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

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Abstract

Over 2 billion people lack clean drinking water. Existing solutions face high costs (piped water) or low demand (point-of-use chlorine). Using a 60,000 household cluster-randomized experiment, we test an alternative approach: decentralized treatment and home-delivery of clean water to the rural poor. At low prices, take-up exceeds 90 percent, sustained throughout the experiment. High prices reduce take-up but are privately profitable. We experimentally recover revealed-preference measures of valuation. Willingness-to-pay is several times higher than prior indirect estimates; willingness-to-accept is larger and exceeds marginal cost. Self-reported health measures improve accordingly. On a cost-per-DALY basis, free water delivery regimes appear highly cost-effective.

Key words: clean water; valuation; environmental quality; development
JEL Codes: O13; Q25; Q53

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1 Introduction

More than 2 billion people still do not have safe 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] Nor is this public health crisis close to being solved, with the UN estimating that achieving universal access to clean water by 2030 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).[2]

The aspirational "gold standard" is universal access to clean water at home, delivered through pipes and taps. Piped drinking water is not a new technology — the United Kingdom has required that new houses have clean piped water since the passage of the Public Health Act of 1875 — and the dramatic expansion of purified water delivered to homes through pipes in developed countries was one of the greatest public health achievements of the 19th and 20th centuries (Alsan and Goldin, 2019).[3] However, 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 (WHO, UNICEF, 2024). Moreover, lab testing reveals that piped water in low-income countries is often just as contaminated as untreated surface water (World Bank, 2017). In 2021, the US Centers for Disease Control and Prevention identified only 58 countries worldwide where tap water is safe for drinking. This list excludes 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, 2021). Even in India's National Capital Region, only 60% of piped water samples were deemed fit to drink (Jalan and Somanathan, 2008).

These facts establish two goals for economists. The first is to evaluate improved solutions that bring us closer to universal safe drinking water. The second is to quantify how much the poor value clean water in order to inform the optimal allocation of limited state funds. This paper contributes on both fronts. We use a cluster-randomized field trial, covering about 60,000 households in 120 villages in rural Odisha in India, to evaluate the demand for drinking

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

[2]The costs of these necessary investments are substantial: achieving global universal clean water and sanitation access by 2030 is expected to require increasing annual spending by between \$131 and \$141 billion (World Bank, 2024).

[3]As of 2022, 94.3% of the population in high-income 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).

water provided using a new approach — decentralized water treatment combined with home delivery in sealed, reusable containers. We show that this alternative could significantly improve access and we use the experiment to recover direct measures of household valuation for clean water.

Our experiment adds to a growing body of work in the economics literature that has both studied options to improve the take-up and efficacy of piped water (Galiani, Gertler and Schargrodsky, 2005; Gamper-Rabindran, Khan and Timmins, 2010; Devoto et al., 2012; Szabó, 2015; dos Santos and Guidetti, 2024), and evaluated cheaper alternatives suitable for low-income countries. The approach that has arguably gained the most traction has been point-of-collection or point-of-use treatment using chlorine tablets or solution. A rich body of evidence suggests that chlorine-based water treatment is both effective at improving health and extremely cheap (Kremer et al., 2011*b*; Luoto et al., 2011; Dupas et al., 2016; Null et al., 2018; Haushofer et al., 2021; Dupas et al., 2023). These studies and others are summarized in a review (Clasen et al., 2015) and meta-analysis (Kremer et al., 2023).[4]

Yet despite its benefits, point-of-use and point-of-collection treatment have proved insufficient to fully solve the clean water access problem in low-income countries. The Kremer et al. (2023) meta-analysis reports average point-of-use take-up rates of just 46%. Crucially, low demand for chlorine tablets or solutions is not due to monetary costs: even at *zero price*, their usage has been well below universal access.[5] Moreover, many households who do obtain chlorine do not ultimately use it to treat their drinking water.[6] As a result, although both piped water and point-of-use chlorine have increased clean water access somewhat, overall progress is far slower than we might wish, lagging population growth in many parts of the world. Indeed, 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).

The intervention we study (home delivered treated water) provides a third option. Private sector variants of this approach are gaining increasing traction, serving a growing segment

[4] A smaller number of papers measure the effectiveness of alternative home treatment methods, such as water filtration (Berry, Fischer and Guiteras, 2020) or solar disinfection (Conroy et al., 1996).

[5] Non-monetary costs include unpleasant taste (Jeuland et al., 2016; Crider et al., 2018; Puget et al., 2010; Smith et al., 2021; Dupas et al., 2016), and the inconvenience and cognitive burden of treating water at home. Point-of-collection treatment has been found to also face contamination risks associated with households collecting and transporting their own water (Kremer et al., 2011*b*).

[6] These results are very 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., 2023), 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, 2010). 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.

of households in urban slums and rural areas who are willing to pay for potable water (Cohen and Ray, 2018; Advani et al., 2011; Brown et al., 2011; Daly et al., 2021). 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). Yet despite evident latent interest, to our knowledge there exists no rigorous evidence quantifying the demand for low-cost interventions that directly supply safe water — as distinct from point-of-use treatment options — to the rural poor.

Our study location, Odisha, is one of the poorest states in India, and has low levels of safe drinking water access. We conducted our study in partnership with a private company operating in rural Odisha. Section 2 provides more detail on our study setting and on how water was treated and delivered to the home. In the experiment, which we describe in detail in Section 3, we randomize villages into one of three treatment arms: (i) a 'Prices' regime where water was sold at varying prices, (ii) a 'Free Ration' regime where households could order up to 400 litres of water per month for free, and (iii) an 'Exchangeable Entitlement' regime, where households could either order treated water for free or, by forgoing an order, receive cash rebates of varying amounts for every unused unit of their monthly entitlement.

Within each village, we randomized households into control or treatment. Control households could buy water at the prevailing high price charged by the company prior to the experiment. In the price arm, treated households were randomly assigned to receive discounts of 10%, 50%, or 90% of this price. In the exchangeable entitlement arm, treated households were correspondingly randomized to receive cash rebates equal to 10%, 50%, 90% or 100% of this price for every bottle of their 400 litre entitlement that they did not order. We analyze this experiment using rich administrative data on water orders and several household surveys, which we describe in Section 4.

Our first main finding, documented in Section 5, is that home delivery of treated water can work well to expand clean water access, but households are quite price-sensitive. We find that take-up of clean water at low prices is nearly universal and is sustained over the experiment duration. Over 90% of households order water when it is free. Take-up is similarly high at low prices: at our lowest price of INR 0.14 / litre, take-up is about 89%. Strikingly, the demand for home-delivered water is high even though households have access to cheap chlorine solution in local stores and about 34% of our sample report having access to piped water at some point during the experiment. Home delivery of clean water thus overcomes take-up ceilings documented in other water-treatment approaches (Dupas et al., 2023).

However, total demand falls substantially as prices rise. Interestingly, this variation stems 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. For households choosing to order, demand is also stable over time, unlike prior evidence on chlorine-based water treatment (Ashraf, Berry and Shapiro, 2010; Berry, Fischer and Guiteras, 2020; Dupas et al., 2016). In addition, demand is not unbounded even when water is free. Our ration was generous enough to exceed the likely drinking water needs of most households. Although households order more water when it is free than under status-quo pricing, the ration does not bind. This suggests that a policy to provide free drinking water might generate limited waste, at least for rural populations similar to our experiment which lack safe *drinking* water but have plentiful access to water for other purposes.

The patterns of demand we observe are consistent with informed consumers seeking to replace most of their drinking water needs with home-delivered clean water, with consequent time savings and health benefits. The latter are well documented in the literature (e.g., Kremer et al., 2023) and as corroboration, we estimate local average treatment effects showing that clean water access reduces self-reported sickness by 23 to 62 percent, and leads to reductions in health expenses and missed work due to illness. Our survey evidence also shows that treated households also spend meaningfully less time collecting water.

Our second 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 lower-bound measure of willingness-to-accept (WTA) derived from households' decisions to forgo cash payments in lieu of water in the exchangeable entitlement arm. Measuring both is substantively important in our setting.

WTP is the standard measure of valuation, describing the maximum amount of money a household would give up to go from drinking dirty to clean water, and is necessary for a Kaldor-Hicks evaluation of public goods provision absent market failures. However, revealed preference measures of WTP can be hard to interpret because they may be biased downwards by various market failures (Greenstone and Jack, 2015). In contrast, WTA describes the minimum amount of money a household would have to receive in order to give up clean water in favor of unsafe water. One way to decide which valuation is more appropriate is to consider property rights: if households have no right to clean water, then WTP may be the relevant metric, but if the government treats access to clean water as a right (as is guaranteed by Article 21 of the Indian constitution), WTA may be the relevant measure of valuation. Even in the absence of market failures or behavioral biases, theory suggests that in cases where a good (in this case, clean water) is both highly valued and has no good substitutes, WTA can (significantly) exceed WTP (Hanemann, 1991).

In the literature, direct evidence on the WTP for clean water is scant (Ahuja, Kremer and Zwane, 2010; Null et al., 2012), because prior work has either quantified households' valuation of water treatment technologies rather than clean water itself (Ashraf, Berry and Shapiro, 2010; Kremer et al., 2011*a*; Berry, Fischer and Guiteras, 2020), inferred valuation indirectly from time costs (Kremer et al., 2011*b*), or uses contingent valuation (Pattanayak et al., 2005). We estimate a population average WTP from our experimentally measured demand curve for clean water of INR 132 per month for clean water access (USD 20 annually at INR 80 per USD), about 1.5 percent of median household monthly expenditure. The most comparable estimate to our number comes from seminal work by Kremer et al. (2011*b*) 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 substantially higher than chlorine-based point-of-use treatment. Adjusted to 2023 dollars, we estimate the population average WTP for chlorine solution from Ashraf, Berry and Shapiro (2010) at about USD 0.11 (14 times lower) for an amount of water equal to the monthly consumption in our sample. Indeed, the WTP from other similar studies is even smaller (Kremer et al., 2011*a*). These extraordinarily low valuations reflect the substantial gap between the monetary costs of point-of-use chlorine and its take-up. A key implication of our experiment is that this does not represent how much households value clean water itself, in part because prior work has evaluated bundles that provide water coupled with other non-monetary costs such as travel time, chlorine taste, or the inconvenience of home treatment.

We estimate a lower-bound for WTA that is even larger, at INR 420 per month (approximately USD 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. Our exchangeable entitlements arm directly points to a new type of contract the government could offer as an adjustment on the large, state-sponsored cash transfers to the poor which are standard in developing-country settings, including our own (Banerjee et al., forthcoming; Niehaus and Suri, 2024). For a component of the full transfer, rather than have the default contract be that cash arrives if households take no action, customers could be offered a default contract where water arrives if households take no action. Our results suggest that many households may choose the water and governments may wish to test this idea in practice.

Does high demand and valuation imply that this approach can contribute to solving the clean water access problem? The answer to this question depends on whether direct delivery of decentralized treated water is amenable to implementation at scale and whether it is cost-effective. On implementation, the intervention appears technically feasible to sustain, given that our partner firm has worked for over a decade in a few hundred villages in Odisha, and demonstrated by the proliferation of private provision of clean water in low-and-middle-income countries (Cohen and Ray, 2018), with decentralized water kiosk and "water on wheels" delivery models like those used by our implementation partner becoming increasingly common (Advani et al., 2011; Brown et al., 2011; Daly et al., 2021).

Our data suggest that providing clean water at high prices can be privately profitable. However, our estimates predict that at these prices, only a small fraction of households will buy water. These results explain why the private market has not fully solved the universal clean drinking water access problem. Thus, in order to achieve population-wide access to clean water using this approach, the state likely needs to intervene in the market, which is typically justified by the presence of externalities, and judged on the basis of cost per Disability-Adjusted Life Year (DALY). We carry out a back-of-the-envelope calculation using health benefits based on Kremer et al. (2023), and take-up and cost estimates from our data. We estimate that the cost per DALY of free home delivery of clean water in our intervention ranges between USD 71 and USD 226, which easily clears conventional cost-effectiveness benchmarks, at costs similar to or slightly higher than well-studied interventions such as coupons or free dispensers for chlorine (USD 106 and USD 33 per DALY, respectively). This suggests that policymakers interested in expanding clean water access should consider a role for decentralized treatment and home delivery. We discuss these issues in more detail in Section 6.

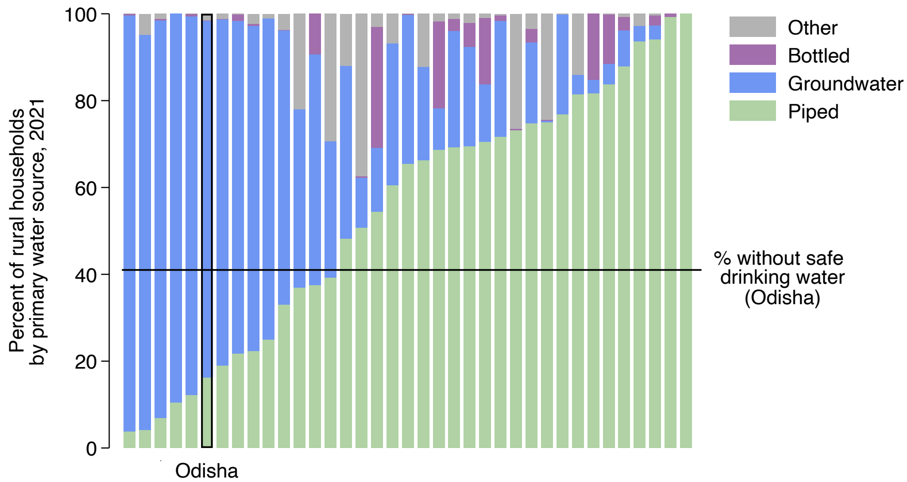
This paper both contributes to the literature on improving access to clean water, and joins a broader conversation in the environmental economics literature on measuring how much people in low-income countries value environmental quality (Greenstone and Jack, 2015). We demonstrate that decentralized treatment and home delivery of free or subsidized water could play an important role in improving clean water access. In doing so, we also provide revealed-preference experimental estimates of the willingness-to-pay for an aspect of environmental quality (in this case, water clean enough to drink). Past work has either inferred this type of valuation by using a hedonic approach, valuing the environment via variation in time costs (Kremer et al., 2011*b*), the cost of air purifiers (Ito and Zhang, 2020), or the cost of protective masks (Baylis et al., 2024), or has instead used take-it-or-leave-it or Becker, DeGroot and Marschak (1964) methods to estimate demand for environmental quality *production technologies* such as chlorine solution (Ashraf, Berry and Shapiro, 2010) or water filters (Berry, Fischer and Guiteras, 2020), rather than directly for the environmental amenity itself. Because our estimate of WTP for clean water is substantially higher than that found in prior work, this distinction appears meaningful. We also provide amongst the first experimental revealed-preference estimates of WTA for clean water or environmental quality more broadly, extending a literature that has been reliant on contingent valuation (Horowitz and McConnell, 2002; Tunçel and Hammitt, 2014). Our results suggest that home delivered clean water is highly valued by households, and may be an important complement to piped water and point-of-use chlorine.

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 by primary water source (piped, groundwater, bottled water, and other) across Indian states and union territories per the 78th round of the National Sample Survey (National Sample Survey Office, 2023). With respect to piped water, Odisha ranks 32nd out of 37, with 83% of households still lacking access as of 2021. Moreover, a 2023 survey in 9,856 villages revealed that 41% of the population lacked access to safe drinking water (Atmashakti Trust, 2023). Drinking water became the subject of grassroots protests (Express News Service, 2024*b*) and was a heated election campaign topic (Express News Service, 2024*a*) in Odisha during India's 2024 general election. Interestingly, privately supplied bottled water, though rare in Odisha, now has a meaningful presence in some wealthier states (Rukmini S., 2024).

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 control group reporting an average monthly household expenditure 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, and median monthly income (elicited using a binned question) was in the INR 12,000 to 16,000 range. 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).

Figure 1: Water access in Odisha



Notes: This figure plots the share of rural households by primary water source (piped, groundwater, bottled water, or other) across all Indian states and union territories in 2021, using data from the 78th round of the National Sample Survey (National Sample Survey Office, 2023), with Odisha outlined in black. The black 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).

The median household reported being able to make a purchase of no more than INR 500 ($\approx$ 6 USD) tomorrow without borrowing.

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 control group reported using groundwater for drinking, and 34% reports using piped water. Less than 1% reported using surface water or bottled water.[7] Households do report spending time on water collection, with the average household spending 32 minutes a day obtaining drinking water.

Nevertheless, many households lack access to household *potable* drinking water, 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), in line with findings from both urban India (Jalan and Somanathan, 2008) and other developing countries (World Bank, 2017) showing substantial contamination in piped water. 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.

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

As indicative evidence, we collected 17 water samples from different sources (open well, tube well, tap, and the drinking water in our intervention) from 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* for the treated water from our intervention. We found *E. Coli* and fecal coliform in all tap water samples. We did not find evidence of arsenic, lead, or cadmium contamination, consistent with government heavy-metal testing in the region (Ministry of Jal Shakti, 2022). There have been several operational challenges with the rural piped water network in Odisha that lead to a risk of biological 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 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; this entrepreneur is normally also the owner of the well providing the input water. The company pays out a monthly stipend to both the operator of the treatment plant and to its delivery staff. 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. The use of solar panels allows the treatment plant to operate without access to electricity from the grid. Sediments are removed using a precipitation tank and water is disinfected using chlorine dioxide gas delivered in precisely measured doses. This procedure is intended to remove coliform and other bacteria and protozoan parasites. A coagulant-flocculant is used to remove flouride, mercury, arsenic, and iron. 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.[8] Deliveries are made multiple times each week. Households could place orders as they like during the week, with payments made against orders fulfilled. Consumers had some flexibility with regards to payment timing: 70% of consumers pay at the end of the month, 10% pay on a daily basis, and 20% pay for the full month in advance. Customers who did not pay would not continue to receive water. As the experiment population may be liquidity constrained, this flexible contract helps us approach the WTP for water while mitigating the influence of credit market failures. Prior work points to the importance of flexibility. For example, Tarozzi et al. (2014) find that demand for bednets was higher when payment in installments was allowed.

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.[9] 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 is contamination at home, underscoring the continued importance of hygiene behaviours and habits such as boiling.

[8] 86% of our households live in 10-litre-bottle villages, and the remainder in 20-litre-bottle villages. These reusable bottles are akin to those used in water coolers, limiting plastic pollution when compared against single-use plastic water bottles.

[9] 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).

3 Experimental design

3.1 Research objectives

The design of our experiment is motivated by two main goals. First, 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 whether it is privately profitable to deliver clean water.

Second, 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 outside of previously-evaluated approaches may have high returns.

Health benefits that can be perceived by households are likely an important component of how much people value clean water. Another useful outcome is therefore the impact of clean water access on self-reported health. As secondary outcomes, we are also interested in the extent to which home delivery of clean water affects drinking water sources, water collection time, and water treatment. Our experiment is thus designed to facilitate estimation of these features of water demand and subsequent benefits of clean water access.

In our empirical setting, 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 forgo clean water access (WTA). WTP is useful as the standard Kaldor-Hicks measure of valuation, and to facilitate comparisons with the (limited) prior literature. However, in a developing-country setting, WTP may be biased downwards due to market failures (Greenstone and Jack, 2015), while WTA, in theory, does not suffer from this problem. Moreover, WTA and WTP need not coincide: if there are no (or limited) market substitutes for a good, which is the case for clean water, WTA can be much larger than WTP (Hanemann, 1991). The WTA is itself also meaningful, as it reveals whether households would prefer receiving free water to receiving cash. This is likely to be policy-relevant in the large number of low- and middle-income countries worldwide which provide low-income households with cash transfers (Banerjee et al., forthcoming). Moreover, WTA is the correct measure of valuation when the government (as India's does) treats water as a right.

The prior literature has either measured WTP using hedonic approaches (e.g., Kremer et al., 2011*b*) or instead measured demand for water treatment technologies (Ashraf, Berry and Shapiro, 2010; Kremer et al., 2011*a*; Dupas et al., 2016; Berry, Fischer and Guiteras, 2020); WTA is typically measured using contingent-valuation approaches (Horowitz and McConnell, 2002; Tunçel and Hammitt, 2014). Our study is therefore designed to directly measure WTP and WTA for clean water itself, using revealed preference via a randomized controlled trial. Because our treatment is clean water that is directly delivered to the home, in contrast to prior work, we estimate how much households value clean water absent the disamenities of collection time or water treatment.

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; the results are thus internally valid for these villages but extrapolating outside the sample warrants some caution.

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 lost a valuable revenue stream from the sale of carbon credits. They scaled down operations on a village-by-village basis in order to continue to be able to reliably provide clean water. 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 *after* accounting for 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, which are not subject to any village-level attrition concerns.

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 ration' arm, we randomly assigned 39 households to receive a free and unconditional ration of up to 400 litres of water per month (set to be well above pre-experiment average consumption). 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. As above, in order to obtain this water, they needed to place an order with Spring Health. 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).

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. Though 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. In order to confirm that households understood this treatment, we conducted a small set of phone-based spot checks, which revealed high comprehension. Of the 28 households across 4 villages we reached for this survey, representing all price groups, when asked how much cash they would receive if they did not order 1 bottle, 24 of these households named the correct amount, 2 households were off by INR 1, 1 household did not know, and 1 household refused to answer.

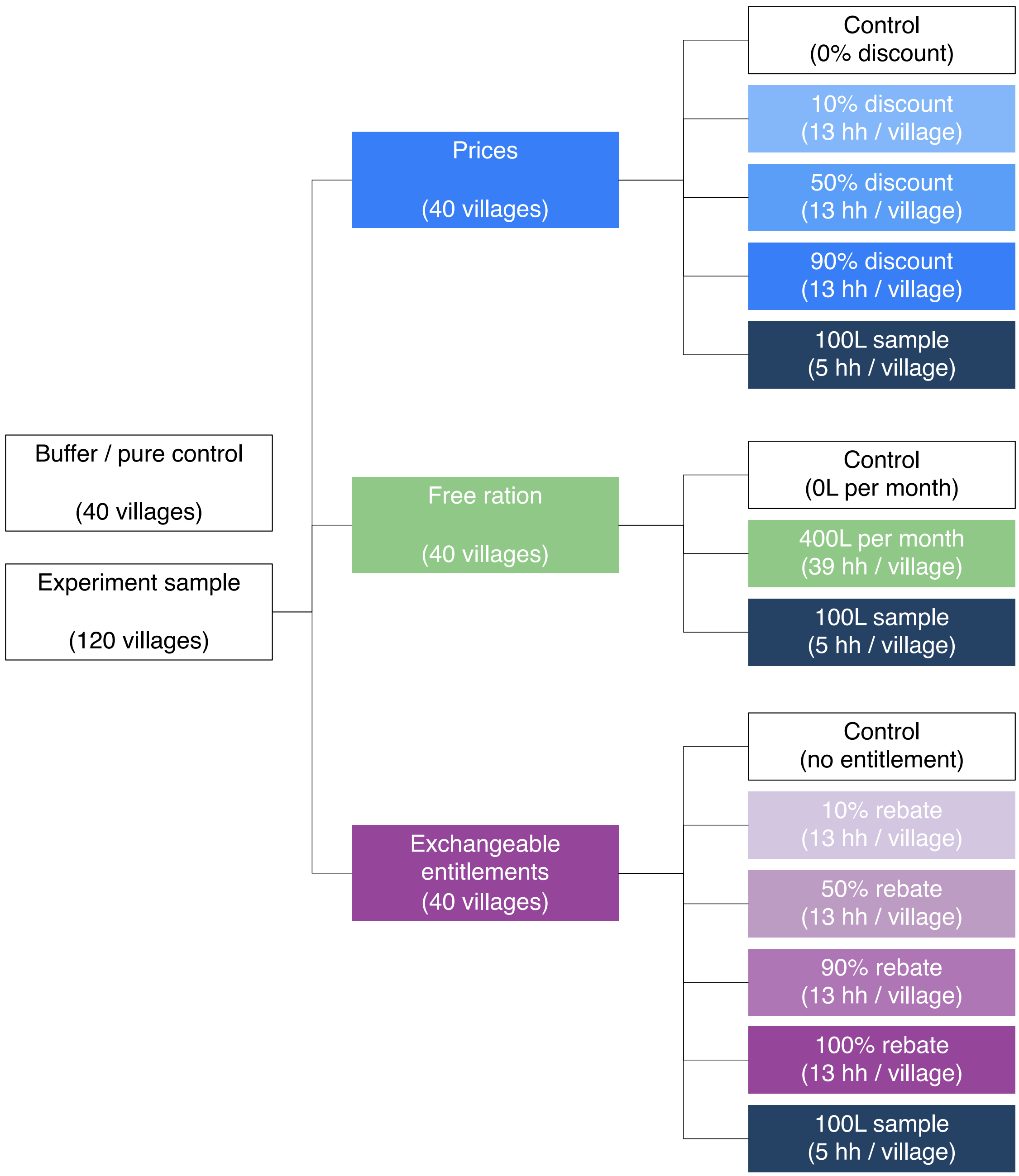
All treatment villages: Free one-time 100 litre sample In addition to the treatments described above, we randomly assigned 5 households in each treatment village to receive a one-time offer of 100 litres of free water. Households who chose not to take this offer received nothing in return. These households 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 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

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 any water they chose not to order. Treatment households in the free ration arm could order up to 400 litres of water per month for free, but received no rebates.

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. Households were informed about their extensions in December 2022–January 2023. We see no change in consumption at the 5-month mark (see Figure 4). 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. Because this balance test is conducted on data collected at endline, it accounts for attrition by construction. We fail to reject balance on all variables and across all pair-wise comparisons, and the magnitudes of any differences are small.

Compliance Appendix Figure C.2 shows that while most of our 99 experiment villages received water deliveries for 100% of experiment months, in 12.3 percent of *village-months*, our implementation partner did not offer delivery, largely due to staffing issues. In such months, water orders are necessarily zero for all households. In Section 5, we present the effects of our treatments in the 87.7 percent of village-months *where delivery occurred.* Our primary empirical object of interest is households' valuation of clean water, which cannot be evaluated in months where no water was offered. When conducting our cost-effectiveness analysis in Section 6, we adjust our compliance measure downwards (costs upwards) to allow for imperfect reliability.

Pre-analysis plan This study was pre-registered through the AEA RCT Registry as AEARCTR-0010545. In the pre-analysis plan, we list the primary outcomes as: (1) Household-level treatment effects on water consumption; (2) Price elasticity of demand; (3) Household-level treatment effects on health.[10] We list non-health benefits and spillovers as secondary, and also describe heterogeneity and multiple hypothesis testing. We generally followed this plan. We report the minor deviations from the pre-analysis plan in detail in Appendix H.

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

[10]Though not included in the PAP, our *ex ante* power calculations, based on water consumption data from our pilot and health data from Clasen et al. (2015), suggested that we should be powered to detect a demand elasticity of -0.03, water consumption differences of 5% or greater, and health effects of 20–30%.

Health administrative dataset must consume 0 litres. This yields a household-by-date panel of Spring Health water orders.

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.[11] 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.[12] 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 drinking water choices, water treatment choices, and health in the week before the survey. Our health outcomes include health expenses, missed work due to illness, self-reported incidents of perceived sickness, and a variety of self-reported symptoms of water-borne disease: vomiting, fever, stomach ailments, flu symptoms, and other symptoms, all of which have been linked to drinking contaminated water (World Health Organization, 2017). To keep the length of repeated surveys manageable we did not collect placebo health outcomes (e.g., broken bones) and our results. Because these are self-reported measures, they can help explain why households value water, but they cannot be interpreted as clinical outcomes.

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. We did not conduct surveys during months with no water delivery, so we are unable to evaluate differences between treatment and control during these periods.

[11] In price discount villages, we selected four control households and three households in each discount level for surveys; in pure quota villages we surveyed six control households and seven quota households; and in exchangeable quota villages, we surveyed three control households, two 10% exchange households, three 50% exchange households, two 90% exchange households, and three 100% exchange households. We did not survey in pure control villages.

[12] 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.

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. While this is not statistically conclusive, 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 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. Since households in study villages were familiar with Spring Health and its product, our results can be interpreted as the impact of water offers on demand, without also introducing new information about a new product.

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.[13] 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.[14]

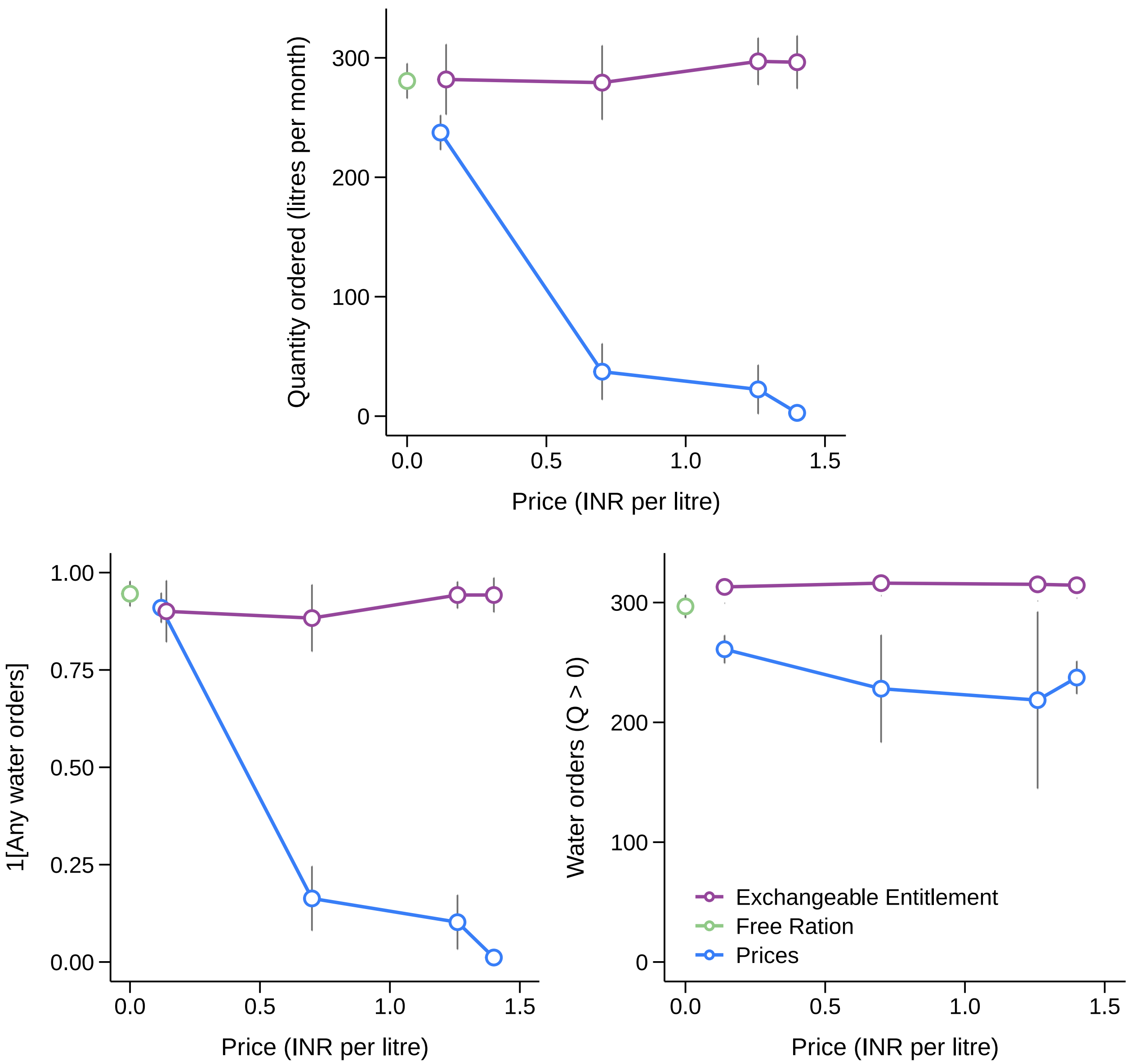
$$\begin{aligned} Y_{it} = {} & \eta_1 \cdot 10\% \text{ discount}_i + \eta_2 \cdot 50\% \text{ discount}_i + \eta_3 \cdot 90\% \text{ discount}_i \\ & + \eta_4 \cdot 10 \ \% \text{ exchange}_i + \eta_5 \cdot 50 \ \% \text{ exchange}_i + \eta_6 \cdot 90 \ \% \text{ exchange}_i \\ & + \eta_7 \cdot 100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

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 named dependent variables correspond to the treatment arms, 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.

[13] 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.

[14] 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.

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)
Control means	0.012	0.012	2.818	2.818
Prices=Exchange	<0.01		<0.01	
Prices=Ration	<0.01		<0.01	
Exchange=Ration	0.695		0.112	

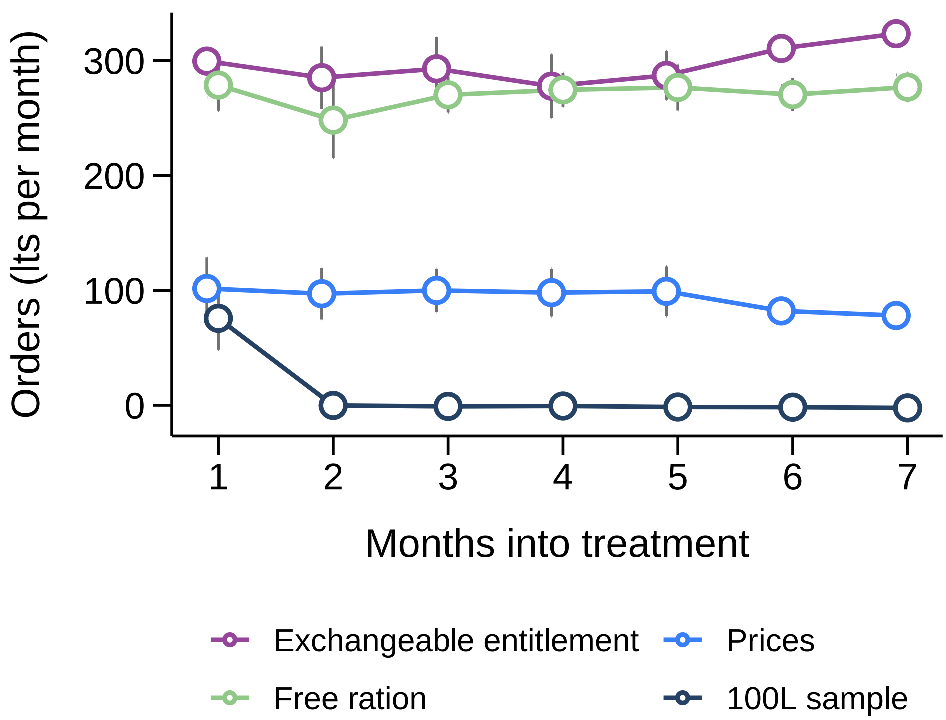
Notes: This table presents intent-to-treat effects of water offers on water orders at the monthly level, estimated using Equation (1) or a pooled version thereof. We restrict the sample to village-months where Spring Health delivered water. In Columns (1) and (2), the outcome is a binary indicator for 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. Price = exchange, Ration = exchange, and Price = ration are tests for equality between the impacts of the respective treatment groups on average monthly orders, pooled across discount and exchange levels.

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 While water purchases are low in the control (i.e., full-price) group, our intervention leads to very high clean water take-up. We can reject impacts on take-up smaller than 34 pp (prices), 86 pp (entitlements), and 85 pp (ration). 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). Accordingly, we strongly reject equality between the 10% discount and 90% discount effects (p-value < 0.01).

Importantly, we see no evidence of sharp changes in demand around a price point of zero (a so-called "zero price effect"). Indeed, we cannot reject equality of take-up on the extensive margin at 90% discounts and zero price ration (p=0.763) and observe a moderate and statistically significant increase in average orders (232 vs 270 litres, p<0.01). 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).

However, we find that households are quite price-sensitive. As shown in Column (2) of Table 1, moving households from the original market price to a 90% discount increases the probability of a monthly orders by 88 pp (s.e. 0.02). Notwithstanding the variation in order probability with price, an interesting feature of demand is that *conditional* on consumption, quantity demanded is relatively 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, $10\times$ the lowest price), mean orders for households who consume more than zero is about 237 litres per month. In our data, the average household size in our experiment is 5. Consuming 1.5 (2) litres per person per day — a standard biological benchmark (EFSA Panel on Dietetic Products, Nutrition, and Allergies, 2010) — would lead the average household to use 225 (300) litres of water 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, suggesting that while this approach can achieve universal access at low prices, higher prices strongly discourage adoption. As the two figures in the bottom panel show, this is driven almost entirely by the extensive margin.

Selection into drinking water purchases This pattern of demand can be rationalized by a simple stylized model where we assume that the main benefit to treated water — for households who already have access to *non-potable* water — comes from improved health. Let a household's probability of falling sick in a month be proportional to the share θ of their biological (i.e., fixed) drinking water needs W that are met by clean water. For instance, let $P(Healthy) = k\theta$ so that the probability of being healthy conditional on setting $\theta = 1$ (i.e., drinking only clean water) is $k \in [0, 1]$. Typically, we would expect $k < 1$ because, even treated water may get recontaminated at home, perhaps due to poor sanitation practices. Assume further that the cost of falling sick is p_{sick} (e.g., lost work, increased health expenses, etc.) and the cost of purchasing water is p_{water}. In Tables D.3 and 3 we provide survey evidence on health outcomes that are consistent with these mechanisms.

Households can thus be modeled as maximizing consumption utility $u_i(c)$ subject to a budget constraint $p_c \cdot c + p_{water} \cdot \theta W \leq I - p_{sick} \cdot k_i \theta$, where the right hand side is monthly income I less sickness costs. If the product $p_{sick} k_i \geq p_{water} W$, households will set $\theta = 1$; otherwise $\theta = 0$.

Lowering the price of water changes the extensive margin and attracts households with lower expected health benefits (i.e., lower values of $p_{sick} \times k_i$) into buying. This may be households with worse sanitation practices, lower wages (hence lower absenteeism costs), or clean alternatives.[15] There is some weak evidence of this type of selection in Table 3 where we document point estimates of clean water on sickness that are greater for the prices arm than free rations. Admittedly this model is sparse and selection could also be induced by allowing for factors such as switching costs, which may vary for other reasons.

Under all treatment arms and all prices, mean consumption of clean water (conditional on ordering) is approximately enough to cover households' biological drinking water needs, but not to displace all other water uses. This is consistent with households setting θ near 1. Survey evidence in 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. Taken together, these pieces of evidence are consistent with households buying water in sufficient quantities to make it their primary drinking water source, but not to use purchased water for other end-uses.

In the model we have outlined, there is no benefit to ordering more clean water than is required to meet drinking water requirements W. In the experiment, households who are provided a 400 litre ration of free water do not use it 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. This treatment effect translates to a population average consumption of 280 litres per month, or 300 litres per month conditional on non-zero orders. These results likely reflect the fact that although our households have limited access to clean drinking water, they do not suffer from water shortages in general. Indeed in our survey data, in both treatment

[15] The fact that better sanitation increases water purchases in this model suggests that households who have access to disinfectants like chlorine but do not like the taste might have a *greater* propensity to pay for home delivered water.

and control arms, households report ever running out of water (for any end-use) in the prior week only 2% of the time.

Modeling water as providing mainly health benefits would be unreasonable if treated households could easily sell or otherwise provide water to people in the control group. To check this, we asked households about re-selling in the endline survey, and not a single respondent 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.[16]

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 (Ashraf, Berry and Shapiro, 2010; 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 consume similar amounts of water every month, suggesting there are no learning or "experience good" factors at play (either about the intervention itself or about clean water) or "experience good" effects. 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. Moreover, despite being informed that the intervention would end after several months, households also do not significantly change behavior in the final month of the experiment, suggesting that the temporary nature of the intervention is unlikely to bias our findings relative to a permanent policy.

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)

[16] 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.

demand curve from Figure 3, imputing linear demand between price points. This yields an average WTP of about INR 132 for monthly clean water access — approximately 280 litres per month, where consumption at the zero price point is given by the average water orders in the free ration treatment arm. These WTP results are robust to alternative functional forms. Fitting a cubic spline through our price points yields a mean WTP estimate of INR 126. Using a step function with the extreme assumption that all households between price points have the WTP of the adjacent but higher (lower) step yields a mean WTP of INR 196 (68). At approximately 1.5% of median consumption, our central WTP estimate is relatively high for a very poor population (comparable to monthly spending on milk, INR 186).

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 litre 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'.[17] 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. We cannot reject equality of water orders in litres between 10% and 100% rebate levels (p=0.343). One concern is that households may value temporary clean water access differently from permanent clean water access. If anything, this should make households in the WTA arm more likely to take the cash, further suggesting that our WTA estimates represent a lower bound.

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, 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

[17] 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.

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 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. Seventh, 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. Eighth, we think income effects are unlikely to drive the gap between WTP and WTA, because the overall magnitude of the implied transfer is both very small compared to household income and available only in the future. Finally, households in our sample have limited ability to borrow; prior research demonstrates that liquidity constraints can lower WTP (e.g., Berkouwer and Dean, 2022). However, because these constraints increase the (shadow) value of cash, they should impact both WTP and WTA, rather than driving a wedge between the two measures of valuation. Nevertheless, if households do not commonly hold liquid cash, purchasing may be more difficult than giving up rebates.

What, then, explains the difference between WTP and WTA in our setting? Theory suggests one possible explanation: under certain conditions, WTA can far exceed WTP. In particular, this can arise for goods with limited market substitutes (Hanemann, 1991, 2003) or when consumers have asymptotically bounded utility functions (Amiran and Hagen, 2003). The intuition is as follows: if there is no bundle of private goods that can compensate for the loss of a particular rationed good in utility terms, then WTA for the rationed good can be infinite. Thus, when there are only imperfect substitutes for the rationed good available on the market, WTA can be much larger than WTP. Support for this hypothesis has been found in lab experiments (Shogren et al., 1994; Shogren and Hayes, 1997). 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. Other commercial bottled water is available, but extremely expensive (e.g., Bisleri-branded water costs INR 4.5 per litre, more than 3x the unsubsidized Spring Health price). Though this need not be the only explanation for the divergence between WTP and WTA – there may be behavioral factors that also drive a wedge between the two measures, such as households treating the entitlement as a labeled transfer – it appears plausible in this context. In Section 5.5, we find suggestive evidence of higher WTP (and thus a smaller gap between WTA and WTP) for higher-income households and those with existing access to piped water.

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 indirectly inferred from time use in Kremer et al. (2011*b*). It is also much higher than for chlorine-based point of use treatment. Adjusted to 2023 dollars, the population average WTP for clorin (a relatively popular brand of home-use chlorine solution), from the demand curve reported in Ashraf, Berry and Shapiro (2010), 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. An important advantage of our estimates is that they directly reveal households' valuation of clean water itself, excluding any disamenity value from water purification or traveling to retrieve water.

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 relatively high.

5.3 Impacts on water collection and water purification

In this section, we use survey data to evaluate the extent to which clean water leads to household behavior changes. We focus on two categories of outcomes. First, we measure time spent collecting water. The water we deliver in our intervention might lead to substantial time savings which households could find valuable if it were to displace trips to wells to collect groundwater. This is plausible in our setting — 34% of the control group reports using taps as a source of drinking water and only 24% as the sole source.

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 (Jeuland et al., 2021), and that mixed drinking sources may be used, home treatment is probably a very useful habit to retain.

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.[18] Our main specification is thus:

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

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.

Water collection We first consider changes in time spent collecting water. Our treatment offers are for delivered drinking water, so we expect water collection time to fall. 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.[19] These impacts may represent a meaningful benefit to households, helping to rationalize their relatively high valuation of clean water. Note that water collection time does not fall to zero, consistent with households continuing to use external water sources to meet their (non-drinking) water needs.

Water purification Next, we measure the effect of our offers on actions households undertake to make their water safe to drink. Columns 2-4 of Table 2 report effects on the probability that households report boiling, chlorinating, or straining water in the past week.[20]

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

[19]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).

[20]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

We find no evidence that households change their self-reported treatment behavior, and can reject relatively modest effects in all treatment arms, suggesting that clean water offers do not crowd out (limited) ongoing water treatment efforts.[21] 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.

5.4 Effects of clean water on self-reported health

We use our survey to measure the impacts of access to Spring Health water on a series of health outcomes as reported by households. Prior evidence demonstrates that drinking clean water improves health (Kremer et al., 2023). We find that the water improves several self-reported health outcomes, which can be detected by households themselves. This result corroborates past work and alongside the time savings described above, helps explain why households value clean water and are willing to spend or forego cash to acquire it, even though

households ever report treating their water with chlorine, the share reporting chlorine usage in the average survey round is only 3.2%. In this setting, we expect households to be aware of the benefits of chlorine, as local health workers train households on the use of chlorine as a water treatment practice (Ministry of Health and Family Welfare, Government of India, 2006, 2021).

[21] In the exchangeable entitlement and free ration arms, point estimates suggest there may be declines in straining, though these are not statistically different from zero. While straining may reduce cholera and worms-based illnesses, it is ineffective for viruses, bacteria, and small protozoa such as giardia (World Health Organization, 2008).

alternatives are available. This is because it is perceived benefits that influence household demand.[22] We use two-stage least squares to estimate the local average treatment effect of drinking clean water from Spring Health (not necessarily exclusively) on self-reported illness, health expenditures, and missed work due to sickness.

We estimate local average treatment effects for each experimental arm, with first stages of:

$$
\begin{aligned}
1[\text{Drinks clean water}]_{it} \times \text{Any discount}_i &= \eta \cdot \text{Any discount}_i + \gamma_v + \delta_t + \varepsilon_{it} \\
1[\text{Drinks clean water}]_{it} \times \text{Any exchange}_i &= \eta \cdot \text{Any exchange}_i + \gamma_v + \delta_t + \varepsilon_{it} \\
1[\text{Drinks clean water}]_{it} \times \text{Free ration}_i &= \eta_1 \cdot \text{Free ration}_i + \gamma_v + \delta_t + \varepsilon_{it}
\end{aligned}
$$

And a second stage of:

$$
\begin{aligned}
\text{Y}_{it} = \; & \theta_1 \cdot 1[\text{Drinks clean } \widehat{\text{water}]_{it}} \times \text{Any discount}_i \\
& + \theta_2 \cdot 1[\text{Drinks clean } \widehat{\text{water}]_{it}} \times \text{Any exchange}_i \\
& + \theta_3 \cdot 1[\text{Drinks clean } \widehat{\text{water}]_{it}} \times \text{Free ration}_i \\
& + \gamma_v + \delta_t + \varepsilon_{it} \qquad (4)
\end{aligned}
$$

where $1[\text{Drinks clean water}]_{it}$ is an indicator for whether the household reports drinking any clean water, all other terms are defined as in Equation (3), and all sample restrictions are identical. We use an indicator for any clean water consumption as the endogenous variable rather than quantity because our primary object of interest is the effect of clean water access on health, rather than a dose-response function. Moreover, conditional on orders, water quantity is fairly homogeneous across households. Table 3 reports the results. The term "clean water" in these regression specifications (as in the remainder of the paper) refers only to the treated water of our intervention. This is because we are interested here in investigating whether this product, for which we observe substantial WTP and WTA, improves self-reported health.

We estimate that drinking clean water substantially reduces self-reported illness: the point estimates in the table correspond to reductions equivalent to 62% of control (0.3 fewer sick individuals reported per week on average, FDR-adjusted $p = 0.03$), 36% (0.18 fewer sick people reported, FDR-adjusted $p = 0.09$), and 23% (not different from zero) in the discount, exchangeable entitlement, and free ration arms, respectively. The 95% confidence intervals

[22] We note, however, that clinical outcomes are what matter for policy. We therefore do not use these self-reported health impacts in the cost-effectiveness analysis below, but nevertheless view them as useful in understanding household choices.

Table 3: Local average treatment effect of clean water on self-reported health

	Sickness (1)	Health Expenses (2)	Missed Work (3)
Drinks SH Water (Prices)	-0.32	-90.95	-0.12
	(0.13)	(24.86)	(0.04)
	[0.03]	[<0.01]	[0.02]
Drinks SH Water (Exchange)	-0.18	-97.07	-0.13
	(0.09)	(55.83)	(0.05)
	[0.09]	[0.11]	[0.07]
Drinks SH Water (Free Ration)	-0.12	-63.76	-0.08
	(0.09)	(56.03)	(0.04)
	[0.28]	[0.28]	[0.24]
N	5,271	5,219	5,271
Control Means	0.516	239.507	0.2
Price=Exchange	0.26	0.91	0.79
Price=Ration	0.23	0.67	0.52
Exchange=Ration	0.70	0.68	0.49

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. The regression includes village and month-of-sample fixed effects. Standard errors are clustered by village. FDR-adjusted p-values in brackets. Price=Exchange, Price=Ration, and Exchange=Ration are p-values on tests for equality between the price and exchange arm, price and ration arm, and exchange and ration arm, respectively.

include effects ranging from 6 to 58 pp (prices), 0 to 36 pp (exchange), and -6 to 30 pp (free ration). Drinking clean water meaningfully reduces weekly health expenditure, by 37% (INR 91 per week on average, FDR-adjusted $p < 0.01$), 41% (INR 97, FDR-adjusted $p = 0.11$), and 27% (INR 64, not different from zero) across the discount, exchangeable entitlement, and free ration arms. 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 $p = 0.02$), 13 pp (66%, FDR-adjusted $p = 0.07$), and 8 pp (40%, FDR-adjusted $p = 0.24$) in the discount, exchangeable entitlement, and free ration arms, respectively.

Again, we document evidence that clean water improves self-reported household health. Appendix Table D.3 also presents intent-to-treat effects of water offers on self-reported health, and includes a breakdown of illness into various symptoms, all of which are linked to drinking contaminated water (World Health Organization, 2017). These also indicate improvements in self-reported health, though as expected, the local average treatment effects are larger than the intent-to-treat estimates.

The impacts of clean water on self-reported sickness are largest and most precise in the price arm, while our estimated local average treatment effects are smaller and less precisely estimated in the exchangeable entitlement arm and the free ration arm (though we are unable to reject equality between our IV effects across arms). To the extent there are differences in these impacts, they are likely driven by (unobservable) selection.[23] While nearly all households order clean water in the exchangeable entitlement and free ration arms, fewer households order water in the price arm, and this varies by discount level. Column (4) of Appendix Table D.2 shows how the probability of households self-reporting that they drank treated bottled water in the last week changes with treatment offers and discount levels. More compliers come from the lower price groups, but high-price groups also screen out take-up. The LATE compliers in the price arm, who must spend to order water, are likely to be the households who are at highest risk of falling ill – precisely the prediction from the selection model we describe in Section 5.1.[24]

Improvements in self-reported health outcomes (alongside self-reported time-savings) help to rationalize households' relatively high valuation of home-delivered clean water. These results align with other papers that study self-reported health outcomes in the context of clean water interventions (Kremer et al., 2011*b*; Dupas et al., 2023). That said, self-reported health improvements are not the same as clinical benefits and therefore the cost-effectiveness calculations in Section 6 are not based on these results.

5.5 Treatment effect heterogeneity

In this section, we explore heterogeneity in our treatment effects in valuation and health.

Heterogeneous 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. 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.

[23]Lee, Miguel and Wolfram (2020*a*) document a similar phenomenon in the context of rural electrification: households who purchase electricity connections at higher prices have larger benefits.

[24]It is also possible 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, though we do not have direct evidence of this.

We cannot reject equality in WTP for any sub-group, so we treat this heterogeneity as suggestive.

Though the differences in WTP are imprecisely estimated, the point estimates imply 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, 2020*a*). This may reflect the notion that WTP and ability to pay are in better alignment in high-income households (e.g., 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).

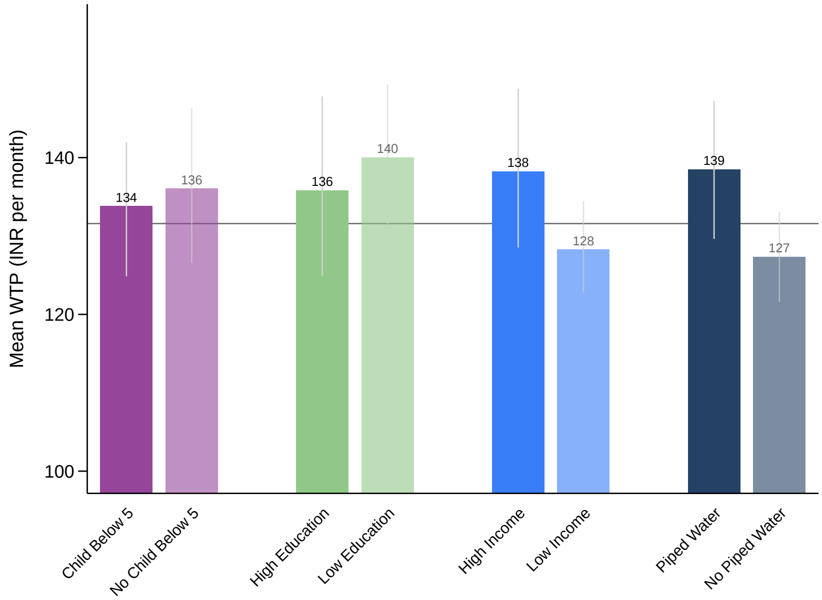
We also see suggestive evidence that households with piped water have a higher WTP than households without, which is perhaps on first glance surprising. However, two simple explanations can rationalize this. First, in order to receive piped water, households in our sample must typically expend both effort and funds.[25] 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.

There is little evidence that the presence of a small child or education levels matters for household WTP. This might suggest that households value benefits for older children or adults as well, such as fewer days of missed work.

These results provide suggestive 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.

[25] 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, linearly interpolating demand between price points. 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.

Heterogeneous health effects In Appendix Table D.4, we present results examining heterogeneity in self-reported health outcomes. We do not see statistically significant differences in treatment effects by above vs. below median education, piped water access, or above-vs.below-median education. We do see statistically significant differences in treatment effects on all three self-reported health outcomes between households with and without children under 5 in the price arm. The relevant point estimates for the other treatment arms go in the same direction but are less precisely estimated. We take this as supportive evidence that our self-reported 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.

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, and suggests that households would be willing to forgo at least INR 420 per month (or 5,040 annually) 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. Interestingly, Odisha's flagship cash transfer for rural farmers, Krushak Assistance for Livelihood and Income Augmentation, provided approximately INR 1,700 per household per month on launch (Dhillon, 2019). It may therefore be possible for the government to repurpose some cash transfer funds to clean water provision, or even provide opt-in choices to households. Because Odisha is far from alone in using large-scale cash transfers, such a policy may be worth testing more widely.[26]

Scaling clean water delivery In this experiment, we compute the partial equilibrium impacts of clean water access in 120 villages in which Spring Health had been operating prior to the study. 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, and though these villages may be different from locations where no such firm already exists, features of our intervention suggest that this may be 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 (see Appendix G for full details). We plot the results in Figure 6.

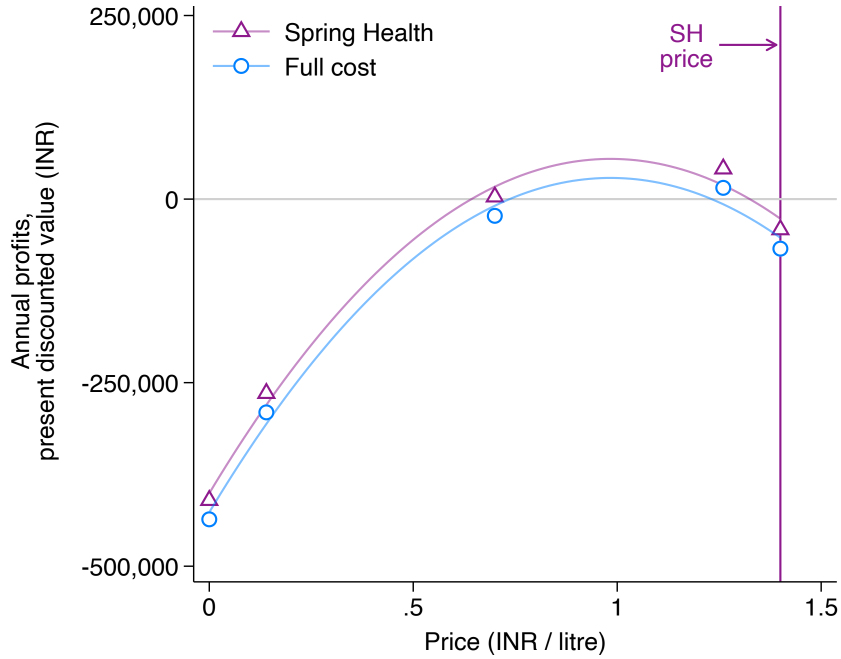
Figure 6 reveals that home delivery of clean water can be privately profitable. Moreover, under the status quo price of INR 1.4 / litre, Spring Health is only just below the profitability cutoff we estimate. Spring Health augments its water sales income with other revenue streams, including the sale of carbon credits generated by treating water using solar electricity.

However, offering the low prices required for *universal* clean water access would not be 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 so, 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.

Indeed, the private provision of clean water is proliferating in low-and-middle-income countries (Cohen and Ray, 2018), with decentralized water kiosk and home delivery models like that used by Spring Health becoming increasingly common (Advani et al., 2011; Brown

[26]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).

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, derived from the price arm, 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.

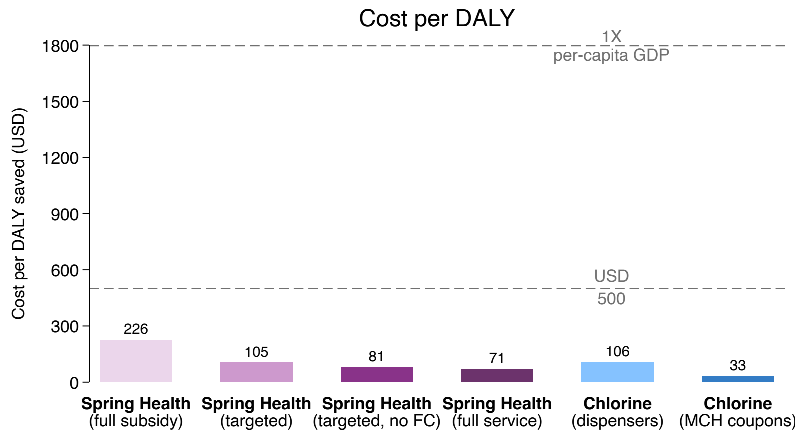
et al., 2011; Daly et al., 2021). The growth of non-tap alternatives reflects some attractive characteristics of this source of clean water. Decentralized treatment and home delivery of drinking water has much lower capital and maintenance costs than piped water, such that it can even be profitably supplied by private providers like our implementation partner. This allows the possibility of rapidly reaching geographies or households who do not yet have safe piped water.

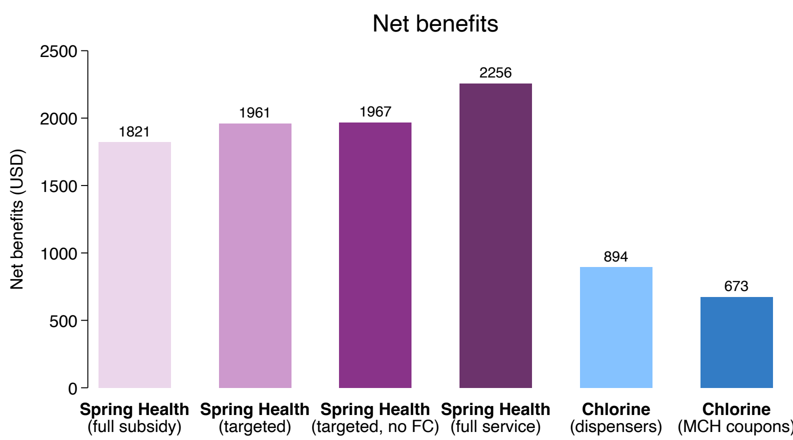
Cost-effectiveness Our data suggest that providing clean water at high prices can be privately profitable. However, our estimated demand curve predicts that only a small fraction of households will buy water at these prices. These results therefore explain why the private market has not fully solved the universal clean drinking water access problem. However, state spending on most preventive health interventions is not based on WTP alone. Rather, the cost-effectiveness of such interventions is typically tested by quantifying expected gains in Disability-Adjusted Life Years (DALYs), and comparing the cost per DALY to standard benchmarks (e.g., 1× per-capita GDP from World Health Organization (2020) or USD 500 for low- and middle-income countries from Jamison et al. (2018)). This approach reflects a social planner who places a high value on universal safe drinking water, due to positive externalities such as reduced water-borne disease transmission. By using GDP rather than wages, the WHO benchmark also seeks to capture societal multiplier effects.

We therefore calculate the cost per DALY saved and the net benefits of clean water delivery to measure cost effectiveness. The key inputs into this calculation are (i) the take-up of clean water when it is provided for free, which we estimate using our free ration arm; (ii) the impact of clean water on infant mortality, which we draw from the Kremer et al. (2023) meta-analysis; and (iii) the cost of providing clean water, which we compute using our data. We calculate cost-effectiveness for Spring Health water, under varying assumptions (subsidies for all households vs. households with children; government covering only variable or variable and fixed costs; and water availability per our experiment or 100% of the time) and for two chlorine-based approaches (chlorine dispenesers and maternal-child-health center coupon distribution), where we take the key parameters from Kremer et al. (2023), but adjust to the Odisha context. We discuss these calculations in full in Appendix G.

Figure 7 presents the results. The top panel plots the cost per DALY under each scenario, compared to standard benchmarks. We estimate a cost per DALY of Spring Health water ranging from USD 71 to 226. In this setting, dispensers cost USD 106 per DALY, and chlorine coupons cost just USD 33. All five measures are far below WHO- and World-Bank-proposed cost-effectiveness thresholds. The bottom panel plots net benefits per child, which are high in all scenarios: USD 894 in welfare gains per child from dispensers, USD 673 from chlorine coupons, and USD 1,821 to 2,256 from Spring Health water. The net benefits from Spring Health water are meaningfully higher than from chlorine in this setting due to large differences in take-up: 89–90% for Spring Health versus 36% for dispensers and 26% for coupons, even though the cost of provision per child is meaningfully lower for chlorine than Spring Health water. All of the water sources we study would remain cost-effective according to the more stringent benchmark even if costs were doubled (or benefits halved), and would continue to satisfy the less stringent benchmark up until costs increase eight-fold, suggesting that this policy result is likely insensitive to study-specific modeling choices or particularities of our sample. Broadly, these results demonstrate that both chlorine and home delivery of clean water pass a cost-benefit test with flying colors, and highlight the value of increasing take-up of clean water.

Figure 7: Cost-effectiveness of clean water





Notes: This figure presents cost-effectiveness measures of clean water under six scenarios: Spring Health (full subsidy), simulating entering a village as-yet-unserved by Spring Health, providing 100% clean water subsidies to all households, and covering fixed costs; Spring Health (targeted), simulating entering a village as-yet-unserved by Spring Health, providing 100% clean water subsidies only to households with children under 5, and covering fixed costs; Spring Health (targeted, no FC), simulating providing 100% clean water subsidies to households with children under 5 in a village already served by Spring Health and therefore excluding fixed costs; Spring Health (full service), same as (targeted, no FC) but simulating Spring Health water being available in 100% of months rather than 87.7%; Chlorine (dispensers), simulating providing point-of-collection chlorine treatment as in Kremer et al. (2011*b*); and Chlorine (MCH coupons), simulating providing coupons for point-of-use chlorine treatment via maternal and child health services as in Dupas et al. (2016). All five scenarios are set in the context of rural Odisha, using local GDP-per-capita, infant mortality, and children per household. The top panel plots costs per DALY saved, with a WHO benchmark (1× GDP per capita) and a World Bank LMIC benchmark (USD 500) represented by gray dashed lines. The bottom panel plots net benefits per child, calculated as DALYs saved (evaluated at 1× per-capita GDP) less the cost of provision. See Appendix G for more details.

Mechanisms for allocating water Finally, our results shed light on whether policy-makers 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 3, 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, 2020*b*). 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, pleasant-tasting 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 and willingness-to-accept for clean water.

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 for clean water delivery 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. Our cost-effectiveness analysis suggests that free provision of clean water would be strongly welfare-improving. 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.

There are important benefits to the two approaches most commonly studied to date. Chlorine treatment at home is still the cheapest way to remove coliform from water and tablets and solutions are easy to distribute even in very remote areas. Piped water is very convenient and if it can be kept clean, it has the potential to ensure all water used by the household is safe, limiting spillover contamination. However, it is clear that neither option is a universal solution in the short-or-medium term. Consequently, 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

Contents

A Additional context

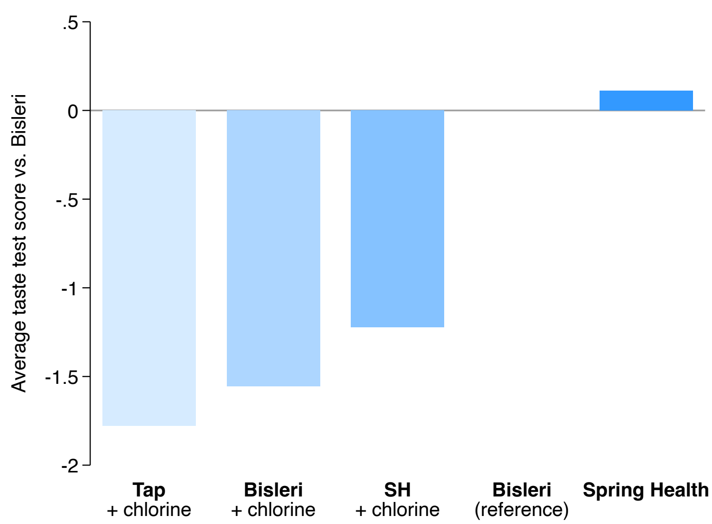
A.1 Taste testing

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

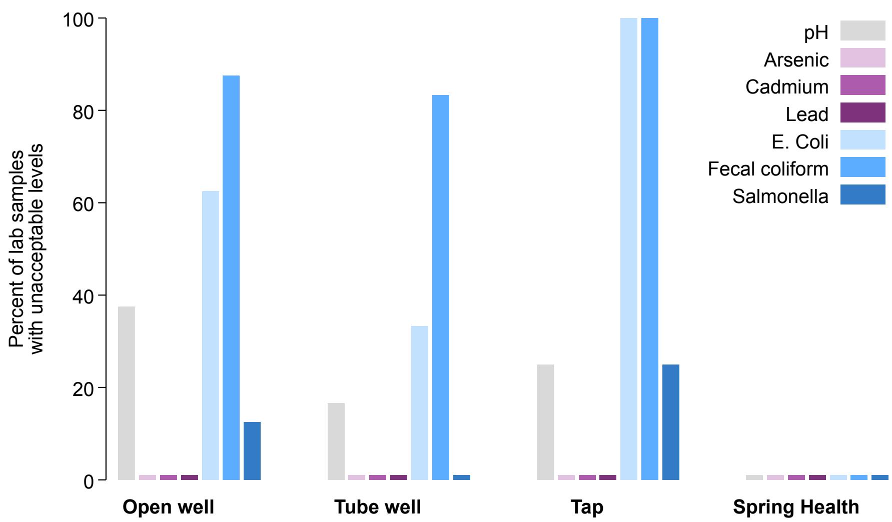


Notes: This figure plots the results from 9 water taste tests we conducted in Odisha (with separate IRB approval from the University of Chicago, IRB23-1363). All respondents were provided a sample of Bisleri, a standard bottled water. They were then provided four other sources in a randomized order: 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

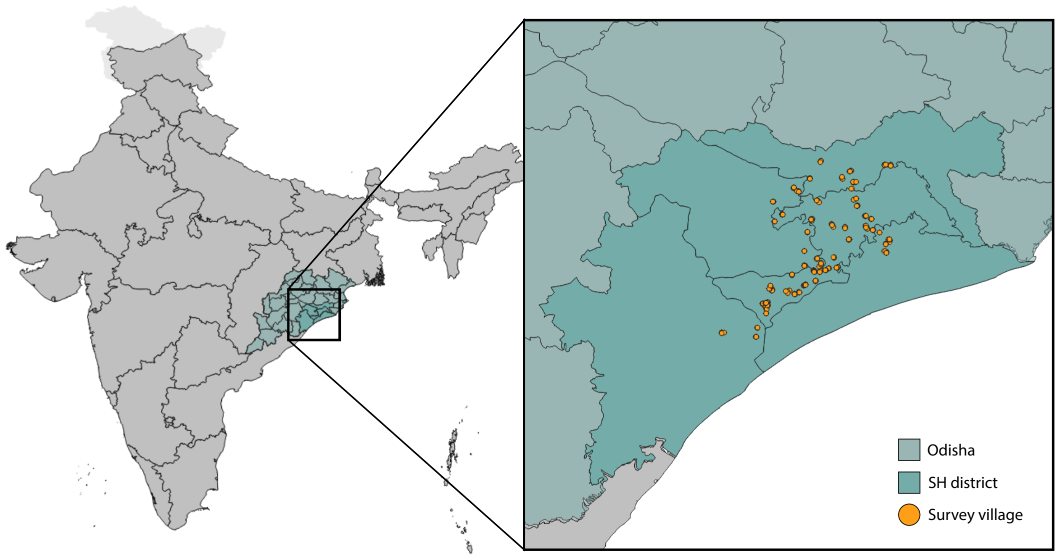
We sent 19 water samples, drawn from our treatment villages, for water quality testing at the State Water Testing Laboratory R.W.S&S. This laboratory is used by the Odisha government for official water quality measurements. We collected 8 open well samples, 6 tube well samples, 4 tap water samples, and 1 Spring Health sample. Each sample was evaluated on heavy metals and biological contaminants. Appendix Figure A.2 plots the share of samples deemed "unacceptable" (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).

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.

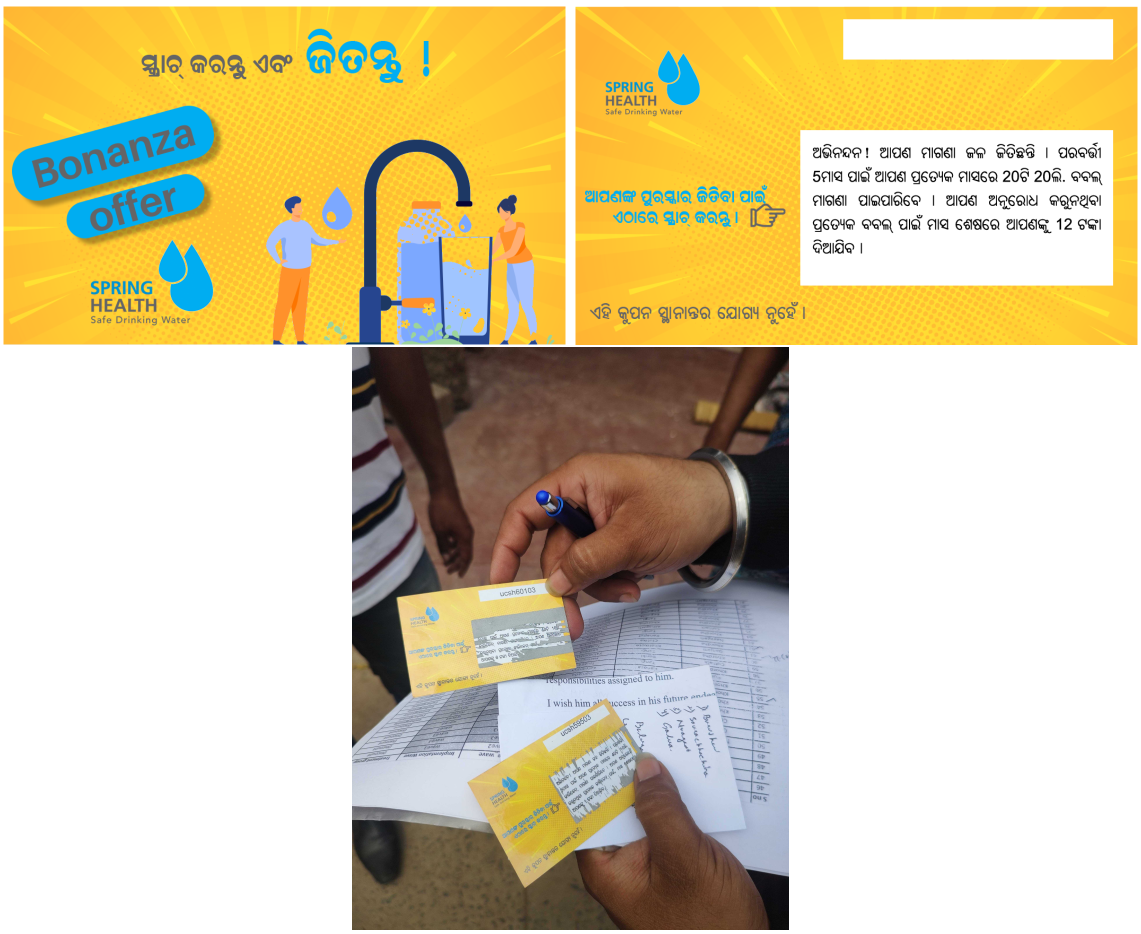
B Additional experiment details

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 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.

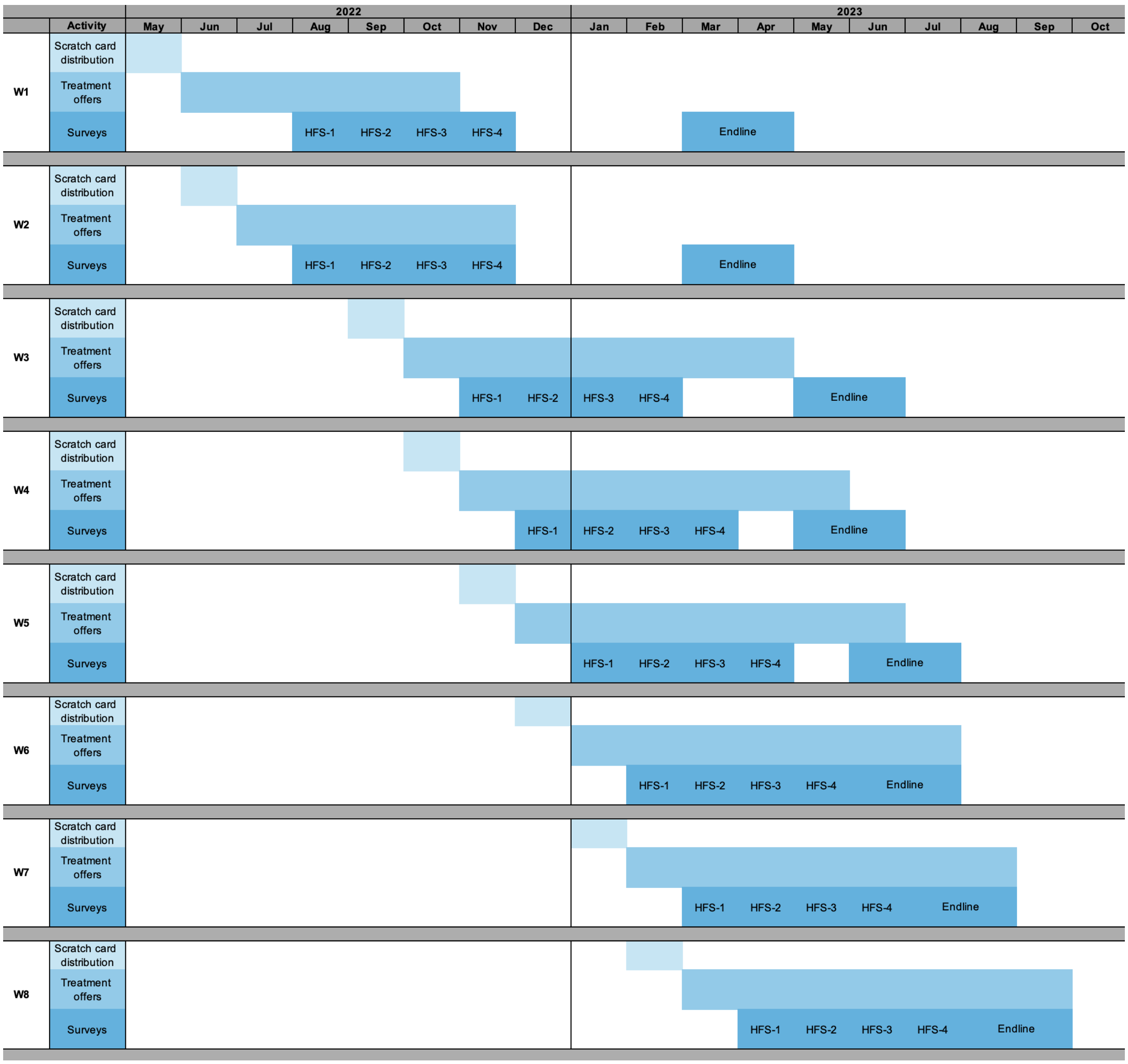
Our experiment took place between May 2022 and October 2023. Appendix Figure B.3 shows the experiment timeline in the form of a Gantt chart. For logistical reasons, the sample was divided into phases or"implementation waves" (W1–W8 on the chart). At the start of each wave, scratch cards were delivered 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.

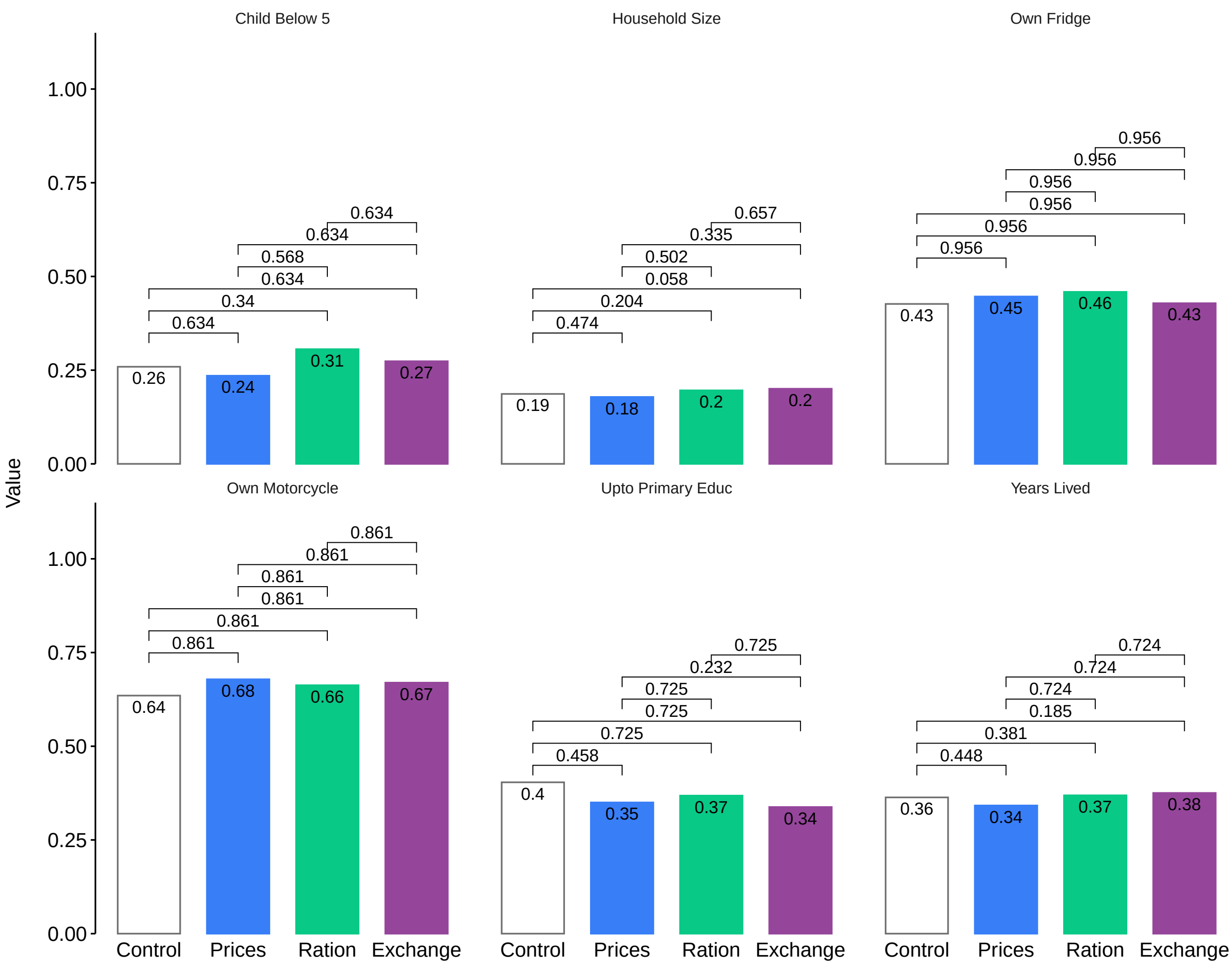
C Experimental integrity

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

C.1 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

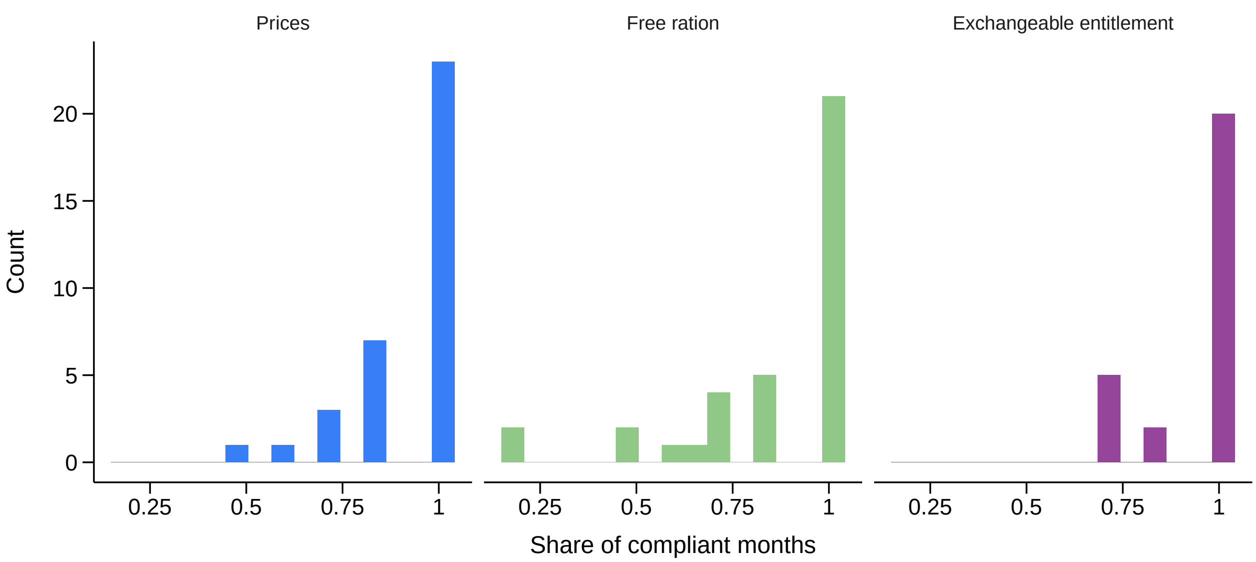


Notes: This figure shows experimental balance. Bars show average values for survey households in each treatment arm for six covariates. From top-left, these are: A dummy for the presence of a child below the age of 5 in the household, number of household members (with minimum and maximum values rescaled 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.

C.2 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; across the sample, water delivery was available for 87.7% of village-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 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. When conducting our cost-effectiveness analysis, we account for the fact that Spring Health deliveries did not occur 100% of the time.

C.3 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.

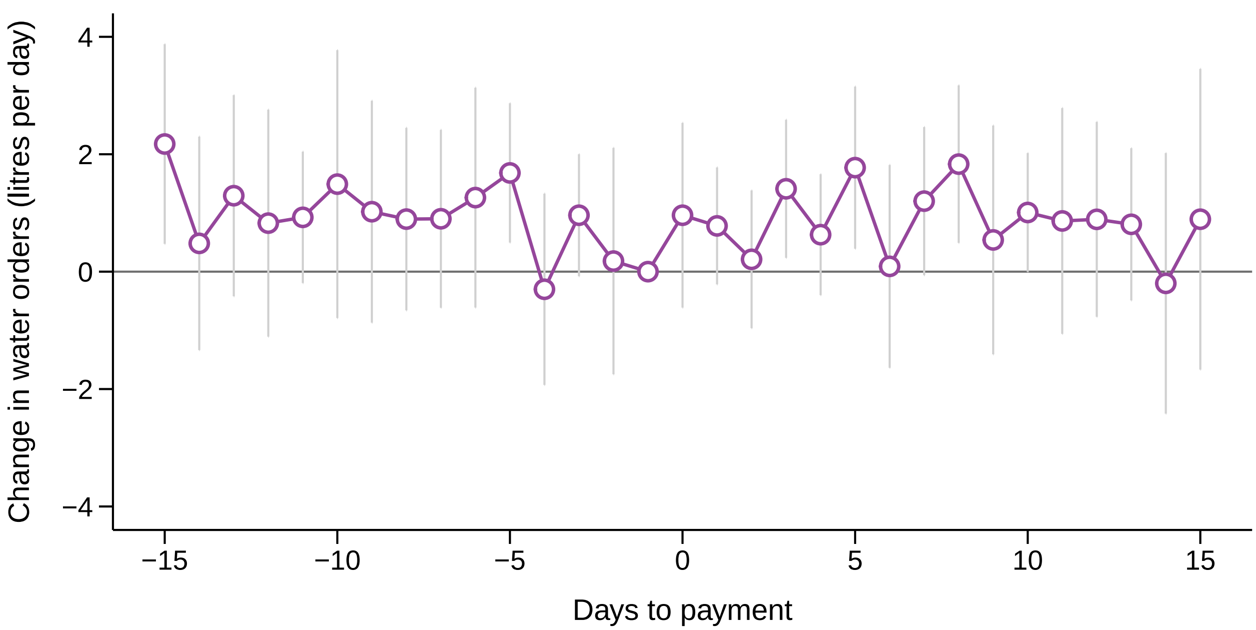
In addition, because transfers were often very small, our implementation partner sometimes clubbed payments for multiple months together creating delays.

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



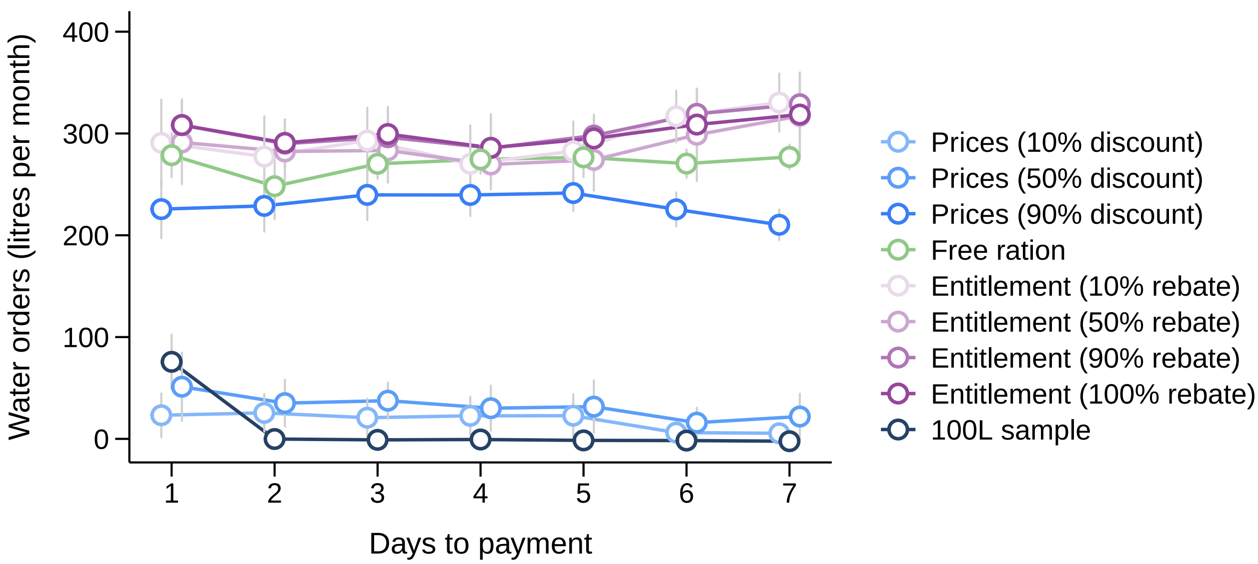
Notes: This figure plots changes in daily water orders among exchangeable entitlement households only, relative to 1 day before the timing of the first rebate payment. We estimate these coefficients using Equation C.1, which includes household fixed effects. Confidence intervals are derived from standard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

Self-reported health effects by sub-treatment Appendix Table D.1 the intent-to-treat effects of offers on self-reported health outcomes using a version of Equation (3) that estimates a separate coefficient for each sub-treatment arm. Point effects are negative across self-reported sickness, health expenses, and missed work outcomes. Standard errors are larger with the most precisely estimated effects on self-reported 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 self-reported 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), and a version of this equation where we break treatment effects out by offer level for the price and exchange arms, 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. Treatment effects grow with discount size in the price arm, but do not meaningfully vary with rebate size in the exchangeable entitlement arm. Note that Column (3) also serves as a first stage for the IV regressions we present in Section 5.4. 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)	Number of sources (2)	Drinks clean water (3)	Drinks clean water (4)	Only drinks clean water (5)	Only drinks clean water (6)
Discount	0.34		0.34		0.09	
	(0.06)		(0.03)		(0.02)	
Exchange	0.75		0.77		0.25	
	(0.09)		(0.04)		(0.03)	
Free Ration	0.61	0.61	0.63	0.63	0.17	0.17
	(0.09)	(0.09)	(0.04)	(0.04)	(0.03)	(0.03)
10% Discount		0.26		0.30		0.09
		(0.06)		(0.04)		(0.03)
50% Discount		0.23		0.24		0.07
		(0.09)		(0.04)		(0.02)
90% Discount		0.51		0.48		0.10
		(0.12)		(0.05)		(0.03)
10% Rebate		0.71		0.76		0.23
		(0.13)		(0.04)		(0.04)
50% Rebate		0.72		0.78		0.24
		(0.11)		(0.04)		(0.04)
90% Rebate		0.66		0.75		0.29
		(0.10)		(0.05)		(0.05)
100% Rebate		0.85		0.78		0.24
		(0.12)		(0.05)		(0.03)
N	5,285	5,285	5,276	5,276	5,285	5,285
Control Means	1.443	1.443	0.004	0.004	0.001	0.001

Notes: This table presents intent-to-treat effects of water offers on consumption of Spring Health water, estimated using Equation (3) in Columns (1), (3), and (5), and using a version of this same equation with effects broken out by sub-treatment arm in Columns (2), (4), and (6). Columns (1) and (2) present the effect on the number of drinking water sources, Columns (3) and (4) report drinking any Spring Health water, and Columns (5) and (6) report 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 Intent-to-treat effects on self-reported health

Appendix Table D.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 individuals in control households reporting having been sick 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 self-reported 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.[27] 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.[28] Finally,

[27] 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.

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

columns 4 through 8 present results on various individual symptoms. Because our goal was to measure treatment effects on water-borne disease, all symptoms included in the survey could have plausibly been improved by clean water; we did not include placebo measures such as broken limbs in the survey. Though these treatment effects are imprecisely measured, the broad pattern of point estimates suggests weak evidence of reductions in specific illnesses.

Taken together, our intent-to-treat results corroborate our household demand estimates: households report experiencing meaningful improvements in health as a result of our clean water offers. Because our offers broadly move households on the extensive, rather than intensive, margin of clean water use, to understand the effect of clean water access on health requires estimating a local average treatment effect. Put differently, simply scaling the results in Appendix Table D.3 by quantities ordered in litres (Column (3) of Table 1) does not yield a dose-response function, because differences in quantities across treatment arms are driven by selection into consumption. This is directly addressed in the instrumental variables estimates in the main text.

In Appendix D.4, 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. We also do not find evidence that self-reported health meaningfully *worsens* during these service disruptions, suggesting that disruptions are not more costly than the absence of clean water itself, though with the caveat that this test relies on a relatively small and selected sub-sample of villages.

Table D.3: Intent-to-treat effect of water offers on self-reported 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.

D.3.1 Heterogeneous treatment effects

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

$$\begin{aligned} Y_{it} = {} & \beta_1 \text{Any discount}_i + \beta_2 \text{Any discount}_i \times \text{Covariate}_i \\ & + \beta_3 \text{Any exchange}_i + \beta_4 \text{Any exchange}_i \times \text{Covariate}_i \\ & + \beta_5 \text{Free ration}_i + \beta_6 \text{Free ration}_i \times \text{Covariate}_i \\ & + \text{Covariate}_i + \gamma_v + \delta_t + \varepsilon_{it} \end{aligned} \tag{D.1}$$

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. 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.

Appendix Table D.4 presents heterogeneity in health intent-to-treat effects. We see statistically significant evidence that the benefits of treatment in the Prices arm on the various self-reported health 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.4: Heterogeneous effect of offers on self-reported 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.1). 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 survey outcomes

In Appendix C we present data on non-compliance during the experiment, showing that water was not delivered (sold) by our implementation partner in only 12.3% of village-months during our sample period. Our broad policy in the field was not to 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 a small number of 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 the 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.

Table D.5 presents the effects of water offers on self-reported health during months when Spring Health deliveries were unavailable, broken out by sub-treatment arm. We see no evidence of improvements in self-reported health outcomes (i.e., 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 self-reported 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.[29]

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 the highest price, relative to what might have been observed in the absence of treatment

[29]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.

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.

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 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.[30]

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

[30]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.

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.

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 hypothesis 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.[31] 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.

[31] 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 Calculating cost-effectiveness

To calculate cost-effectiveness metrics, we follow the calculation approach laid out in Kremer et al. (2023) for several clean water provision scenarios. For each scenario, we only consider benefits from reduced mortality among children under 5. We do not measure this treatment effect directly, but instead rely on the Kremer et al. (2023) meta-analysis estimate. For all scenarios, we evaluate the intervention for the rural Odisha context, using data on the per-capita GDP in Odisha, the number of children per household, and the under-5 mortality rate. All cost estimates assume 5 years of water provision. Our scenarios are as follows.

We begin with the Spring Health (full subsidy) scenario. In this scenario, we assume that *all* households in a village receive offers of free Spring Health water. To simulate a setting in which Spring Health or an equivalent has not yet entered, our cost measure includes fixed costs, and our average total costs are socialized across all households who take up clean water. We assume the control group bought zero clean water, thus our intervention take-up rate is the intent-to-treat effect of the free ration offer on take-up plus the average take-up in the control group. We scale this take-up rate by the share of village-months where Spring Health water deliveries were available during our sample period to account for service disruptions.

Second, in the Spring Health (targeted) scenario, we only provide subsidized water to households with children. This lowers the cost of provision per child, but because fewer households in the village will participate in clean water, our average total costs are socialized over a smaller number of households.

Third, our Spring Health (targeted, no FC) scenario assumes that a Spring Health water treatment plant already exists in the village, and we provide subsidies to households with children only. Thus, this scenario removes fixed costs, and uses the intent-to-treat impact on take-up from the free ration arm as our intervention take-up rate.

Fourth, our Spring Health (full service) scenario repeats the (targeted, no FC) scenario, but assumes that there are no water service disruptions (i.e., that Spring Health water is always available).

Finally, we compare these against two chlorine scenarios: point-of-collection chlorine dispensers, as described in Kremer et al. (2011*b*), and coupons for point-of-use chlorine treatment distributed through maternal and child health services, as described in Dupas et al. (2016). We use data on these two approaches from Kremer et al. (2023), adjusting the relevant parameters for the Odisha context. Because Kremer et al. (2023) does not provide information about disruptions, we conservatively assume this water is always available.

Under each scenario, we compute expected deaths averted per child under-5 as:

$$\text{Deaths averted per child} = \frac{\text{U5 mortality rate}}{100} \times [1 - \text{risk ratio}] \tag{G.1}$$

$$\times \frac{\text{Intervention take-up}}{\text{Meta-analysis compliance}} \times \text{Water availability}, \tag{G.2}$$

cost per DALY saved as:

$$\text{Cost per DALY saved} = \frac{\text{Cost of provision}}{\text{Deaths averted}} \div (\text{DALYs per life}), \tag{G.3}$$

and net benefits per child as:

$$\text{Net benefits} = (\text{DALYs saved} \times \text{per-capita GDP}) - \text{Provision cost}, \tag{G.4}$$

Table G.1 provides the values for each object in the above equations. We summarize the cost per DALY and net benefits in Figure 7.

Table G.1: Details of cost-effectiveness calculation

	Spring Health (full subsidy)	**Spring Health** (targeted)	**Spring Health** (targeted, no FC)	**Spring Health** (full service)	**Chlorine** (dispensers)	**Chlorine** (MCH coupons)	**Source**
DALYs per life	79.25	79.25	79.25	79.25	79.25	79.25	World Health Organization (2020)
Under-5 mortality rate (p.p.)	4.27	4.27	4.27	4.27	4.27	4.27	Ministry of Health and Family Welfare (2022)
Odisha per-capita GDP	1797	1797	1797	1797	1797	1797	Government of India (2023)
Meta-analysis risk ratio	0.77	0.77	0.77	0.77	0.77	0.77	Kremer et al. (2023)
Meta-analysis compliance rate	0.53	0.53	0.53	0.53	0.53	0.53	Kremer et al. (2023)
Intervention take-up rate	0.90	0.90	0.89	0.89	0.36	0.26	SH: Free ration arm; Chl: Kremer et al. (2023)
Water availability	0.88	0.88	0.88	1.00	1.00	1.00	SH: Disruptions; Chl: N/A
Treated children per household	0.4	1.0	1.0	1.0	1.0	1.0	Government of India (2011)
Cost of provision, per child (USD)	261	122	93	93	56	13	SH: Appx. F; Chl: Kremer et al. (2023)
Deaths averted, per child	0.0146	0.0146	0.0145	0.0165	0.0067	0.0048	Eq. (G.2)
DALYs saved, per child	1.16	1.16	1.15	1.31	0.53	0.38	Deaths averted × DALYs per life
Cost per death averted (USD)	17,878	8,354	6,421	5,631	8,410	2,636	Provision cost / deaths averted
Cost per DALY (USD)	226	105	81	71	106	33	Eq. (G.3)
Net benefits (USD)	1,821	1,961	1,967	2,256	894	673	Eq. (G.4)

Notes: This table reports our cost-effectiveness calculations, including the assumptions required to carry them out. Spring Health (full subsidy) assumes subsidies are given to all households, and fixed costs, which are socialized across all households who consume, are included. In this group, we assume control take-up is zero. Spring Health (targeted) assumes subsidies are only given to households with children, and fixed costs, which are socialized across only households with children who consume, are included. In this group, we assume control take-up is zero. Spring Health (targeted, no FC) assumes subsidies are only given to households with children, and fixed costs are excluded (simulating a situation in which a private firm enters on its own). Spring Health (full service) assumes water is available 100% of the time. Chlorine (dispenser) and chlorine (MCH coupons) information is taken from Kremer et al. (2023), but adjusted to use Odisha's GDP per capita, mortality rate, and number of children per household. Throughout, we follow Kremer et al. (2023) in using 5 years of costs. See the text for equations.

H Deviations from our pre-analysis plan

This experiment was pre-registered with the AEA as AEARCTR-0010545.[32] 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), 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)). 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.

- **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 3 (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.

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

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 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.

  Furthermore, rather than using PAP Equation (15), which proposes estimating heterogeneous treatment effects on each sub-treatment, we instead measure how WTP for clean water varies with our four covariates. Because take-up is close to 100% for the exchangeable entitlement and free ration arms, there is no meaningful heterogeneity in covariates, so we focus on the price arm. Estimating heterogeneity in WTP rather than demand at each price point enables us to more parsimoniously summarize the impact of household characteristics on demand.

  We present the results in Figure 5.

  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 D.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.