05$), 45% (FDR-adjusted $p = 0.15$), and 25% (FDR-adjusted $p = 0.24$) for prices, entitlements, and rations, respectively.[37] Finally, columns 4 through 8 present results on various individual symptoms. Though these treatment effects are imprecisely measured, the broad pattern of point estimates suggests weak evidence of reductions in specific illnesses. Taken together, these results corroborate our household demand estimates: households report experiencing meaningful improvements in health as a result of our clean water offers.

In the Appendix, we take advantage of the occasional service disruptions discussed in Section 3.6 to carry out a further robustness check, re-estimating these specifications using only observations from months when water sales were disrupted. If our health effects are indeed driven by access to clean drinking water, they should be significantly attenuated or disappear entirely in months where households could not obtain this water. Appendix Table D.5 demonstrates that this is broadly the case, though with the caveat that this test relies on a relatively small and selected sub-sample of villages.

[36] Regression estimates are, per our survey question, on health spending in the past week. We thus scale by 4 when comparing to our estimated WTA for monthly clean water access.

[37] We did not test for infant mortality, as our sample is too small to detect effects on this rare outcome (Kremer et al., 2023).

Table 3: Intent-to-treat effect of water offers on health

	Sickness	Health expenses	Missed work	Symptoms				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Prices (Discounts)	-0.11	-18.25	-0.04	-0.03	-0.02	-0.02	-0.02	-0.04
	(0.04)	(21.90)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)
	[0.02]	[0.41]	[0.05]					
Exchangeable entitlements	-0.14	-64.49	-0.09	-0.03	-0.06	-0.04	-0.02	-0.02
	(0.08)	(53.87)	(0.05)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
	[0.15]	[0.23]	[0.15]					
Free ration	-0.07	-30.39	-0.05	0.00	-0.01	-0.03	0.00	-0.02
	(0.06)	(37.00)	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)	(0.03)
	[0.29]	[0.41]	[0.24]					
N	4,670	4,433	4,670	4,670	4,670	4,670	4,670	4,670
Control Means	0.516	239.507	0.2	0.044	0.159	0.108	0.053	0.223

Notes: This table presents intent-to-treat effects of our treatment offers on health outcomes, estimated using Equation (3. In column (1), the outcome is the number of household members being sick in the past week. In column (2), the outcome is household spending on health in the past week in INR. In column (3), the outcome is an indicator for the number of household members missing work due to illness in the past week. In columns (4)–(8), the outcome is an indicator for a household member reporting symptoms of vomiting, fever, stomach ailments (gastric pain/abdominal pain/diarrhea), flu symptoms (cough/congestion/headache/fatigue), and other symptoms (e.g., skin infection, joint pain, etc.). We restrict the sample to village-months where Spring Health delivered water, and drop the top 1 percent of health expenses to remove large outliers. All regressions include village and month-of-sample fixed effects. Standard errors are clustered by village. FDR-adjusted p-values in brackets.

Health: Local average treatment effects The intent-to-treat estimates reported in Table 3 describe the effect of our water *offers* on health, rather than the effects of clean water itself. Thus, we use two-stage least squares to estimate the local average treatment effect of drinking clean water (not necessarily exclusively) on health, where the former comes from survey responses. We estimate separate local average treatment effects for each experimental arm:

$$
\begin{aligned}
1[\text{Drinks clean water}]_{it} &= \eta \cdot \text{Any discount}_i + \gamma_v + \delta_t + \varepsilon_{it} \\
1[\text{Drinks clean water}]_{it} &= \eta \cdot \text{Any exchange}_i + \gamma_v + \delta_t + \varepsilon_{im} \\
1[\text{Drinks clean water}]_{it} &= \eta_1 \cdot \text{Free ration}_i + \gamma_v + \delta_t + \varepsilon_{im} \\
\text{Y}_{it} &= \theta_1 \cdot \widehat{1[\text{Drinks clean water}]_{it}} + \gamma_v + \delta_t + \varepsilon_{it} \qquad (4)
\end{aligned}
$$

where $1[\text{Drinks clean water}]_{im}$ is an indicator for whether the household reports drinking any clean water in the survey, all other terms are defined as in Equation (3), and all sample restrictions are identical. Table 4 reports the results.

As expected, the local average treatment effects are larger than the intent-to-treat estimates reported in Table 3. We estimate that drinking clean water substantially reduces illness: The point estimates in the table correspond to reductions equivalent to 62% of con-

Table 4: Local average treatment effect of clean water on health

	Sickness (1)	Health expenses (2)	Miss work (3)
Panel A: Prices (Discounts)			
Drinks Treated Water	-0.32	86.05	-0.12
	(0.13)	(176.97)	(0.04)
	[0.04]	[0.64]	[0.04]
N	3,254	3,254	3,254
Panel B: Exchangeable entitlement			
Drinks Treated Water	-0.18	-0.35	-0.13
	(0.09)	(78.88)	(0.05)
	[0.08]	[1.00]	[0.07]
N	3,063	3,063	3,063
Panel C: Free ration			
Drinks Treated Water	-0.12	-68.55	-0.08
	(0.09)	(154.34)	(0.04)
	[0.34]	[0.67]	[0.25]
N	2,752	2,752	2,752
Control Means	0.516	239.507	0.2

Note: This table reports instrumental variable estimates of the effect of a household reporting drinking clean water on health outcomes, estimated using Equation (4). In column (1), the outcome is the number of household members being sick in the past week. In column (2), the outcome is household spending on health in the past week in INR. In column (3), the outcome is an indicator for the number of household members missing work due to illness in the past week. We restrict the sample to village-months where Spring Health delivered water, and drop the top 1 percent of health expenses to remove large outliers. Each panel is a separate regression. All regressions include village and month-of-sample fixed effects. Standard errors are clustered by village. FDR-adjusted p-values in brackets.

trol (0.3 fewer sick individuals per week on average, FDR-adjusted $p = 0.04$), 36% (0.18 less sick persons, FDR-adjusted $p = 0.08$), and 23% (not different from zero) in the discount, exchangeable entitlement, and free ration arms, respectively. In the IV estimation, our health expenditure estimates become so noisy as to be uninformative. We also find large negative IV effects of drinking clean water on missing work in the price and exchange arms: 12 pp (59% of control, FDR-adjusted $p = 0.04$), 13 pp (66%, FDR-adjusted $p = 0.07$), and 8 pp (40%, FDR-adjusted $p = 0.25$) in the discount, exchangeable entitlement, and free ration

arms, respectively. Again, we document clear evidence that clean water improves household health.

Interestingly, the health benefits of clean water are both larger and more precise in the two treatment arms that involve financial stakes: the price arm and the exchangeable entitlements arm, while our estimated treatment effects are smaller in the free ration arm.[38] Though we do not have the data to speak to this directly, it may be the case that households who face monetary costs for clean water are more likely to take care to keep it clean, or to engage in complementary sanitation behaviors. We can however, rule out that differences between the exchangeable entitlement arm and the free ration arm are driven by selection, as approximately 90% of consumers order water in both regimes.

5.4 Treatment effect heterogeneity

In this section, we explore heterogeneity in our treatment effects across our various outcome measures. We begin with valuation. Due to extremely high take-up in the exchangeable entitlements arm, there is limited scope for heterogeneity in WTA. However, as take-up varies by offers in the prices arm, we can estimate heterogeneity in WTP.

To do so, we split our main sample based on the few covariates that are determined before the experiment, re-estimate our demand curves as described previously, and calculate the area under these curves to obtain population average WTP for each sub-sample.[39] Specifically, we measure heterogeneity according to whether the household has (i) A child below 5, (ii) A household head with more than primary education, (iii) Above median income, or (iv) A piped water connection. Figure 5 shows the WTP estimates for each of these subgroups.

There is little evidence of the presence of a small child or education levels mattering for household WTP. However, we find that WTP is meaningfully higher among households with above-median monthly income and for households with a piped water connection. It is unsurprising that households with higher incomes have higher WTP, and indeed, prior studies have shown that richer households are willing to pay higher amounts for clean air (Ito and Zhang, 2020) and electricity (Lee, Miguel, and Wolfram, 20220). This may reflect the notion that WTP and ability to pay are in better alignment in high-income households (for example, owing to the presence of liquidity constraints), or simply that clean water is a normal good, and poorer households would prefer to reserve their limited income for even more basic needs (Greenstone and Jack, 2015).

[38] Appendix Tables D.1 presents separate effects for each sub-treatment. Our health intent-to-treat effects are strongest in the groups facing the highest financial incentives.

[39] Appendix Table D.3 presents the underlying heterogeneous treatment effects on water orders.

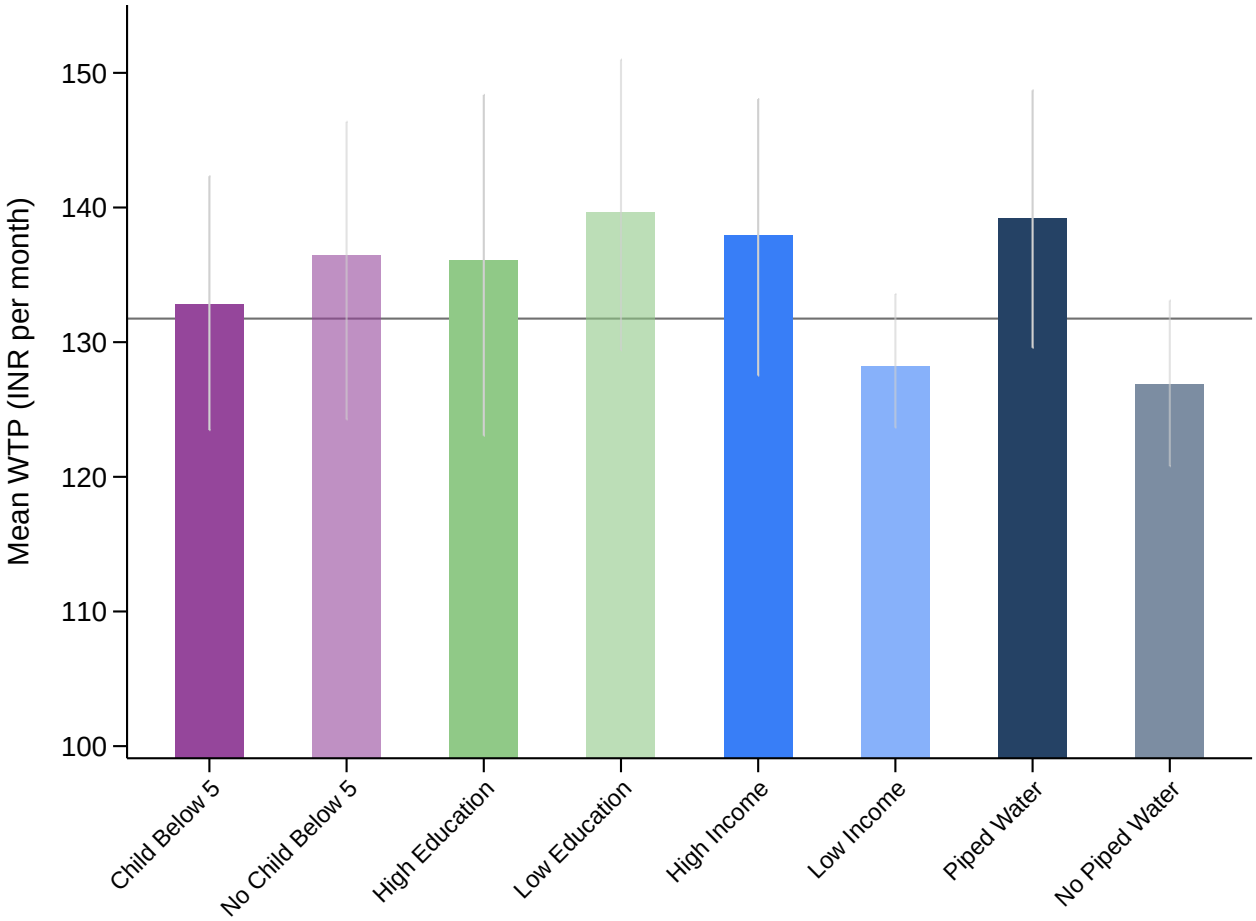
The fact that households with piped water have a higher WTP than households without is perhaps on first glance surprising. However, two simple explanations can rationalize this result. First, in order to receive piped water, households in our sample must typically expend both effort and funds.[40] Thus, this dimension of heterogeneity could reflect the same income dimension as discussed above. Second, households who are willing to pay for a tap are likely to value the benefits of tap water — many of which are shared by Spring Health's model — highly, explaining their large WTP.

These results provide evidence that a piped water connection is not viewed by many households as a satisfactory substitute for clean drinking water. The evidence on water quality presented in Appendix Figure A.2 suggests that piped water is not automatically a solution to the lack of clean drinking water from the standpoint of a technocratic planner. The evidence from WTP measures presented here suggests it is not seen as one by households either. It is important to note here that our results do not imply that piped water in itself is not a worthwhile investment, nor do they preclude high WTP or WTA for piped water (see, e.g., Devoto et al. (2012)). There are many benefits to piped water even if it is not safe to drink, including convenience and access to a large volume of water for many end uses. Our results do, however, demonstrate that piped water alone may not meet the clean water needs of many consumers.

Heterogeneous health effects In Appendix Table D.4, we present results examining heterogeneity in health outcomes. We do see statistically significant evidence indicating that the health benefits of treatment in the prices arm are concentrated amongst households with small children (the relevant point estimates for the other treatment arms go in the same direction but are less precisely estimated). We take this as further evidence that our health effects are driven by clean water access, as children are likely the household members who are most susceptible to diseases such as diarrhoea, and the presence of ill children can easily lead parents to miss work.

[40] This normally consists of a subsidized connection fee and sometimes a subsidized monthly tariff. In the case of rural connections this may be paid to a village-level managing institution, in the case of municipal connections to a water utility.

Figure 5: Heterogeneity in willingness to pay for clean drinking water



Notes: Each bar shows the average WTP for the subset of survey households that report a particular value of four key covariates: the presence (or absence) of children below 5 (purple), whether the household head has (or has not) completed primary education (green); whether the household has above- (or below-) median income (light blue); and whether the household ever reports using piped water or not (navy blue). To compute WTP, we estimate subsample-specific demand curves, as in Figure 3, and calculate the area under the step-wise demand function using the trapezoidal method. Error bars show 95% confidence intervals of the mean WTP, estimated using a village-wise block bootstrap. The horizontal line shows the population-average WTP, calculated using the demand curve in Figure 3.

6 Discussion

Drinking water policy and household valuation A key contribution of this experiment is we are able to estimate a lower-bound of a WTA measure of the value of clean water via revealed preference. Our lower bound estimate of INR 420 per month for clean water access is substantial, which has important policy implications. Given this high valuation, a government choosing between cash transfers and the direct provision of clean drinking water (or of public goods that indirectly lead to cleaner water) may be able to justify significant expenditures on clean water.

Cash transfer programs exist in more than 120 low- and middle-income countries (Banerjee et al., forthcoming), covering more than 1 billion people around the world (Niehaus and Suri, 2024). India is no exception, with flagship central schemes such as Pradhan Mantri Kisan Samman Nidhi (PM-Kisan) for farmers and Pradhan Mantri Matru Vandana Yojana for expectant mothers (Weaver et al., 2024). Individual state schemes also distribute large sums of cash to poor households. On launch, Odisha's flagship cash transfer program for rural farmers, Krushak Assistance for Livelihood and Income Augmentation (KALIA), provided INR 20,000 per household per year (Dhillon (2019)).

Given budget constraints and the restrictions on borrowing that sub-national governments may face, such transfers imply reduced spending on other dimensions. Our experiment suggests households would be willing to forgo at least INR 420 per month for free access to clean water sufficient to cover all their drinking needs. This amount of money would more than cover the variable costs of clean water provision. Importantly, it is also significantly lower than existing cash transfer schemes run by both state and central governments.

Put differently, the government need not raise taxes in order to pay for clean water. Instead, it would be possible to implement precisely the trade-off households reveal themselves as being willing to accept, by devoting some erstwhile cash transfer funds to instead pay for clean drinking water using the technology we study.

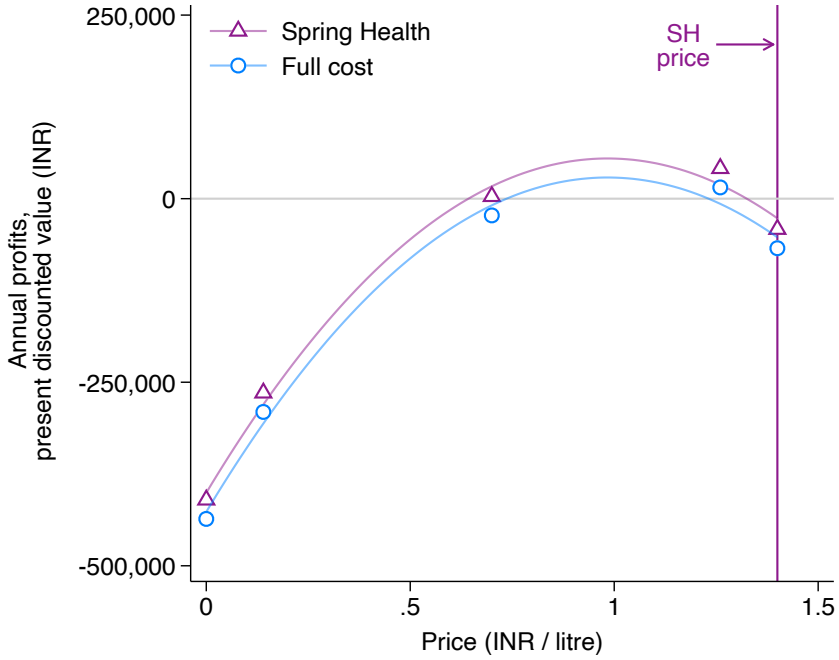
Cost-effectiveness We do a back-of-the-envelope calculation, based on Kremer et al. (2023)'s meta-analysis of the effects of clean water on infant mortality, to estimate the cost per DALY of clean water delivery. Aggregating over 18 studies, Kremer et al. (2023) estimate that clean water reduces infant mortality by 24%, and calculates costs per DALY of between \$27 and \$66.[41] We do not measure infant mortality directly, because we would not have been powered to detect meaningful effects, so we take the meta-analysis estimate as given. However, home delivery of clean water in our intervention induces greater take-up of water treatment than chlorine (2.4× as much when treatment is free), but does so at a higher cost (6× higher when treatment is free). We therefore scale the cost-per-DALY by (6/2.4), implying an estimate of between \$68 and \$165 per DALY. While more expensive per DALY than chlorine, these numbers suggest that home delivery of clean water remains highly cost-effective. Even at the top end, the cost per DALY for our intervention is more than 14 times lower than the WHO-proposed 1x GDP per capita cost-effectiveness threshold (Bertram et al., 2021)[42], and well below the World Bank's proposed threshold of \$500 per DALY for LMICs (Jamison et al., 2018).

[41] 13 of the studies were water chlorination treatments; three used water filtration; one used spring protection; and one solar disinfection.

[42] This calculation uses \$2,730 as India's per-capita GDP, per IMF (2024).

Scaling clean water delivery In this experiment, we compute the partial equilibrium impacts of clean water access in 120 villages. However, solving the global clean water access problem will require interventions that can be delivered at scale. While our experiment does not speak to the logistics of scale directly, features of our intervention suggest that this may be entirely feasible. First, our partner organization, Spring Health, is a private company which has been delivering clean water in Odisha for more than 10 years. Second, we use cost data from Spring Health to compute the net present value of profits under different price levels. We plot the results in Figure 6.[43]

Figure 6: Profitability of clean water at different prices



Notes: This figure plots the present discounted value of annual profits from home delivery of clean water. We assume a clean water plant has a 10-year lifespan, a discount rate of 5%, an average village size of 450 households, and average consumption among water buyers of 237 litres / month (per the control group in the price regime), and fit the zero price point using take-up in the free ration arm. We use our estimated extensive margin demand curve to calculate the share of households that order at each price level, which affects total revenue and total variable costs. In light blue circles ("full cost"), we plot profits at each price level calculated as total revenue less one-time capital costs, monthly fixed costs, and total variable costs. In purple triangles ("Spring Health"), we subtract off capital costs, as Spring Health receives donor funding to cover these costs. Curves are quadratic fits. Finally, we plot the Spring Health status quo price (INR 1.4 / litre) as a purple vertical line.

Figure 6 reveals that home delivery of clean water can be privately profitable. Moreover, under the status quo price of INR 1.4 / litre, Spring Health is only just below the profitability cutoff we estimate. Their continued existence is unsurprising, given that they also have additional revenue streams, especially from selling carbon credits they earn using solar power to treat their water. One benefit of this project for our partner was understanding the demand curve which suggests slightly lowering prices may be worthwhile. However, offering

[43]See Appendix Table F.1 for the full set of assumptions we use in these calculations.

the low prices required for widespread take-up is not privately profitable. Doing so would therefore require subsidies, but our results suggest that governments would not need to cover the fixed costs of water treatment plants. Even were they to do, perhaps in the interests of scaling up rapidly, the costs are substantially lower than building an extensive piped water network. As a result, we expect that home delivery of clean water is likely to be feasible to scale.

Mechanisms for allocating water Finally, our results shed light on whether policymakers should use rations or price mechanisms to allocate clean water, in the tradition of Weitzman (1977). While standard economic theory tells us that prices optimally allocate resources, rations and in-kind transfers are widely used: more than 90% of low-income countries' social safety nets include in-kind transfers (Gentilini, Honorati, and Yemtsov, 2014). These zero-price mechanisms are used to distribute a series of subsistence goods, including food (Cunha, de Giorgi, and Jayachandran, 2019; Gadenne, 2020; Gadenne et al., forthcoming) and electricity (Jack and Smith, 2020). While determining which approach is preferred is a complicated public finance question (Gadenne and Singhal, 2024), the classic argument against rationing is that (i) rations fail to allocate scarce resources to those with the highest demand; and (ii) rations lead to waste.

In our setting, preferences for clean drinking water appear to be stable, relatively homogenous, and we find little evidence of waste under free distribution, in the sense that households order less than the ration limit. Under these conditions, free distribution may be a reasonably efficient way of allocating clean water to the poor. This approach can be logistically easier than subsidized pricing and our experiment suggests households value water highly enough to justify such a policy. Furthermore, over and above household valuation, there are arguably positive externalities to ensuring universal access to clean drinking water (e.g., reduced transmission of water-borne diseases).

That said, from Table 4, our local average treatment effects on health appear to be strongest in the discount group. This suggests that higher-WTP households may enjoy larger health benefits from clean water (as also shown for rural electrification in Lee, Miguel, and Wolfram (20220)). Replicating and investigating this result in future studies would be valuable, especially because other important preventive health investments have not shown higher benefits when sold rather than given away (Cohen and Dupas, 2010). One reason why water may be different is that maximizing the health benefits of drinking water likely requires complementary investments from households, including proper sanitation behaviour and keeping utensils and containers clean.

7 Conclusion

In this paper, we conduct a randomized trial to study a novel approach to addressing the global clean water access problem: home delivery of safe, tasteless drinking water. We use this experiment to provide two valuation measures. We produce what are, to the best of our knowledge, the first direct experimental revealed-preference measures of both households' willingness-to-pay for clean water, and their willingness-to-accept.

Both our willingness-to-pay and our willingness-to-accept measures demonstrate that households value clean drinking water highly. Our estimated willingness-to-pay is several times higher than previous work on water in the literature, which has either estimated this quantity indirectly or instead observed demand for point-of-use purification as opposed to clean water. We show that a lower-bound on the willingness-to-accept is higher still, consistent with classic theoretical predictions for valuation in the absence of substitutes. Our results highlight both the importance of directly measuring the good in question when valuing environmental quality and the importance of measuring both willingness-to-pay and willingness-to-accept in order to interpret consumer demand in environmental and development settings.

In addition to our findings on valuation, our intervention generated near universal and sustained take-up at low prices. Although we are unaware of a similar evaluation in the literature, our intervention is informed by an emerging private market in similar solutions targeted at the poor. We therefore suggest that decentralized treatment and home delivery of clean water should be given serious consideration as a solution to arguably one of the most important health risks of our time — lack of access to clean drinking water.

This approach to increasing clean water access appears to be both sustainable and scaleable. The lower-bound estimate of the willingness-to-accept in our setting is large enough to justify replacing some portion of existing cash transfers with free home delivery of clean water, which could plausibly be done without raising additional tax revenue. Moreover, cost data from our implementation partner suggests that water sales using decentralized treatment and home delivery can even be privately profitable at high prices. As a result, governments could likely scale clean water access substantially simply by providing rebate vouchers to private providers in order to reduce consumer prices to zero.

Of course, our paper does not suggest that there is no role for the two leading approaches to solve the drinking water problem: (i) chlorine treatment at home, which is more cost effective on the margin, but has struggled with very low take-up; and (ii) piped water, which provides many convenience and quantity benefits but has been held back by high costs and an inability to maintain quality. However, it is clear that neither option is a universal solution

in the short or medium term. Consequently, we believe it would be very valuable to build a greater body of evidence on directly and conveniently providing drinking water at home, cleaned using decentralized treatment.

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Appendix for:

The value of clean water: Experimental evidence from rural India

Fiona Burlig, Amir Jina, and Anant Sudarshan

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A Additional context

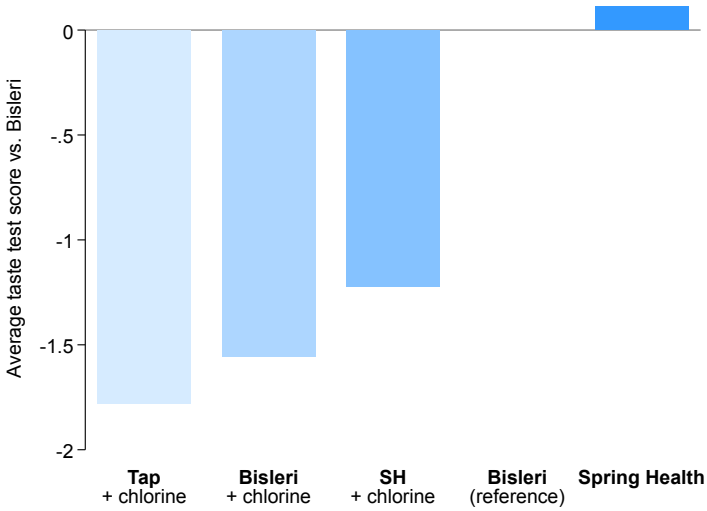
A.1 Taste testing

We conducted a set of taste tests with five different water samples where the testers included 9 local survey enumerators. The purpose of these tests was to evaluate whether there was any *indicative* evidence in our setting for the commonly cited concern about the taste of water purified with point-of-use chlorine tablets or solution. These tests had separate IRB approval from the University of Chicago under Protocol No. IRB23-1363.

Subjects were asked to drink a cup of water labeled "Sample A," (this was Bisleri, a leading bottled water brand) and think of it as a score of a 5 on a 1-10 taste scale. Then, subjects received four more blind water samples ("B" through "E"), presented in a randomized order, and asked to rate each sample on the same 1-10 scale. Respondents could return to Sample A whenever they wanted. The other samples were: Bisleri water where we added locally-available chlorine treatment solution added as per the dosing instructions on the packet; tap water with chlorine added; Spring Health water with chlorine added; and regular Spring Health water.

Appendix Figure A.1 plots the results of the taste test. Households rated all chlorinated sources at least 1 full point worse than Bisleri, and rated the treated water of our treatment (Spring Health) slightly better than Bisleri. 100% of respondents reported that one of the chlorinated water samples was their least preferred, and none reported that a chlorine-treated source was their most preferred.

Figure A.1: Water taste tests

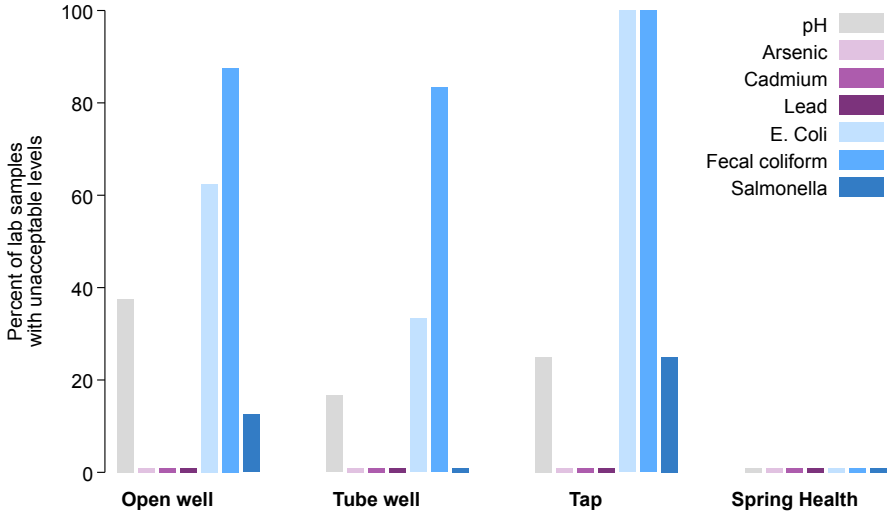


Notes: This figure plots the results from 9 water taste tests we conducted in Odisha (with separate IRB approval from the University of Chicago, IRB23-1363). All respondents were provided a sample of Bisleri, a standard bottled water. They were then provided four other sources in a randomized order: tap water treated with chlorine (per packet treatment instructions); Bisleri water treated with chlorine; Spring Health water treated with chlorine; and Spring Health without chlorine. Subjects were asked to compare each water source to Bisleri on a 1-10 scale, with Bisleri set to 5. Here, we plot the mean difference between the score of each source and the reference Bisleri. 100% of respondents ranked a chlorine-treated water source last.

A.2 Lab water quality testing

We sent 19 water samples, drawn from our treatment villages, for water quality testing at the State Water Testing Laboratory R.W.S&S. This laboratory is used by the Odisha government for official water quality measurements. We collected 8 open well samples, 6 tube well samples, 4 tap water samples, and 1 Spring Health sample. Each sample was evaluated on heavy metals and biological contaminants. Appendix Figure A.2 plots the share of samples deemed "unnacceptable" (i.e., worse than the "acceptable" threshold) across open wells, tube wells, taps, and Spring Health. None show evidence of heavy metal contamination. All but the Spring Health water show at least some unacceptable pH levels and biological contamination. Our implementation partner Spring Health also tests their water at regular intervals so this exercise was primarily intended to be informative about other sources.

Figure A.2: Water lab testing results



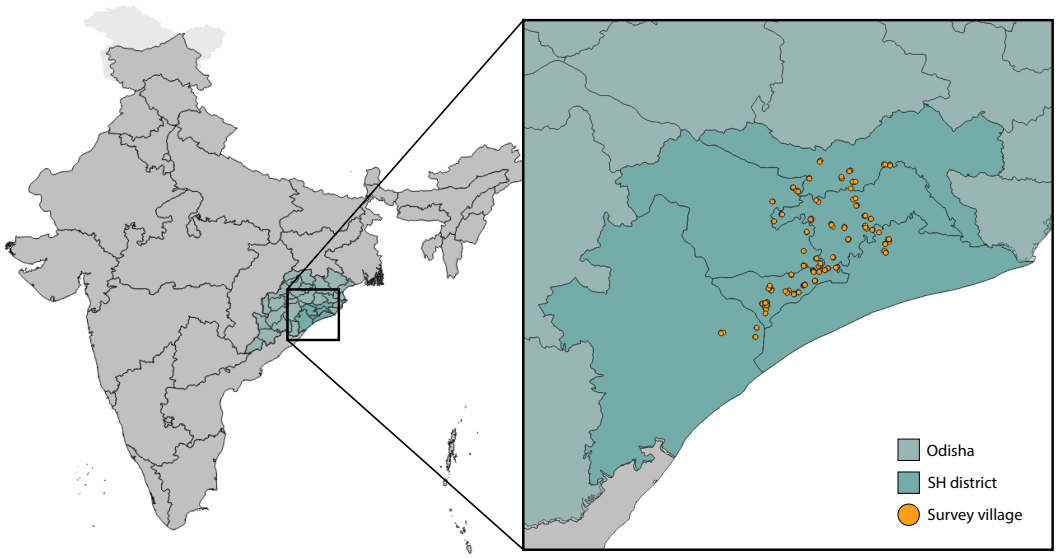
Notes: This figure plots the results from testing water sampled in our experimental villages. Lab testing was conducted by the State Water Testing Laboratory R.W.S&S, Odisha, in Bhubhaneshwar. We collected 8 open well samples, 6 tube well samples, 4 tap water samples, and 1 Spring Health sample. Samples were classified as "unacceptable" per lab thresholds for each test. Bars show the share of samples deemed unacceptable for pH (gray), heavy metals (shades of purple), and biological contaminants (shades of blue).

B Additional experiment details

This Appendix presents additional details about the experimental design and implementation. Appendix Figure B.1 plots the location of our study sample within India. The extruded view shows these districts in the context of Odisha, with the 120 treatment villages shown in orange circles.

Within each treatment village, we randomized households into sub-treatments or the control using scratch cards. The top panels of Appendix Figure B.2 shows a sample scratch card. All scratch cards were identical on the outside, save a unique ID number that linked

Figure B.1: Study village locations

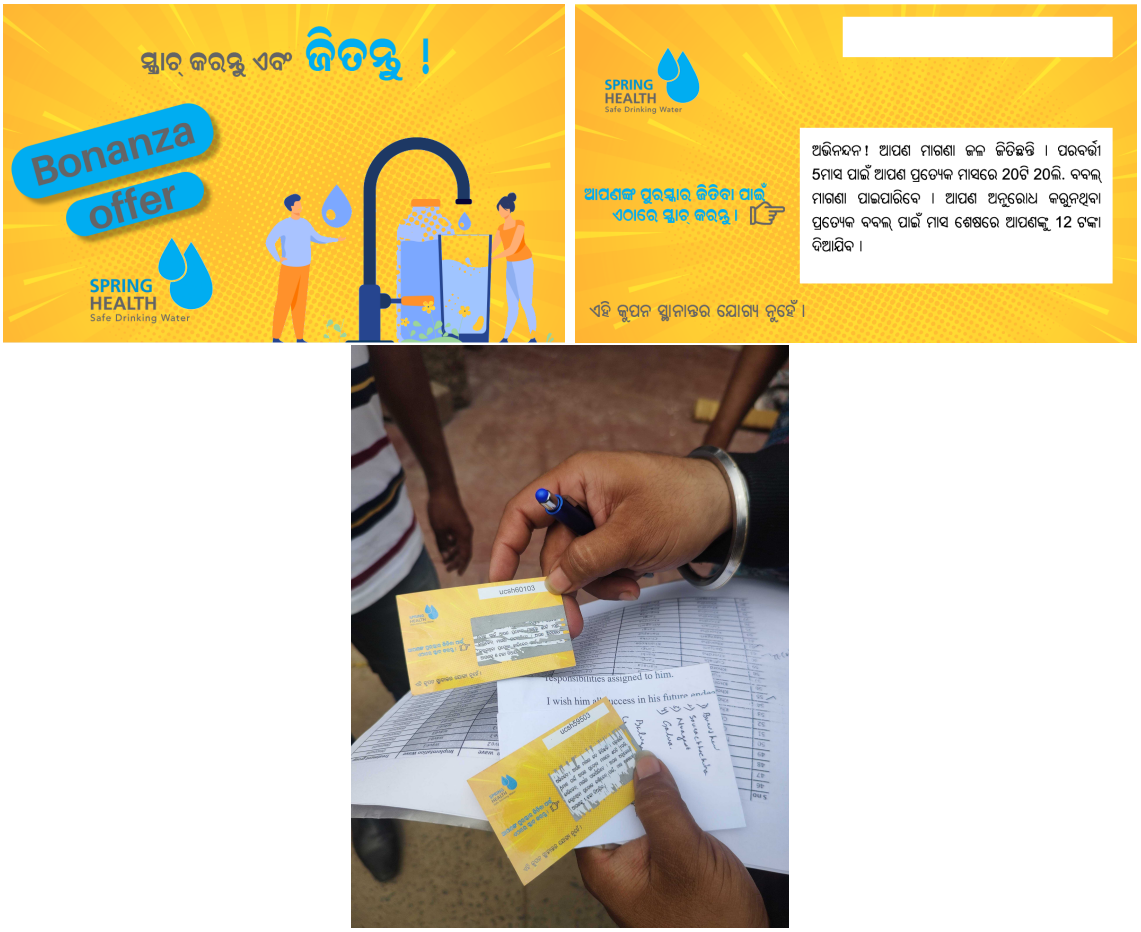


Notes: This figure plots the location of our study villages within India. The green-shaded districts plot the state of Odisha. Our experiment took place in the six dark-green Spring Health districts plotted in the extruded view. The precise location of our experimental treatment villages (i.e., those where we conducted surveys, excluding the pure control) are denoted by orange circles.

scratch cards to treatment status. This mapping was known to the research team but not to the field staff. The bottom panel of Appendix Figure B.2 shows a photo of the scratch card in use.

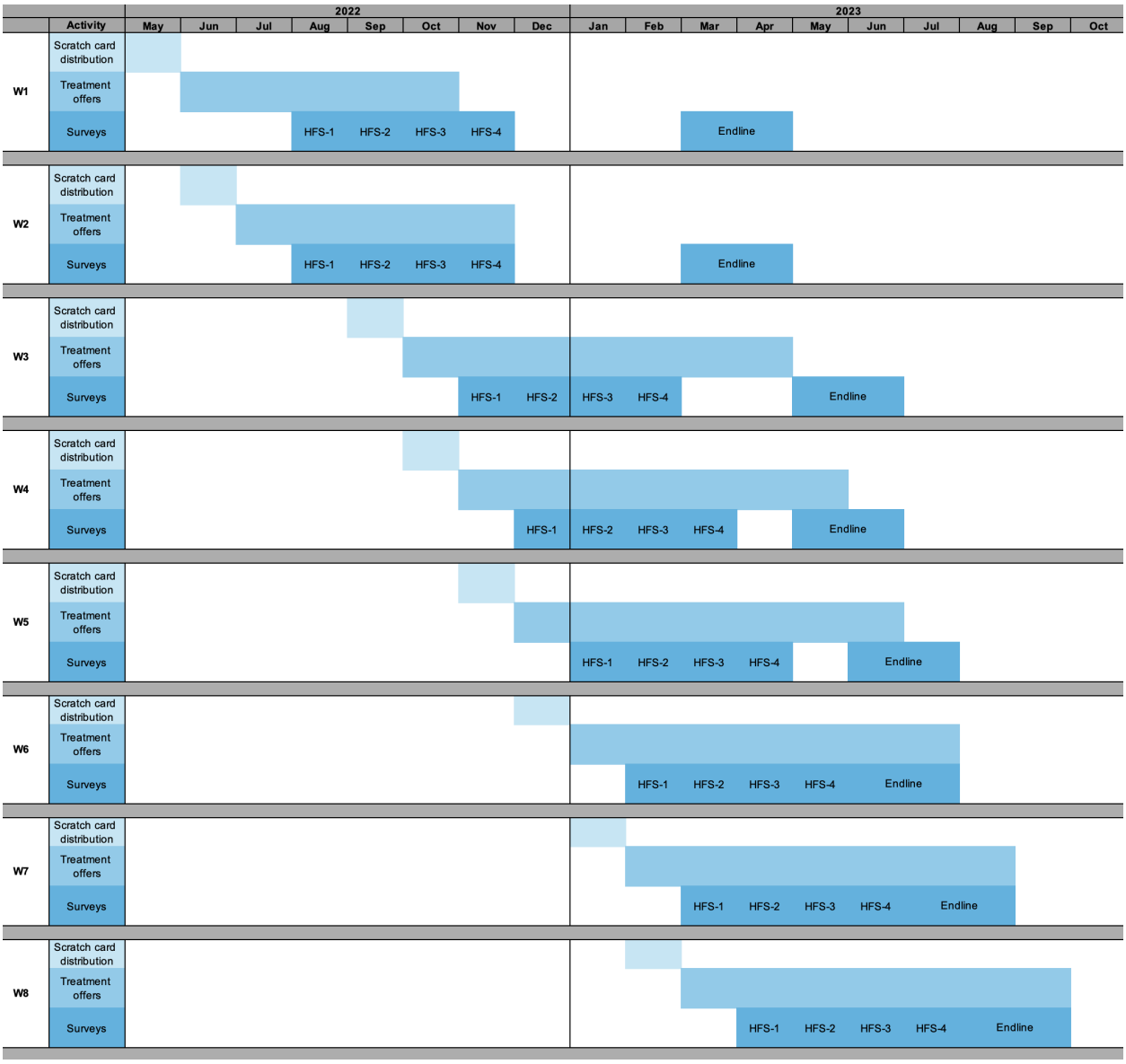
Our experiment took place between May 2022 and October 2023. Appendix Figure B.3 shows the experiment timeline in the form of a Gantt chart. For logistical reasons, the sample was divided into phases or"implementation waves" (W1–W8 on the chart). At the start of each wave, scratch cards were delivered to households, with treatment offers beginning the following month. All treatment households were told their offers were valid for five months. As we obtained additional funding, at the end of these five months, we informed waves W3–W8 that their offers would be extended by two months. We also conduct five surveys (four short "high-frequency" checks designed to capture health and a longer endline) with a randomly-selected subset of households in each village. These are described in more detail in Section 4.

Figure B.2: Sample scratch card



Notes: This figure shows the promotional scratch card used to randomize households into treatments. The top panels show an example scratch card. On the left, we show the front of the card, which is common among all offers. On the right, we show the back of the card, which differs across offers (the white portion was hidden behind a scratch-off cover). The bottom panel shows a photograph of the real scratch cards. Each scratch card contained a unique ID, known to the research team but not to the field staff.

Figure B.3: Experimental timeline



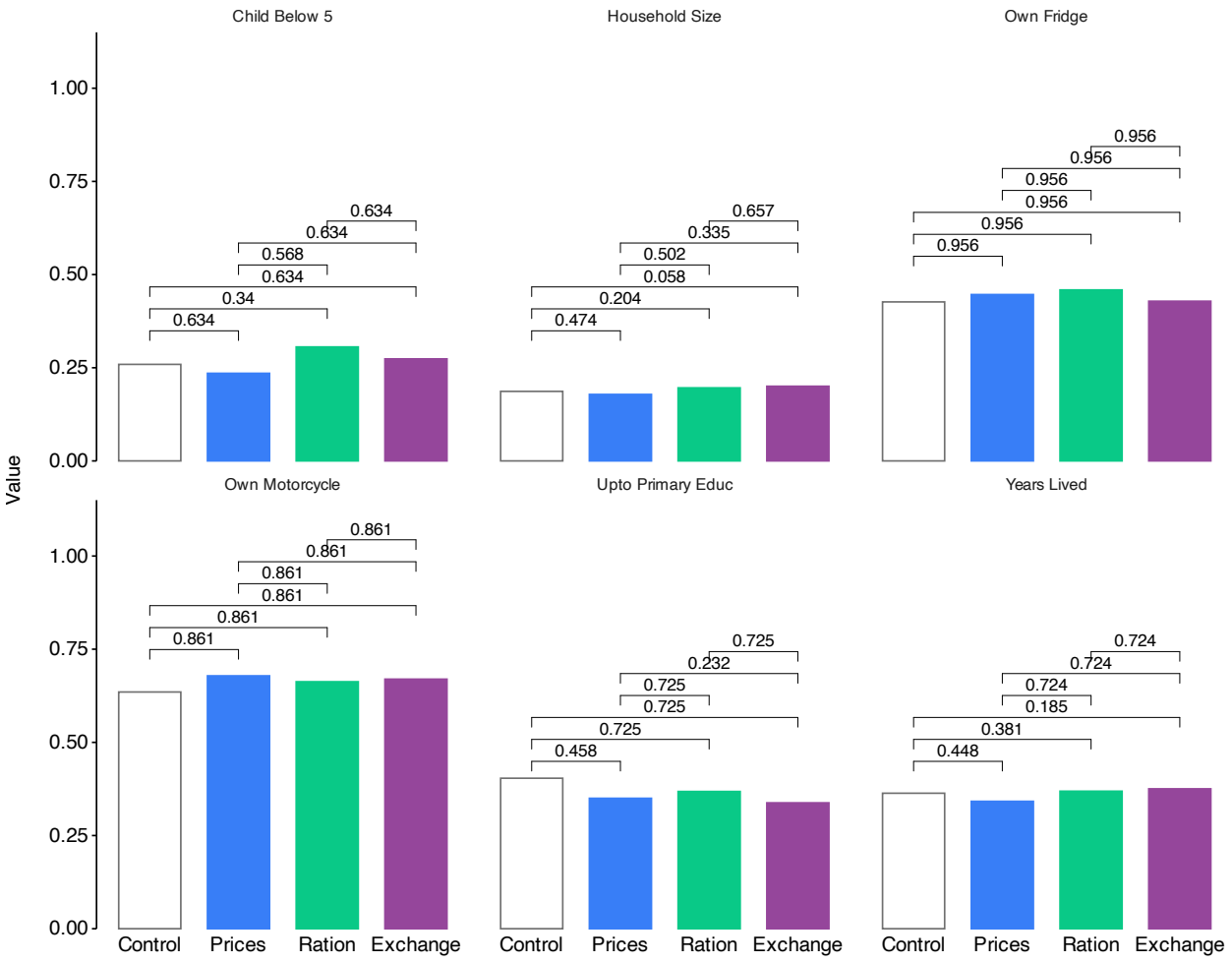
Notes: This figure shows the experimental timeline in the form of a Gantt chart. W1–W8 refer to "implementation waves," the staggered treatment roll-out. Randomization was stratified by wave. This figure shows when the scratch card distribution (and thus, the listing data collection) took place; the 5 (W1, W2) or 7 (all other waves) months of treatment offer validity; and the timing of all surveys. We conducted four high-frequency surveys (HFS-1 – HFS-4), and a longer endline survey for all villages in each wave.

C Experimental integrity

This section discusses the integrity of the experiment, including balance and compliance.

Balance Due to logistical constraints with our partner organization, we were unable to conduct a baseline survey prior to the start of the experiment with our earliest survey conducted during the first month of offers. We therefore test for balance using using data from our endline survey on demographic and other variables that we do not expect to change as a result of our treatments: household size, whether the household contains young children, the household head's education level, years the household head has lived in the village, and ownership of expensive appliances. Appendix Figure C.1 presents the means of these variables for each experimental group, p-values from pair-wise balance tests between each group. We find neither economically meaningful nor statistically significant differences between treatment arms on any variables. Because we check for balance using the endline data, the results presented in Appendix Figure C.1 account for attrition by construction.

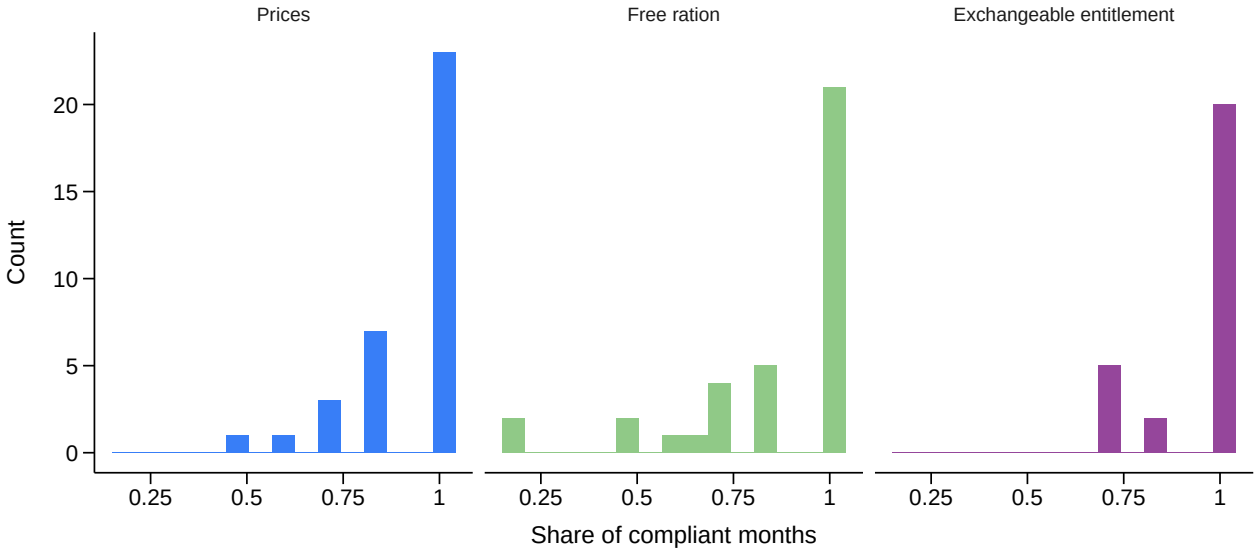
Figure C.1: Experimental balance



Notes: This figure shows experimental balance. Bars show average values for survey households in each treatment arm for six covariates. From top-left, these are: A dummy for the presence of a child below the age of 5 in the household, number of household members (with minimum and maximum values rescaled to lie between 0 and 1 for graphical convenience), a dummy for whether the household owns a refrigerator, a dummy for whether the household owns a two-wheeler/motorcycle, a dummy for whether the household head has at most a primary education, and the number of years the respondent has lived in the village (also rescaled for graphical convenience). We also conduct pairwise t-tests between each of the three main treatment arms and the control. Brackets show p-values with a Hommel adjustment for multiple comparisons.

Compliance Midway through the experiment, our implementation partner had to deal with a period of increased operational and staffing challenges, partly caused by losses in revenue flow from sales of carbon credits, or by factors unrelated to the experiment such as flooding in some villages, wells running dry, or the franchise entrepreneurs operating the treatment arm choosing to quit. As a result in some months (and for some villages) the company temporarily paused operations. At these times water orders by all households are zero by definition. Figure C.2 shows that for the majority of villages, water delivery was available for 100% of scratch card offer months.

Figure C.2: Water delivery non-compliance



Notes: This figure plots histograms of the number of months households in each village were actually able to order water from Spring Health, divided by the intended offer duration, for each of our treatment arms. For example, households in a village with an offer duration of 7 months where Spring Health only operated for 5 months have a share of $\frac{5}{7} = 0.71$. Each observation is one village.

In our main demand analysis, we exclude months where water was not delivered, as we are interested in households' take-up and usage of clean water under different allocation regimes, and months without a functioning seller reveal no information about demand for water or valuation.

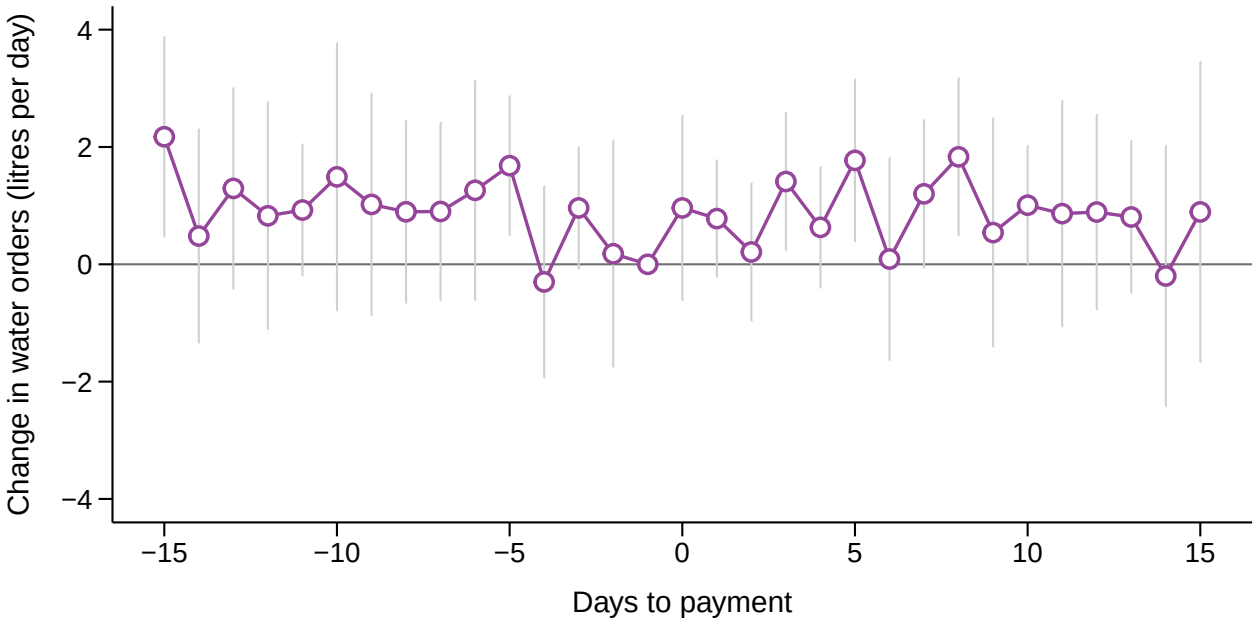
Exchangeable entitlement payments We might be concerned that households in the exchangeable entitlement arm chose to order water rather than receiving cash payments because they did not think they would actually be paid. This is a particular issue because while water arrives at the time of ordering, the rebate for which households were eligible could only be calculated at the end of the month. In addition, because transfers were often very small, our implementation partner sometimes clubbed payments for multiple months together creating delays.

If trust is a concern here then we would expect that households would change their ordering behavior following the first payment since this transfer makes it clear that the rebate offer was real. We therefore carry out an event study style analysis, using only exchangeable entitlement households to estimate the equation below:

$$Y_{it} = \sum_{d=-30}^{30} \beta_d 1[\text{Days to payment} = \text{d}]_{it} + \alpha_i + \varepsilon_{it} \tag{C.1}$$

where Y_{it} are water orders by exchangeable entitlement household i on date t. $1[\text{Days to payment} = \text{d}]_{it}$ is an indicator equal to 1 if household i is d days from payment on date t, α_i are household fixed effects, and ε_{it} is an error term, clustered at the village level. We restrict the sample to days around the first time households are paid, to avoid contamination of the pre-period in subsequent months, and because the first payment is where we are most likely to see trust related effects. Appendix Figure C.3 plots the results. We find no evidence of changes in water ordering behaviour at the timing of payment.

Figure C.3: Effect of payment on water orders in the exchangeable entitlement arm



Notes: This figure plots changes in daily water orders among exchangeable entitlement households only, relative to 1 day before the timing of the first rebate payment. We estimate these coefficients using Equation C.1, which includes household fixed effects. Confidence intervals are derived from standard errors which are clustered at the village level.

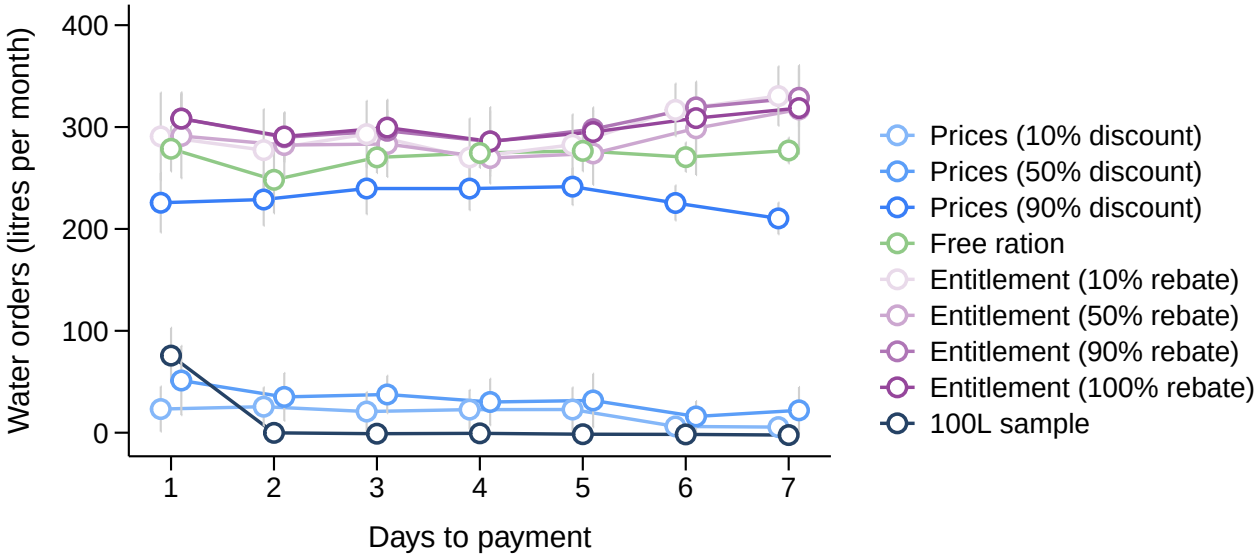
D Additional results

D.1 Sub-treatment-specific effects

Dynamic effects by sub-treatment Figure 4 plots the effects of treatment on water prices as a function of months since the beginning of treatment. In this main text figure, we pool across sub-treatments. Appendix Figure D.1 presents results for each sub-treatment

separately. As in Figure 4, we find that demand is very stable across time for all sub-treatments.

Figure D.1: Water orders event study (unpooled)



Notes: This figure plots the effect of our treatments on water orders as a function of time since treatment started (with the first month offers were active set to 1), estimated using a sub-treatment-specific version of Equation (2). Standard errors are clustered at the village level. 35 villages — the first enrolled in the experiment — had only 5 months of treatment, while the remainder had 7. The sample is restricted to months when deliveries occurred.

Health effects by sub-treatment Appendix Table D.1 the intent-to-treat effects of offers on health outcomes using a version of Equation (3) that estimates a separate coefficient for each sub-treatment arm. Point effects are negative across sickness, health expenses, and missed work outcomes. Standard errors are larger with the most precisely estimated effects on sickness and missed work measures in the discount group although we cannot reject equality of treatment effects across most treatment arms. Nevertheless, it is interesting that the sub-group facing the highest price signal – namely the 10% discount group – also shows the largest treatment effects on these outcomes, suggestive of high prices perhaps inducing screening behaviour or impacts on complementary sanitation activities.

Table D.1: Effect of treatment offers on health, unpooled

	Sickness	Health Expenses	Missed Work	Symptoms				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
10% Discount	-0.14	-32.58	-0.08	-0.03	-0.02	-0.03	-0.02	-0.06
	(0.05)	(28.51)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)
50% Discount	-0.11	-20.67	-0.03	-0.03	-0.03	-0.02	-0.03	-0.03
	(0.05)	(31.99)	(0.03)	(0.01)	(0.03)	(0.02)	(0.01)	(0.03)
90% Discount	-0.09	-2.72	-0.02	-0.02	-0.02	-0.02	0.00	-0.03
	(0.06)	(32.35)	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)	(0.03)
10% Rebate	-0.11	8.80	-0.08	-0.02	-0.06	-0.04	-0.02	-0.01
	(0.10)	(56.41)	(0.07)	(0.03)	(0.04)	(0.02)	(0.04)	(0.04)
50% Rebate	-0.13	-87.78	-0.09	-0.04	-0.07	-0.04	-0.03	-0.03
	(0.11)	(79.75)	(0.06)	(0.02)	(0.04)	(0.04)	(0.03)	(0.04)
90% Rebate	-0.19	-86.53	-0.15	-0.03	-0.07	-0.06	-0.02	0.00
	(0.08)	(58.70)	(0.05)	(0.02)	(0.04)	(0.03)	(0.03)	(0.04)
100% Rebate	-0.12	-75.82	-0.06	-0.03	-0.04	-0.03	-0.02	-0.04
	(0.09)	(56.28)	(0.05)	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)
Free Ration	-0.07	-30.46	-0.05	0.00	-0.01	-0.03	0.00	-0.02
	(0.06)	(37.03)	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)	(0.03)
N	4,670	4,433	4,670	4,670	4,670	4,670	4,670	4,670
Control Means	0.516	239.507	0.2	0.044	0.159	0.108	0.053	0.223

Notes: This table presents intent-to-treat effects of our treatment offers on health outcomes, estimated using Equation (3), and unpooled to provide estimates for each sub-offer arm of the main treatment arm. We restrict the sample to months when water delivery occurred and drop the top 1 percent of health expenses to remove large outliers. In column (1), the outcome is an indicator for the number of household members being sick in the past week. In column (2), the outcome is household spending on health in the past week in INR. In column (3), the outcome is an indicator for the number of household members missing work due to illness in the past week. In columns (4)–(8), the outcome is an indicator for a household member reporting symptoms of vomiting, fever, stomach ailments (gastric pain/abdominal pain/diarrhea), flu symptoms (cough/congestion/headache/fatigue), and other symptoms (e.g., skin infection, joint pains, etc). All regressions include village and month-of-sample fixed effects. Standard errors are clustered by village.

D.2 Survey evidence on clean water use

We use our survey data to corroborate our demand results. Using Equation (3), we estimate the effect of our water treatment offers on the number of drinking water sources used by the household, whether the household drinks any clean water, and whether the household drinks only clean water. Appendix Table D.2 reports the results. We find large and significant impacts of all treatments on all three outcome variables. We restrict data to months where water was sold and to households reporting at least one drinking water source since some households did not report any sources during one or more survey rounds.

Table D.2: Effects of treatment on water use: survey data

	Number of sources (1)	Drinks SH water (2)	Only drinks SH water (3)
Prices (Discounts)	0.34	0.34	0.09
	(0.06)	(0.03)	(0.02)
Exchangable entitlements	0.75	0.77	0.25
	(0.09)	(0.04)	(0.03)
Free ration	0.61	0.63	0.17
	(0.09)	(0.04)	(0.03)
N	5,285	5,276	5,285
Control means	1.443	0.004	0.001

Notes: This table presents intent-to-treat effects of water offers on consumption of Spring Health water, estimated using Equation (3). Column (1) presents the effect on the number of drinking water sources, Column (2) report drinking any Spring Health water, and Column (3) reports only drinking Spring Health water. We restrict the sample to village-months where Spring Health delivered water. All regressions include village and month-of-sample fixed effects. Standard errors are clustered at the village level.

D.3 Heterogeneous treatment effects

We test for heterogeneous treatment effects on demand for clean water in the discount group only using the following specification:

$$\begin{aligned} Y_{it} = {} & \beta_1 10\%\text{ discount}_i + \beta_2 10\%\text{ discount}_i \times \text{Covariate}_i \\ & + \beta_3 50\%\text{ discount}_i + \beta_4 50\%\text{ discount}_i \times \text{Covariate}_i \\ & + \beta_5 90\%\text{ discount}_i + \beta_6 90\%\text{ discount}_i \times \text{Covariate}_i \\ & + \text{Covariate}_i + \gamma_v + \delta_t + \varepsilon_{it} \end{aligned} \tag{D.1}$$

where Y_{it} is variously the monthly consumption of water in litres. Covariate_i are household characteristics: an indicator for children below 5 in the household; a dummy for whether the household head completed at most primary education; a dummy for ever reporting the use of piped water during any survey round; and a dummy for above-median income; above- vs. below-median monthly household income (binned) in the first survey. γ_v and δ_t are village and month-of-sample fixed effects, and ε_{it} is an error term, clustered at the village level. We restrict the sample to households in price treatment villages.

We test for heterogeneous treatment effects on health (across all treatment arms) using the following specification:

$$\begin{aligned} Y_{it} = {} & \beta_1 \text{Any discount}_i + \beta_2 \text{Any discount}_i \times \text{Covariate}_i \\ & + \beta_3 \text{Any exchange}_i + \beta_4 \text{Any exchange}_i \times \text{Covariate}_i \\ & + \beta_5 \text{Free ration}_i + \beta_6 \text{Free ration}_i \times \text{Covariate}_i \\ & + \text{Covariate}_i + \gamma_v + \delta_t + \varepsilon_{it} \end{aligned} \tag{D.2}$$

where Y_{it} is an indicator for any ill household member in the previous week, spending on health in the past week, or an indicator for any household member having missed work in the past week due to illness. All other terms are defined as in Equation (D.1). We use all treatment villages in this regression.

Appendix Table D.3 presents heterogeneity in demand. Appendix Table D.4 presents heterogeneity in health intent-to-treat effects. We do not observe meaningful heterogeneity in water orders (though there is some evidence of heterogeneity in WTP when aggregating along the full demand curve, as shown in Figure 5).

We do see statistically significant evidence that the benefits of treatment in the Prices arm on the various outcomes are concentrated amongst households with small children. Health expenses also slightly higher amongst higher-income households. We view the first of these facts as further evidence demonstrating that our health effects indeed result from clean water access, as children are likely the most susceptible to diseases such as diarrhoea, and their illnesses can plausibly lead to parents missing work.

Table D.3: Heterogeneous effect of offers on water orders in the prices arm

	(1)	(2)	(3)	(4)
10% Discount	30.37	13.20	18.59	25.05
	(16.16)	(10.57)	(8.77)	(11.25)
50% Discount	37.40	24.81	39.39	41.06
	(16.20)	(10.72)	(12.79)	(12.52)
90% Discount	224.59	238.93	222.87	240.75
	(14.85)	(10.67)	(12.32)	(11.50)
Covariate	-0.28	-0.84	4.95	8.06
	(2.28)	(4.91)	(3.15)	(4.23)
10% Discount × Covariate	-21.34	12.31	16.57	-6.89
	(12.57)	(10.57)	(12.61)	(7.62)
50% Discount × Covariate	-2.25	27.23	-0.31	-6.53
	(10.38)	(22.66)	(11.94)	(9.56)
90% Discount × Covariate	29.73	9.06	22.04	-26.44
	(14.23)	(19.18)	(17.13)	(17.46)
N	1,481	1,481	2,716	2,730
Covariate	Child below 5	Low education	Piped water	High income

Notes: This table presents intent-to-treat effects of water offers on water orders at the monthly level for the prices arm, with discount levels interacted with time-invariant covariates, estimated using Equation (D.1). The outcome variable in all columns is total monthly water order quantity in litres. The covariates we include are: (i) a dummy for the presence of a child below 5, (ii) a dummy for the household head having at most primary education, (iii) a dummy for ever reporting the use of piped water in any survey round, and (iv) a dummy for having above-median income. We restrict the sample to price group villages only, and include only village-months where Spring Health delivered water. All regressions include village fixed effects. Standard errors are clustered at the village level.

Table D.4: Heterogeneous effect of offers on health outcomes

	Sickness (1)	Expense (2)	Missed work (3)	Sickness (4)	Expense (5)	Missed work (6)	Sickness (7)	Expense (8)	Missed work (9)	Sickness (10)	Expense (11)	Missed work (12)
Prices (Discounts)	0.17	306.03	0.25	-0.10	-20.12	-0.04	-0.12	-22.36	-0.03	-0.10	-4.17	-0.04
	(0.17)	(122.27)	(0.16)	(0.05)	(23.71)	(0.02)	(0.06)	(26.25)	(0.03)	(0.06)	(26.74)	(0.03)
Exchangable entitlements	0.30	365.66	0.26	-0.19	-109.04	-0.13	-0.16	-67.84	-0.10	-0.08	-30.63	-0.07
	(0.34)	(239.17)	(0.34)	(0.10)	(63.44)	(0.06)	(0.09)	(65.99)	(0.05)	(0.08)	(50.76)	(0.05)
Free ration	0.09	-24.85	0.07	-0.08	-30.25	-0.06	-0.06	-21.11	-0.01	-0.06	-42.93	-0.04
	(0.18)	(107.84)	(0.08)	(0.06)	(38.07)	(0.03)	(0.07)	(51.72)	(0.03)	(0.08)	(39.46)	(0.04)
Covariate	0.02	22.61	0.02	0.00	-23.15	-0.04	0.01	-23.78	0.03	0.09	37.46	0.01
	(0.13)	(85.19)	(0.07)	(0.07)	(37.69)	(0.03)	(0.05)	(41.29)	(0.03)	(0.06)	(35.76)	(0.04)
Discount × Covariate	-0.31	-310.75	-0.28	-0.02	11.13	0.00	0.01	-22.18	-0.04	-0.05	-79.06	-0.03
	(0.18)	(127.63)	(0.15)	(0.09)	(47.37)	(0.04)	(0.08)	(43.10)	(0.04)	(0.08)	(42.67)	(0.04)
Exchange × Covariate	-0.49	-355.59	-0.42	0.20	141.32	0.10	0.04	-26.60	0.01	-0.19	-116.88	-0.08
	(0.35)	(246.92)	(0.34)	(0.11)	(63.91)	(0.05)	(0.08)	(59.89)	(0.05)	(0.08)	(56.57)	(0.04)
Ration × Covariate	-0.14	28.99	-0.11	0.04	-4.83	0.05	-0.01	-17.09	-0.09	-0.03	28.77	-0.02
	(0.20)	(103.31)	(0.10)	(0.10)	(64.33)	(0.05)	(0.08)	(63.25)	(0.05)	(0.10)	(51.08)	(0.05)
N	1,405	1,364	1,405	4,681	4,445	4,681	4,512	4,282	4,512	4,512	4,282	4,512
Covariate	Child below 5			Low education			Piped water			High income		

Notes: This table presents intent-to-treat effects of water offers on health, with offer types interacted with time-invariant covariates, estimated using Equation (D.2). The outcome in columns 1, 4, 7, and 10 is the number of household members reporting illness in the last week; the outcome in columns 2, 5, 8, and 11 is health expenses in INR in the last week, and the outcome in columns 3, 6, 9, and 12 is the number of household members that missed work due to illness in the last week. The covariates we include are: (i) a dummy for the presence of a child below 5, (ii) a dummy for the household head having at most primary education, (iii) a dummy for ever reporting the use of piped water in any survey round, and (iv) a dummy for having above-median income. We restrict the sample to village-months where Spring Health delivered water. All regressions include village and month-of-sample fixed effects. Standard errors are clustered at the village level.

D.4 Placebo effects on health outcomes

In Appendix C we present data on non-compliance during the experiment, in the form of months where water was not delivered (sold) by our implementation partner. We generally did not conduct surveys in these village-months, both because households could not place orders and in some cases because our partner advised us about household sensitivities where they had suddenly ceased operations. That said, we did conduct some surveys in these months, typically because we were not informed of operational disturbances in advance.

We thus run a set of regressions using Equation (3), restricting the sample to village-months where water distribution was halted. With the (significant) caveats that the disruption itself could affect household responses and the fact that these village-months are not representative of the broader sample, this exercise provides a useful placebo test of the impact of clean water on health. If our health effects (as reported in Tables 3 and D.1) are truly driven by clean water access, we should expect to see these impacts disappear in the absence of water deliveries.

Table D.5 provides results from this analysis, split by sub-treatment offers. We see no evidence of improvements in health outcomes (negative treatment effects) in these months. For the exchangeable entitlement and free ration groups, the point estimates appear attenuated, albeit noisy. Perhaps surprisingly, we see a *positive* effect on health outcomes (i.e., households report being *sicker* than the control) in the 90% discount group. However, we see no similar effects in the other discount groups. Thus, it is likely that this is driven by idiosyncratic differences in health outcomes in a few households given the relatively small sample size we are left with in this regression (393 household-month observations with 6 discount group villages, 4 exchangeable entitlement villages, and 5 free ration villages).

Table D.5: Placebo intent-to-treat effect of water offers on health

	Sickness	Health Expenses	Missed Work	Symptoms				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
10% Discount	0.04	-47.86	0.04	-0.01	-0.05	0.07	0.00	-0.02
	(0.07)	(47.25)	(0.03)	(0.03)	(0.03)	(0.06)	(0.00)	(0.05)
50% Discount	0.03	119.90	0.06	-0.01	-0.02	0.14	0.03	-0.01
	(0.13)	(131.35)	(0.04)	(0.02)	(0.07)	(0.06)	(0.03)	(0.02)
90% Discount	0.26	265.76	0.28	-0.03	0.11	0.12	0.03	-0.03
	(0.09)	(108.50)	(0.09)	(0.02)	(0.06)	(0.05)	(0.02)	(0.06)
10% Rebate	0.03	19.63	-0.09	0.00	-0.02	0.01	0.01	-0.01
	(0.12)	(73.29)	(0.06)	(0.00)	(0.10)	(0.07)	(0.07)	(0.09)
50% Rebate	-0.32	-106.05	-0.09	0.00	-0.18	-0.05	-0.05	-0.19
	(0.18)	(58.69)	(0.06)	(0.00)	(0.13)	(0.05)	(0.05)	(0.10)
90% Rebate	0.18	158.72	0.44	0.00	0.02	0.04	-0.05	-0.03
	(0.33)	(281.73)	(0.12)	(0.00)	(0.16)	(0.02)	(0.05)	(0.13)
100% Rebate	-0.09	-157.64	0.03	0.08	-0.14	0.03	-0.05	-0.15
	(0.27)	(102.84)	(0.10)	(0.05)	(0.13)	(0.10)	(0.05)	(0.13)
Free Ration	-0.18	-74.84	-0.01	0.00	0.02	-0.08	0.05	-0.08
	(0.19)	(84.95)	(0.10)	(0.02)	(0.07)	(0.08)	(0.02)	(0.07)
N	393	361	393	393	393	393	393	393
Control Means	0.423	250.417	0.109	0.044	0.131	0.102	0.007	0.197

Notes: This table presents intent-to-treat effects of our treatment offers on health outcomes, estimated using Equation (3, on only the sample of village-months where water distribution was halted. In column (1), the outcome is the number of household members being sick in the past week. In column (2), the outcome is household spending on health in the past week in INR. In column (3), the outcome is an indicator for the number of household members missing work due to illness in the past week. In columns (4)–(8), the outcome is an indicator for a household member reporting symptoms of vomiting, fever, stomach ailments (gastric pain/abdominal pain/diarrhea), flu symptoms (cough/congestion/headache/fatigue), and other symptoms (e.g., skin infection, joint pain, etc.). We drop the top 1 percent of health expenses to remove large outliers. All regressions include village and month-of-sample fixed effects. Standard errors are clustered by village.

E Pure control households vs. control households in treatment villages

In this Appendix, we compare household water orders in pure control villages to water orders in control households in treatment villages. We restrict the sample to households who place orders for water, because while we have a full listing of potential customers in the treatment villages (i.e., we observe households both ordering and not ordering water), in the pure control villages, we only observe households who order water.

To carry out this comparison, we first run a regression of the following type on pure control and control consumers:

$$Y_{it} = \alpha + \beta_1 \cdot 1[\text{Treatment village}]_v + \delta_t + \varepsilon_{vt} \tag{E.1}$$

where Y_{it} is the total water orders or the mean order size of consumer i in month t, $1[\text{Treatment village}]_v$ is a dummy that takes the value 1 if consumer i is a control household in one of the treatment villages and 0 if they are a consumer in a pure control village.

We also run a variant of this regression to compare the number of unique control-group consumers in treatment vs. pure control villages at the village-month level as below:

$$N_{vt} = \alpha + \beta_1 \cdot 1[\text{Treatment village}]_v + \delta_t + \varepsilon_{vt} \tag{E.2}$$

where N_{vt} are the total number of consumers in village v in month t, counting only control consumers in treatment villages and all consumers in pure control villages. $1[\text{Treatment village}]_v$ is a dummy that takes the value 1 if village v is a treatment village and 0 if it is a pure control village.

Because both types of consumers face exactly the same price, we might expect no difference in water orders. In practice, however, this is not the case. Table E.1 presents the results. We find that consumers in the pure control villages (where no experiment activities or surveys took place) order more water (column 1) and are more numerous (column 3) than control condition consumers in treatment villages. However, we find that the average order size in the two groups is similar (column 2), suggesting that pure control households order water more often.[44]

Table E.1: Water orders, control households in pure control vs. treatment villages.

	Total orders (1)	Average order (2)	Consumers (3)
Experiment Village Control	-45.12	-0.13	-24.32
	(7.39)	(0.99)	(1.52)
N	14,501	14,501	638
Reference (Pure Control) Means	293.51	12.36	36.97

Notes: This table presents tests for differences between all consumers in Pure Control villages vs. control-condition consumers in other (treatment) experiment villages following Equations E.1 (columns 1 and 2) and E.2 (column 3). We only include households that ever ordered positive quantities of water. The outcome in (1) is mean total monthly orders in litres, (2) is the average order size in litres, and (3) is the total number of non-zero consumers. Regressions include month-of-sample fixed effects, and standard errors are clustered at the village level.

There are two explanations for this divergence. The first is that there are spillovers induced by the treatment. Such spillovers could occur if households facing full price in villages where others have been given special offers react by reducing the probability of placing an order and reducing how much they order. This type of spillover would lead us to *underestimate* the WTP for water, because it would dampen the demand observed at

[44]Some of the gap in the consumer count would occur because there is a smaller pool of controls in experiment villages since 39 households were assigned to treatment offer. Because the size of villages is large this is a small effect. The average population of villages in our sample is 460 households, balanced across conditions by randomization. Based on this we could adjust the true consumer count difference to 22 consumers instead of 24.

the highest price, relative to what might have been observed in the absence of treatment households. It would also lead us to slightly overestimate the intent-to-treat effects on health outcomes, though the instrumental variable estimates should not be affected.

The other explanation for the divergence is that our implementation partner did not market as intensively to control consumers in the treatment villages as they did to households in the pure control villages. A difference in sales *effort* would render the comparison of these two groups uninformative about behavioural spillovers, since full-price consumers would no longer face similar seller behaviour.

We do not have direct measures of "effort," but there is suggestive evidence that this is the more likely explanation. The first piece of evidence comes from a comparison of control water orders across the three arms of the experiment. Specifically, we compare water orders across our three treatment village types among control households only:

$$Y_{it} = \pi_1 1[\text{Exchangeable entitlement village}]_v + \pi_2 1[\text{Free ration village}]_v + \delta_t + \varepsilon_{it} \quad \text{(E.3)}$$

where Y_{it} is water orders for household i in month-of-sample t, $1[\text{Exchangeable entitlement village}]_v$ and $1[\text{Free ration village}]_v$ are indicators equal to one if household i resides in an exchangeable entitlement or free ration village, δ_t are month-of-sample fixed effects, ε_{it} is an error term, clustered at the village level, and the sample consists only of control households.

Table E.2: Effect of village treatment type on control household water orders

	Orders in litres
Exchangeable entitlement controls	0.51
	(1.02)
Free ration controls	0.39
	(0.92)
N	218,003
Dependent Variable Mean	2.818

Notes: This table presents a test of differences between water orders among control households only between our three treatment arms, estimated using Equation (E.3). The price arm is the omitted category. The regression includes month-of-sample fixed effects, and standard errors are clustered by village.

Table E.2 presents the results. There is no difference between control water orders across the three arms. Point estimates are very small (0.4 or 0.5 litres per month), and not statistically different from zero. Since the types of offers and number of customers ordering water across the three arms are very different, this result implies that neither spillovers between

treatment and control households nor seller capacity constraints are likely to explain lower control-group orders in treatment villages.[45]

Finally, as we discuss in Section 5, households are not re-selling clean water, so this cannot explain reductions in orders among the control group. We also see no evidence of a consumption kink between consumers paying full price, vs a 10% discount, as seen from the bottom-right panel of Figure 3. If anything, full price consumers order slightly more (conditional on buying anything) than do those with a 10% discount.

More broadly, our experience in the field suggests the marketing effort explanation is more likely. In treatment villages, our implementation partner was guaranteed more consumers because of the discount, free ration, or entitlement cards that were provided to a meaningful number of households. The research team fully reimbursed the implementing partner for all subsidies, raising revenues in these villages. As a result, it is highly likely that the implementing partner expended less marketing and sales effort among control customers in treatment villages relative to pure control villages. For all these reasons, although we cannot rule out either of these explanations, our prior is that seller effort may be more important than spillovers or behavioral responses by control consumers.

Nevertheless, it is straightforward to de-bias our WTP measure by re-estimating the demand curve with the assumption that under equal effort / no spillovers (i) the number of consumers at the highest price would rise to match levels in the pure control, and (ii) consumers at all price levels would use additional water equal to the estimate from Column (1) of Table E.1. Doing so leads to an adjusted WTP of INR 153, up from INR 132 in the main experiment sample.

F Calculating profitability

To calculate the net present value of the profits from selling clean water, we use data from Spring Health on costs, as well as a series of assumptions, enumerated in Table F.1. We begin by computing annual revenues and variable costs for clean water take-up levels ranging from 10 to 100% of the households in a representative village. We use the extensive-margin demand curve to identify the price associated with each take-up share. The number of consumers at each take-up level is simply the number of households in our representative village multiplied by the take-up share. Annual revenue is thus simply Revenue = Price $\times$ Consumers $\times$ Per-consumer usage $\times$ 12, and, annual total variable costs are: Variable cost = Per-consumer cost $\times$ Consumers $\times$ 12.[46] Finally, we compute the net present value of costs as the up-front cost of installing a water treatment plant plus annual fixed costs and annual total variable costs over the assumed life of the plant, and compute the net present value of revenue as annual total revenue over the life of the plant, both discounted using our assumed discount rate. Profits are thus revenues net of costs.

[45] Beyond this evidence from our data, we have no independent reason to suspect capacity constraints — delivery vans had lots of room, treatment plants could easily serve demand, and no concerns were raised by our implementation partner.

[46] Our cost data are monthly, so we multiply by 12 to compute annual costs.

Table F.1: Assumptions for profit calculation

Panel A: Up-front costs	
Item	**Cost per plant (INR)**
Water purifier	260,700
Tank, motor, & fittings	39,570
Plumber	3,500
Painting	15,000
Plant structure	45,000
Iron plate frame	10,000
Transportation of machine and bottles	3,000
Launching costs	12,000
Total	*388,770*
Panel B: Monthly fixed costs	
Item	**Cost per month (INR)**
Employee salaries	5,543
Total	*5,543*
Panel C: Variable costs	
Item	**Cost per customer-month (INR)**
Water bottle	21.83
Water bottle stickers	2.92
Entrepreneur commission (incl. water costs)	66.45
Delivery costs	75.31
Total	*166.51*
Panel D: Assumptions	
Item	**Value**
Nr. of households	450
Litres per month	237
Discount rate	5%
Plant life (years)	10
Mean villages served by each plant	1.5
Life of water bottle (years)	1
Monthly bottle rental price (INR)	66.67

Notes: This table reports the cost data and assumptions used in our profitability calculation.

G Deviations from our pre-analysis plan

This experiment was pre-registered with the AEA as AEARCTR-0010545.[47] Though we endeavour to follow the PAP as closely as possible, we enumerate our deviations below:

- **Analysis.** PAP equations (1), (2), and (3) use purely cross-sectional variation (plus LASSO-selected controls). In the paper, we present results only using Equation (4),

[47] The registry entry is available from https://www.socialscienceregistry.org/trials/10545.

a panel specification, for the sake of parsimony. Results using the cross-section are quantitatively similar.

- **Analysis.** In the PAP, we pre-specified using pure control villages as robustness checks in analysis. Because our analysis includes village fixed effects, our treatment effects are not identified off of between-village differences, so adding pure control villages to the regression would not change our estimates. We therefore only analyze these villages in Appendix E.

- **Analysis.** On page 9 of the PAP, we pre-specified using post-double-selection LASSO to choose controls. We do not include controls (other than fixed effects) for two reasons. First, since we submitted our PAP, new work (Cilliers, Elashmawy, and McKenzie, 2024) has arisen, arguing that selection of controls via LASSO in RCTs is essentially useless. Moreover, even if we wanted to add controls, we only have control variables for the subset of households that participated in our survey, so we cannot add household observables into the administrative data regressions for the vast majority of households.

- **Analysis.** PAP section 4.2 proposes estimating the price elasticity of demand, both separately for price and exchangeable entitlement arms (Equation (6)) and jointly, with an interaction term for being in the exchangeable arm (Equation (7)). PAP Equation (16) proposes a heterogenous version of this specification. Given the large number of zeroes in our consumption data, per Chen and Roth (2024), this log-log specification does not deliver the quantity of interest. Following this paper's guidance, we therefore do not present these specifications and instead focus on the extensive-margin effects and levels effects documented in Table 1 and the corresponding heterogeneity results documented in Appendix Table D.3.

- **Analysis.** PAP Equation (8) proposes estimating the effects of exchangeable entitlements vs. discounts in a point-by-point manner. This is effectively subsumed by Table 1, so we omit it here.

- **Analysis.** The PAP proposes estimating the effect of our offers on water shortages. In our survey data, households report they ran out of water (across all uses) in the prior week only 2% of the time. As a result, there is no margin for adjustment on this variable, so we omit it from our analysis.

- **Analysis.** PAP equations (10) and (11) propose instrumenting for the quantity of water ordered with water offers. In Table 4 (and its variants), we instead use whether the household reports drinking any clean water for two reasons. First, this ensures that the endogenous variable that comes from the same survey as the outcome variable. Second, the effect of drinking any clean water is easier to understand than the effect of ordering one unit of clean water, which must be rescaled to be meaningful. We therefore prefer this endogenous variable.

- **Analysis.** In the PAP, Equations (10) and (11) use all sub-treatment arms in the first stage. We instead present pooled IV estimates for the free ration, exchangeable entitlement, and price arms separately, to measure differential local average treatment effects of each offer type.

- **Analysis.** PAP section 4.3.3 proposes "medium-run" health effects, which use only the health data from endline. As we show in Figure 4, water demand is stable throughout the study. Furthermore, our endline data are collected well after the end of water distribution for the first two implementation waves in our sample, and, as we show in Appendix Table D.5, health treatment effects disappear in the absence of water distribution. Thus, this endline-only exercise is unlikely to add meaningful information and we omit it here.

- **Analysis.** PAP section 4.6 proposes a series of heterogeneity analyses. First, PAP Equation (15) proposes heterogeneity on water quantity by household size, number of children, household income, and quality of drinking water options, all measured at baseline; above- vs. below-median liquidity constraints; an indicator for whether agriculture is the main source of income; whether a household ever wanted to take a loan but was unable to; above- vs. below-median consumption; and above- vs. below-median assets. This equation also proposes a cross-sectional regression specification. We instead estimate heterogeneity using Equation (D.1), for alignment between the heterogeneous treatment effects and main effects. We also limit the set of covariates to four: an indicator for having any children below 5 in the home; above- vs. below-median household head education; above- vs. below-median income; and an indicator for ever reporting using piped water for drinking during the experiment. We do this for several reasons. First, we did not conduct a baseline survey prior to treatment implementation, and many of the variables we intended to use in heterogeneity could plausibly be affected by treatment. Second, given the high take-up in both the free ration and exchangeable entitlement arm, there is limited scope for heterogeneity. In the interest of parsimony, we therefore present heterogeneity only along a few key covariates. We present the results in Figuree 5 and Appendix Table D.3.

  As discussed above, we omit PAP Equation (16) due to concerns about log-log specifications with zeroes.

  PAP Equation (17) – and the subsequent un-numbered equation – propose heterogeneity on intent-to-treat effects and local average treatment effects of clean water on health. Here, we pre-specified heterogeneity by the number of children in the household, household income, quality of drinking water measured at baesline, and whether the household treats their drinking water at baseline. We replace these covariates with the same set that we use in our demand heterogeneity analysis, namely an indicator for having any children below 5 in the home; above- vs. below-median household head education; above- vs. below-median income; and an indicator for ever reporting using piped water for drinking during the experiment. We present the results in Appendix Tables D.4. Because we find limited evidence of heterogenous ITT effects on health, we omit heterogeneous IV effects on health for the sake of parsimony.

- **Multiple hypothesis testing.** In PAP Section 4.7, we outline multiple hypothesis testing corrections for our health outcomes. We did not measure outcomes for children, so we omit this. We present FDR adjusted p-values for whether any household member was sick in the past week (which is essentially a symptom index), health expenditures, and whether anybody in the household missed work due to illness in Table 3.

- **Information intervention.** In PAP Section 5, we propose testing the impacts of an information intervention on household water demand. We will conduct this analysis separately, and thus do not include it here.