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Expanding Access to Clean Water for the Rural Poor: Experimental Evidence from Malawi†

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Data from an 18-month randomized trial show large and sustained impacts on water purification and child health of a program providing monthly coupons for free water treatment solution to households with young children. The program is more effective and much more cost effective than asking Community Health Workers (CHWs) to distribute free chlorine to households during routine monthly visits. This is because only 40 percent of households use free chlorine, targeting through CHWs is worse than self-targeting through coupon redemption, and water treatment promotion by CHWs does not increase chlorine use among beneficiaries of free chlorine. (*JEL* I12, I18, J13, O12, O13, Q53)

Free access to essential health products such as vaccines, antimalarial bed nets, or clean water is widely accepted as a cost-effective way to reduce the disease burden in lower-income countries (Jamison et al. 2018). This is supported by a large body of research showing that take-up of preventatives is typically very low absent very large subsidies (see Dupas and Miguel 2017 for a review). In some cases full subsidies are not enough, however. Take-up rates of free flu shots in the United States are notoriously low (Milkman et al. 2011). Full immunization rates in Udaipur, India plateaued at 18 percent even when reliable, free immunization camps were set up in villages (Banerjee et al. 2010). Duflo et al. (2019) found no effect on sexually transmitted infection rates after Kenyan youths received a large supply

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of free condoms. Also in Kenya, Dupas et al. (2016) find that only 40 percent of households who get access to a free water treatment product use it.

Common strategies to boost take-up of free health services and products involve either information (e.g., mass media information campaigns), financial incentives, or nudges (e.g., SMS reminders). Such strategies are common in higher-income countries. In lower-income countries where target populations are often rural, dispersed, and may not all have access to a radio or a phone, such strategies can be prohibitively expensive. Even when they are feasible, their impacts on take-up appear limited. A meta-analysis of ten recent studies of text message reminders in the United States, Kenya, Nigeria, Guatemala, and Zimbabwe find that they increase childhood vaccination by 13 percent, on average (Mekonnen et al. 2019). Full immunization rates increased from 18 percent to 36 percent when in-kind incentives were provided to parents in the Udaipur experiment of Banerjee et al. (2010)—an impressive doubling, but even with incentives more than half of children did not receive full immunization. What else can be done, then, to increase take-up of free preventative health services among the rural poor?

An increasingly prevalent model for health outreach in rural areas of lower-income countries is the community health worker (CHW) model. In this model, lay members of the community work (either for pay or as volunteers, depending on the model) in association with the local health care system, typically visiting households at home, providing basic diagnostic services, and referring sick patients to facilities. Recent studies have found that properly compensated CHWs can effectively be used to expand access to curative products (Wagner et al. 2019) and reduce child mortality (Björkman Nyqvist et al. 2019). Whether CHWs are effective at encouraging households to adopt specific health practices remains an open question, however. As members of the community, they might have useful information about which households are likely to reap higher returns from investing in prevention. They also have the ability to make recurrent visits, monitoring and nudging households to be consistent in their preventative behaviors.

We study the potential complementarity between CHWs and full subsidies in the context of safe water access in Malawi. In a region with a well-established, NGO-supported CHW program in which CHWs work under the government's Health Surveillance Assistants (HSAs) and are assigned 20 to 30 households each to monitor, we randomly assign households to receive either 18 months of coupons for free chlorine solution redeemable at local shops, free monthly chlorine deliveries by CHWs, or to be in a control group. We cross-randomize CHWs to incorporate active water, sanitation, and hygiene (WASH) education into their monthly household visits to test whether this increases chlorine take-up or helps sustain chlorine use over 18 months. In a neighboring region with only the HSA program (no NGO-supported CHWs), we randomize households between the coupon program and a control group.

We find that the coupon program increases verified chlorine usage rates during spot check visits from 4 to 30 percent. This effect is sustained for the duration of our follow-up period (18 months). The share of households with verified chlorine usage during at least one of the two follow-up visits increases from 6 to 45 percent. This level of usage seems to be the ceiling, however: none of the CHW-based interventions have additional impact. To start, the impact of the coupon program is not higher in the region with a CHW program compared to the region without one.[1] Within the region with the CHW program, the impact of the coupon program is indistinguishable whether or not the CHW was randomized into being trained to promote WASH ($p = 0.66$, and we can reject any positive impact greater than 5.6 percentage points at the 95 percent confidence level). Finally, and most surprisingly, relying on CHWs to deliver free chlorine to all households during monthly visits does no better than the coupon program at improving chlorine usage. In fact, it does somewhat worse: chlorine usage with home delivery by CHWs is 7.1 percentage points lower than under the coupon program ($p = 0.044$). This is due to imperfect compliance among CHWs combined with imperfect targeting. The share of households who report receiving a visit from their WASH-trained CHW in the past month is only about 70 percent, and the share receiving the free treatment solution is only 60 percent—just over half of what it should be. If CHWs targeted beneficiaries based on their underlying propensity to use chlorine, such noncompliance would not be an issue, since no more than one-third of households use free chlorine. But targeting by CHWs is imperfect, and as a result home delivery by CHWs fails to match the usage rate observed under the coupon program.

The large increase in chlorine usage under the coupon program translates into substantial child health impacts. Caretaker-reported incidence of diarrhea, fever, and vomiting—all symptoms of waterborne infections—among children under age ten goes down by 23 to 27 percent in the coupon group within 6 months and remains lower by the end of our 18-month follow-up period.[2] The health effects appear weaker under the home delivery by CHW program: an 18 percent reduction in the total number of illnesses is insignificant at conventional levels ($p = 0.24$); however, we cannot reject the null hypothesis that the effect on the number of illnesses is equal across the two programs ($p = 0.144$).

Taken together, our results suggest that (nonincentivized but well compensated) CHWs neither complement nor substitute for subsidies: they do not increase take-up absent subsidies, nor do they increase take-up of subsidies. Using CHWs as behavior change communication or nudge agents appears to be an inefficient use of their time in contexts where households are already informed of the importance of clean water during prenatal and postnatal care.[3]

Less than half of households in our sample ever use chlorine they receive for free, even in the presence of a WASH promotion program. That means that blanket free dispensing leads to substantial wastage. We test whether CHWs can effectively target the subsidies to households who have an interest in the product when specifically tasked to do so. After CHWs in the home delivery group had been doing deliveries for eight months (during which they received enough chlorine to distribute to their

[1] In Mwanza, the region without the CHW program, the impact of the coupon program on chlorine usage is greater by 6.4 percentage points with a standard error of 0.040 ($p = 0.108$), so the 95 percent confidence interval excludes any positive effect of CHW presence greater than 1.6 percentage points.

[2] One limitation of our study is that our health outcomes are not measured objectively but reported by households, and such self-reports can be subject to bias (Wolf et al. 2018).

[3] 95 percent of women in the DHS received formal postnatal care for their most recent birth (National Statistical Office 2017).

entire community), we randomized a rationing treatment: while some CHWs continued as before (no rationing), others received only 60 percent as many chlorine bottles as households in their community (medium rationing) and some only 40 percent (strong rationing). Rationed CHWs were explicitly asked to try to deliver the chlorine bottles to households most likely to use the chlorine to treat their water.[4] While fewer households received deliveries after the rationing started, the share of households with verified chlorine usage did not significantly decrease ($p = 0.96$), though we cannot reject nontrivial negative effects (the 95 percent confidence interval for the 40 percent rationing arm is $[-19.9, +9.7]$). What's more, rationed CHWs appear to leave out a number of high-return households: the health effects of home delivery with rationing is lower than that of the coupon program ($p = 0.018$ for any child illness and $p = 0.068$ for total number of child illnesses) and indistinguishable from zero, though here again the confidence intervals are quite large. Our finding that health impacts are lower in the rationed home delivery arm imply that some of the households left out from the home delivery scheme are households who have high returns from free water treatment and would redeem a coupon under the coupon program. In other words, CHWs do not effectively target the households most vulnerable to waterborne diseases.

Overall, the coupon program is more effective and more cost effective than using CHWs to target chlorine subsidies. This is because only a small number of households go through the hassle of redeeming coupons for free chlorine without using the product afterward and those with higher returns to product use are more willing to pay the hassle cost, so self-targeting under the coupon scheme is very good.

We view our study as making two main contributions. First, we demonstrate large and sustained health impacts of a program that provides coupons for free chlorine to households with young children. While Dupas et al. (2016) have previously demonstrated the effectiveness of coupons as self-targeting micro-ordeals in the context of western Kenya, that study only looked at short-term usage and did not include health outcomes. The health effects of chlorine have been studied extensively in the public health literature, but the evidence from subsidized chlorine interventions does not consistently show improvements in child health (Clasen et al. 2007; Luby et al. 2018; Null et al. 2018; Pickering et al. 2019). Thus, only measuring impacts on chlorine usage (as in Dupas et al. 2016) is not enough to assert whether the subsidy program yields welfare gains. We find that the targeting results of Dupas et al. (2016) in Kenya are sustained over time, hold in Malawi ten years later, and that effects on health can be substantial, for a very low cost. This suggests that governments facing a nontrivial waterborne disease burden may want to consider embedding a coupon program into their existing well-baby schemes, the same way most governments around sub-Saharan Africa have by now put in place free bed net distribution programs through prenatal and child clinics (Dizon-Ross, Dupas, and Robinson 2017). New parents could easily receive a booklet of coupons at the time they come for prenatal care, deliver a child, or bring a child for immunization. Coupons could then be redeemed at local

[4] There were no incentives for CHWs under the rationing treatment.

shops, pharmacies, or health facilities, depending on the procurement system that can be set up. Physical coupons may even become unnecessary as countries adopt biometric IDs—a reform currently underway in Malawi, Côte d'Ivoire, Ghana, and many others.

Second, we contribute to the growing literature on CHW programs. CHWs are a large and essential part of the health system in nearly all low- and middle-income countries. However, CHW programs across the globe are highly heterogeneous, and it is not clear how best to utilize the CHW infrastructure to improve community health. Programs evaluated to date have been a bundle of multiple interventions, as CHWs are typically asked to do many things: provide information and encouragement, provide diagnostic help and refer serious cases to a health facility, and deliver essential health products, sometimes for free and sometimes at a fee. It is unclear whether the impact of successful programs stems from the information provision, the encouragement, the surveillance, or the delivery component. Our results suggest that information provision and encouragement may only play a limited role in contexts where households have strong preferences.

I. Study Background

A. *Unclean Water, Disease Burden, and Chlorine Access*

Globally, an estimated 1.9 billion people lack access to clean water, meaning that they use either an unimproved water source or an improved source that is contaminated with fecal matter.[5] Unclean water leads to waterborne diseases, chief among them diarrhea. Diarrheal disease is the second leading cause of childhood mortality. In countries that cannot afford to provide piped water to dispersed, rural households, point-of-use water treatment by households can reduce reported child diarrhea by 29 percent.[6] Chlorine disinfects drinking water against most bacteria and keeps water protected for several weeks as long as it is stored safely. The dilute chlorine itself is not harmful to health even if widely used over a long period.[7] Additionally, chlorinated water does not need to be boiled or filtered, which saves users time and money spent on fuel while conserving environmental resources. The problem is that the current usage rate of chlorine among rural households in sub-Saharan Africa is low. At baseline in 2017, it was only 5 percent in our study context of southern Malawi. Finding ways to increase chlorine use among rural households is an outstanding challenge for the global health community as it strives to achieve the sustainable development goal of universal safe water by 2030.

In areas where a single water source is used by many households, communal infrastructure (dispensers placed at water sites) have been proposed as a delivery mechanism with the potential to increase chlorine usage (Kremer et al. 2011). But in

[5] https://www.who.int/water_sanitation_health/publications/hwt-scheme-round-1-report.pdf.

[6] See Wolf et al. (2018), Arnold and Colford (2007), Clasen et al. (2007), and Fewtrell et al. (2005) for reviews.

[7] Chlorination has a long history of use and became a standard of water treatment during the first half of the twentieth century in Europe and the United States. Long-term experience with the substance in piped water supplies has made clear that its use is safe. The European Commission, US Environmental Protection Agency, and World Health Organization support the use of chlorine for water treatment as long as it is used within acceptable dose ranges.

areas where the number of users per water source is low, the current standard approach is to encourage populations via promotional campaigns to purchase and use chlorine bottles from local shops, and take-up remains low under this model. For example, in Malawi and Kenya, where the NGO Population Services International spent over a decade promoting a diluted chlorine product locally called "WaterGuard liquid" as well as another point-of-use water treatment product locally called "WaterGuard powder," the usage rate of any WaterGuard product at the onset of this study was estimated by Population Services International at 12 percent in Malawi (2017), and we estimate it at 16 percent in Kenya.[8]

An alternative approach would be free distribution to parents of young children through health centers. This approach was tested in an experimental study in Kenya in 2007, and was shown to drastically increase usage but also to generate wastage if not all recipients of subsidized chlorine use it for water treatment. Dupas et al. (2016) found that only around 40 percent of households who had received enough WaterGuard for a year were treating their water with it during a six-month spot check visit. One mechanism proposed to reduce wastage is to require that households pay some hassle cost in order to get the free water treatment product. Dupas et al. (2016) show that coupons that required retrieval of free chlorine from a local shop achieved the same level of chlorine use as free home delivery at a much lower cost, since households not interested in using the product do not bother to redeem coupons but do not refuse the home delivery.

While targeting via coupons improves the cost effectiveness of free distribution, it falls short of achieving widespread coverage of safe water. How can the effectiveness of free distribution schemes (be they home delivery or coupon programs) be enhanced? As numerous countries scale up the use of CHWs, one potential approach is to rely on CHWs to promote the use of water treatment. There are three main mechanisms through which CHWs could increase the returns to dollars invested in water treatment subsidies. First, CHWs could increase take-up through what is known as behavior change communication—namely, explaining to households the returns to safe water and how to treat water with chlorine. Second, CHWs could increase take-up through a reminder effect: CHWs are typically tasked with visiting households monthly, and these monthly visits could act as a reminder for households to treat their water. Third, CHWs could use their local information to target the subsidies to households most likely to put the chlorine product to good use.

B. *WaterGuard/MadziGuard*

We focus on efficient distribution of a product called WaterGuard, a dilute chlorine solution (1.5 percent sodium hypochlorite) that was produced and socially marketed by Population Services International in Malawi until December 2018. Production of this solution was transferred to Pharmanova Malawi Limited from December 2018

[8] The Malawi figure comes from http://www.psi.org/country/malawi. We compute the figure for Kenya as follows: 27 million month-long treatment units sold (based on https://www.pskenya.org/wp-content/uploads/2019/04/PS-Kenya-2018-Annual-Report.pdf) for a country with 14 million households; 27 million units/12 months/14 million households = 0.16.

onward (about 12 months after our study began) under the brand name MadziGuard ("madzi" means water in the local language). For simplicity, for the rest of the paper we use the term "WaterGuard" to refer to both brands of the treatment solution. One capful of the product contains enough chlorine solution to treat 20 liters of visually clean water (two capfuls is recommended for water that is cloudy or dirty looking). The WaterGuard product was recognized throughout the study area: 51 percent of our study participants reported having ever used WaterGuard at baseline (see Table 1).

C. *Government's Health Surveillance Assistants*

The Malawi Ministry of Health has a cadre of Health Surveillance Assistants (HSAs) linked to primary-level health facilities. HSAs are tasked with delivering a range of services to their assigned communities including information on hygiene and sanitation, immunizations, antenatal and postnatal care education, and nutrition counseling, among other things. During cholera outbreaks, HSAs can be asked to provide free water treatment solution to households. Households in our sample report some nontrivial amount of interactions with HSAs on water issues. Fifty-one percent report that they ever received water management advice from an HSA (Table 1).[9] Distribution of water treatment solution by HSAs is relatively rare, however, with only 4 percent of households reporting that HSAs regularly dispense WaterGuard and less than 8 percent having ever received free chlorine from an HSA.

D. *PIH's Community Health Workers*

We worked with the CHW program run by Partners In Health (PIH). PIH works with the Ministry of Health to provide comprehensive care for about 150,000 people in the rural district of Neno in Southern Malawi. PIH complements its clinical services with outreach programs including CHWs. The CHWs employed by PIH act as "foot soldiers" to the cadre of HSAs employed by the Ministry of Health and are able to support the HSAs in the community and in the home. Whereas HSAs in Neno have a ratio of about 1:2000 population, the CHWs have a ratio of about 1:150.

CHWs are trained and paid 17,000 Malawian kwacha a month (about \$23—for reference, the annual GDP per capita in Malawi is \$340). They are supposed to visit each household in their catchment area once per month, checking on the health needs of the entire household and referring family members to clinics for care. Topics covered during home visits include child nutrition, maternal health, and management of HIV, tuberculosis, and noncommunicable diseases. Summary statistics on CHWs in the study are shown in Table A1 in the online Appendix. CHWs are 37 years old, on average, and 65 percent female. About 65 percent completed at least primary school (compared to only 45 percent of the population in their catchment area). CHWs are responsible for about 27 households and work about 11 hours per week on CHW

[9] The likelihood of having received such advice is lower for those who live farther away from a trading center. Each kilometer reduces it by 2 percentage points.

Table 1—Description of Sample

Variables	Mean	Standard deviation	P-value of joint test for orthogonality
Demographics			
Age	30.0	9.05	0.793
Education (none)	0.103	0.304	0.309
Education (standard 1–4)	0.237	0.425	0.992
Education (standard 5–8)	0.479	0.499	0.982
Education (secondary+)	0.180	0.384	0.843
Wealth index	0.001	2.37	0.383
Household size	5.12	1.82	0.691
Age of youngest child	2.31	1.48	0.288
Chlorine beliefs and use			
Ever used WaterGuard	0.509	0.500	0.208
Believes chlorine makes water safe	0.715	0.451	0.512
Believes chlorine makes water taste bad	0.464	0.498	0.310
Positive chlorine test	0.047	0.213	0.071
Water source			
Any protected source	0.707	0.455	0.171
Protected: piped	0.038	0.193	0.003
Protected: borehole	0.647	0.477	0.361
Protected: well or spring	0.021	0.144	0.856
Believes own water source always safe	0.502	0.500	0.567
HSA interaction			
HSA gave water management advice	0.512	0.499	0.661
HSA dispenses WaterGuard	0.044	0.205	0.538
HSA gave free chlorine	0.078	0.268	0.784

Notes: Data are from the baseline surveys conducted between March and April 2018. N = 2,313. P-values of joint tests for orthogonality were estimated using F-tests after regressing the variable on indicators for treatment assignment: Coupon, WASH, Coupon × WASH, Home Delivery, Home Delivery × Rationing, Coupon × Mwanza, and Mwanza. Mwanza was omitted from the F-test. The wealth index was created using principle components analysis based on household assets.

activities. Seventy-five percent have a job in addition to their CHW role, and CHW work is the primary source of income for 43 percent of CHWs. A cadre of senior CHWs has added mentoring responsibilities vis-à-vis other CHWs.

II. Conceptual Framework

In many contexts, a principal would like an agent to undertake an action that cannot be routinely observed. One tool at the disposal of the principal is to provide the agent an input that is complementary to the desired action. In our setting, the principal subsidizes chlorine solution to promote water treatment among households with young children. We consider a principal who values the health benefit of a health product, nonhealth utility, and alternative uses of funds. Denote b_i the health benefit when i uses the health product appropriately, z the dollar value of a unit of health to the principal, and h_i the binary variable indicating whether i uses the product appropriately. The household's nonhealth utility is u_i. Letting S denote the total cost of subsidies to promote use of the health good and λ the marginal cost of public funds, the principal's payoff is

$$\textstyle\sum_i \left(z \times b_i \times h_i + u_i\right) - \lambda S.$$

Because some households may accept a subsidized health good but not use it for a health purpose (or not use it at all), we distinguish between "takers" and "users," where the former includes all of those who take the good regardless of whether and how they use it and the latter refers to those who use the health good for the health purpose. From the perspective of the principal considering whether to introduce a subsidy or other measures to increase usage of the health product from some baseline level, a subsidy is preferred if the value of health benefits generated by marginal users of the product plus any changes in the nonhealth utility of marginal and inframarginal users exceed the opportunity cost of the subsidy spending:

$$\left(zb_{mar} + du_{mar}\right) use_{mar} + du_{inf} use_{inf} \; > \; \lambda s\left(take_{inf} + take_{mar}\right).$$

On the left-hand side of this inequality is the per household benefit (suppressing subscript i) to the principal of introducing the subsidy. The health benefit of the policy is the individual value of this benefit, z, multiplied by use_{mar}, the proportion of households induced to use the product under the policy. The nonhealth utility benefit to such households is represented by du_{mar}. Households that already use the health product for the health purpose experience no additional health benefit but may experience an increase in nonhealth utility du_{inf} through the reduced monetary or effort cost of obtaining the product. The proportion of such households is represented by use_{inf}. On the right-hand side is the principal's cost of the potential policy: s is the cost of the policy per household that takes up the health product. The terms $take_{inf}$ and $take_{mar}$ represent the proportion of households already taking the health product before the change and those newly induced to take it, regardless of whether they use it for the health purpose.

This simple framework helps highlight the potential tradeoffs across possible subsidy policy designs. If $use_{mar} < take_{mar}$ (i.e., the subsidy policy induces some people to take the input, but they end up not using it appropriately), then the policy leads to errors of inclusion, which are costly. Reducing the gap between use_{mar} and $take_{mar}$ will increase cost effectiveness. One potential way to reduce this gap is to encourage takers to put the product to its intended health use through information or behavioral change communication. Alternatively, the gap between use_{mar} and $take_{mar}$ can be reduced through a subsidy delivery mechanism that screens out nonusers. Our study studies both margins: we study a program that attempts to increase usage among takers (WASH encouragement by CHWs) as well as two screening mechanisms: self-screening through coupon redemption and screening by CHWs.

The effectiveness of the policy will also depend on b_{mar}, the health return to product use for households induced by the subsidy to take and use the input. As shown in Table 1, households in our sample are heterogeneous in the baseline safety of their water, with about half of households already benefiting from a protected source, suggesting that a large share of the sample may have fairly low

b_{mar}. A subsidy delivery mechanism that helps screen out these households will be more cost effective. Whether it maximizes welfare depends on the value placed on health and on the cost of the product. This highlights the importance of comparing the targeting properties of possible subsidy policies in terms of the underlying need for water treatment, not only usage. Finally, the framework highlights that the higher the baseline take-up of the product ($take_{inf}$), the higher the program cost. In our context, as shown in Table 1, baseline usage is below 5 percent, so the potential for subsidies to be cost effective is high.

III. Study Design and Data Collection

A. *Study Setting*

The study took place in an area formerly known as Mwanza District in southern Malawi. In 2003 the district was split into two districts, Neno and Mwanza, under the decentralization program. The two neighboring districts are rural and relatively poor compared to the rest of Malawi. We chose these two districts because they are very similar, yet one has a CHW program and one does not: PIH and its CHWs work only in Neno; there is no CHW program in Mwanza. Waterborne diseases are a leading cause of death for children under age five. A representative survey in 2015 shows that over 20 percent of children in the southern region of Malawi had a case of diarrhea within the prior two weeks (National Statistical Office 2017). This may stem from the fact that very few households have piped water: less than 3 percent in the 2015 survey and 4 percent in our baseline sample (see Table 1).

B. *Sample*

We sampled households from the catchment areas of the four health centers in Neno where the PIH CHW program was fully implemented. We sampled households in Mwanza that were close to the Neno border to help ensure comparability with our Neno sample. We conducted a household listing in November 2017 in the selected study areas, enumerating every household, for a total of 10,576 households in Neno and 3,946 households in Mwanza (Figure 1). Among these, households with a child under the age of six were considered eligible for the study, and a subset of those eligible were sampled for the study.

CHWs in Neno are assigned to specific clusters of households. To sample households for the study, we stratified by these clusters. Specifically, we randomly sampled 6 eligible households from clusters with more than 15 households and 4 eligible households from clusters with fewer than 15 households. To mimic this sampling strategy in Mwanza district, where there is no CHW program, we created 110 areas of geographically proximate households and randomly sampled 4 households from each.

A baseline survey was conducted between March and April of 2018. A total of 2,313 households were successfully surveyed and incorporated into the study. Table 1 shows the demographics and water-related characteristics of our sample. Since we sampled households with young children, the sample is young, with

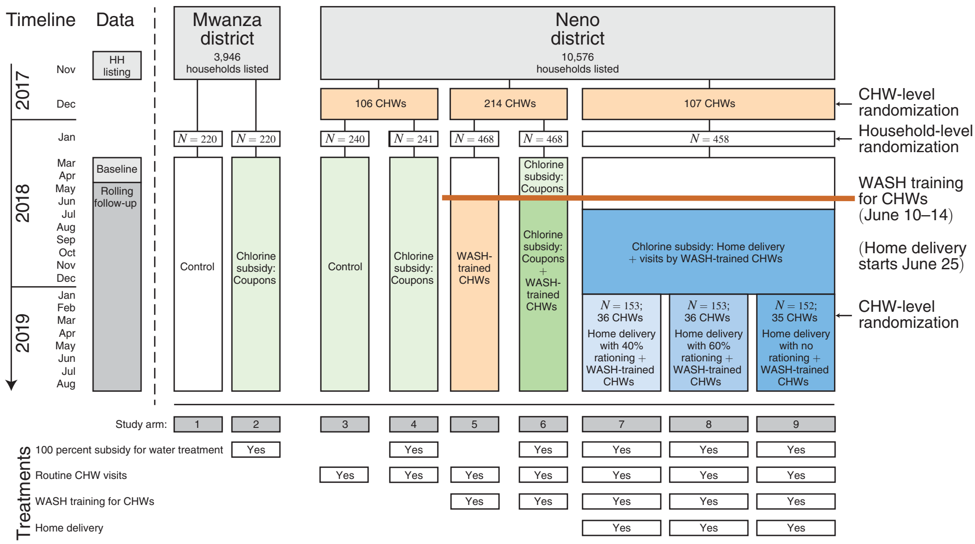

FIGURE 1. EXPERIMENTAL DESIGN AND TIMELINE

mothers aged 30, on average. The average household has just over five members. Sixty-five percent of households use a borehole as their primary source of drinking water, and 29 percent use an unprotected source such as a spring. While the sample appears quite knowledgeable about WaterGuard and its effectiveness (71 percent mention WaterGuard or chlorine when asked to list ways to make water safe), and although about half report ever using it, there is widespread concern that it makes the water taste bad (46 percent of respondents). At baseline, only 4.7 percent of households had drinking water in which residual chlorine could be detected.

C. *Experimental Design*

The experimental design and associated timeline are shown in Figure 1. We implemented a series of randomly assigned and partially nested interventions designed to test specific hypotheses, resulting in a total of nine study arms. Below we describe each intervention in turn before describing the hypotheses and, finally, the randomization procedures.

Treatment: Coupons for Free Chlorine (Household-Level Randomization).— Households assigned to the coupon intervention received coupons that could be redeemed for a free bottle of WaterGuard at a local shop. The coupons were given to them by survey enumerators at the conclusion of the baseline survey, as discussed below (see Section IIID). Households could redeem one coupon per month for 18 months. Each coupon corresponded to a unique month (e.g., the coupon for March 2018 could only be redeemed in March 2018). Coupons were attached to a calendar, which was also given to control households and which many households displayed on their walls (see Figure A1 in the online Appendix). Each coupon was exchangeable for one 150-milliter bottle of WaterGuard, enough to treat a standard 20-liter jerry can of water approximately 30 times. One bottle, if used correctly, is enough for roughly one month's supply of treated water (for drinking and cooking) for a family of five.

Shop owners were recruited to be part of the WaterGuard distribution network from whom households could redeem coupons. We recruited shops in all of the trading centers in our study area so that all households could redeem coupons at their closest trading center. The average distance to the closest shop where households could redeem coupons was 2.1 kilometers. (The median was 1.85 kilometers.) Shop owners received an initial WaterGuard supply and were restocked each month as needed. (No shop ever ran out.) All participating shop owners received a monthly stipend of MKW10,000 (roughly $13) and signed a contract agreeing to (1) accept coupons in exchange for WaterGuard bottles with no additional fee, (2) only accept coupons in the month for which the coupon was specified, (3) record each coupon redemption with a coupon serial number in a record book, (4) retain the coupon until collected by the study team, and (5) pay the cost of any missing bottles not recorded in the record book (approximately $0.50). This generated highly reliable data on coupon redemption for each household.

Treatment: CHWs as WASH Promoters (*CHW-Level Randomization*).—The week of June 8, 2018, PIH trained a random subset of CHWs on how to promote safe drinking water practices: how to talk to households about the importance of water treatment with emphasis on chlorine use, in addition to other aspects of water, sanitation, and hygiene (e.g., proper boiling methods, handwashing, sanitation of cooking areas and materials, and the safe storage of food). Trained CHWs were instructed by PIH to incorporate chlorine promotion into their monthly household visits. Therefore, households in the catchment area of a CHW assigned to this arm were expected to receive promotion of chlorine from their CHW at their homes each month.

Since the WASH training was done about two months after the baseline, some households started with the coupon only and then, after three months, their CHW received the WASH training. This allows us to test whether WASH training of and by CHWs boosts coupon redemption among households.

Treatment: Home Delivery of Free Chlorine by CHWs (*CHW-Level Randomization*).—A random subset of CHWs were trained by PIH on how to promote chlorine usage, as above, but were also assigned to deliver chlorine directly to households on a monthly basis. Starting on June 25, 2018 these CHWs received a stock of 150-milliliter chlorine bottles at routine staff meetings held monthly. They received exactly one bottle per month per household in their catchment area and were instructed to deliver one bottle per household each month. Therefore, each household with a CHW in the home delivery group was expected to receive a free home delivery of chlorine each month. (Whether this happened is an outcome of interest and will be discussed in Section IV.) As shown in Figure 1, this group did not overlap with the coupon arm: households receiving some form of chlorine either received coupons or home delivery but never both. In addition, there was no home delivery without WASH promotion (i.e., there is no home delivery equivalent to the coupons-only arm).

Rationing Intervention (*CHW-Level Randomization*).—Eight months after the start of the home delivery treatment, two-thirds of the CHWs who had been doing home delivery were selected for the "rationing" intervention. Starting in February 2019 they were given enough WaterGuard for only a subset of their household. (One-third of all home delivery CHWs received enough for 60 percent of households and one-third received enough for 40 percent; the remaining one-third continued receiving enough for all households). CHWs sampled for rationing were told that data from household surveys suggested that only a minority of households used chlorine, and therefore rationing was introduced as a wastage reduction measure and they were expected to use their local knowledge and discretion to decide to whom to give the bottles. They were explicitly asked to try to target households most likely to put the chlorine solution to good use.

Hypotheses.—In terms of the framework above, the rationale behind the coupon program is that, as shown in Dupas et al. (2016), it can reduce the gap between $take_{mar}$ and use_{mar} by screening out nonusers. The concern is that it may fail at encouraging usage among households who may have a high health return b but either do not know it or for whom the hassle of redeeming the coupon is high, e.g., due to distance or forgetfulness. Training CHWs to promote chlorine use and deliver chlorine is an attempt to palliate this concern. The rationale for the rationing intervention is that although free home delivery can help reduce exclusion errors, it may create a lot of inclusion errors—i.e., a huge gap between $take_{mar}$ and use_{mar} if distribution is not rationed. The hypothesis behind the rationing is that forcing CHWs to pick and choose which households receive a free delivery could reduce errors of inclusion, but this may come at the cost of errors of exclusion.

Randomization Procedures.—The procedures were different between the two sites, given that Mwanza does not have CHWs but Neno does. In Mwanza, households were randomly assigned to receive coupons or no coupons, stratifying on geographic areas used for sampling. In Neno, the first step was to randomly assign the 427 CHWs into three groups: status quo (25 percent), WASH training (50 percent), and WASH training + home delivery (25 percent), stratifying on gender. Households whose CHWs had not been assigned to home delivery were randomly assigned to receive coupons or no coupons, stratifying on cluster (Figure 1).

D. *Data*

Baseline Survey.—The baseline survey conducted in March and April 2018 measured basic household demographics and socioeconomic status as well as self-reported water quality and treatment and child health. We also tested for the presence of chlorine in the drinking water using a chlorine colorimeter.

All households received the same calendar at the end of the interview. For households in the coupon treatment, 18 monthly coupons were attached to the calendar, and they received an explanation for how to redeem the coupons.[10] Treatment status was blinded to both the household and the surveyor until the end of the interview.

Rolling Follow-Up Survey.—In order to trace usage over time, we conducted follow-up surveys on a rolling basis. Specifically, we visited around 260 households every month. We aimed to visit each household twice. The first round of visits started in May 2018 and was completed by mid-December 2018. The second round started immediately following the completion of the first round in mid-December 2018 and ended in June 2019. The order of the visits was random but stratified by cluster. Households that were determined to have moved away permanently as of the first visit were not sampled again. From our baseline sample of 2,313 households, we completed follow-up surveys for 2,105 households in the first round and 1,731 during the second round. The attrition in the second round was in part due to torrential floods that cut out access to entire sections of the sample area for a few months. Because we had some funds left, we randomly selected 90 households to receive a third visit in July 2019, and of these 73 were surveyed. See Section IVE for an assessment of the potential bias resulting from attrition.

Coupon Redemption Data.—Redeemed coupons were collected from enrolled shops on a monthly basis starting in March 2018, and the ID numbers on the coupons were scanned. Households for whom a coupon stub for a given month was not present at the shop are considered not to have redeemed the coupon. As discussed in Section IIIC, incentives were high for shopkeepers to keep coupons safe and our resulting redemption data is highly reliable.

CHW Survey.—We completed two rounds of surveys with CHWs in Neno District. The first round was conducted in November 2017, prior to the CHW-level interventions. It primarily collected data on CHW demographics. The survey was self-administered during routine area-level monthly staff meetings: an enumerator attended the meeting and explained each question to CHWs who were present, and CHWs filled in the questionnaires on their own.

A second round of CHW surveys was completed in June 2019. The survey elicited views on the intervention and asked about household attitudes toward WaterGuard as perceived by CHWs and the CHWs' WaterGuard distribution activities (if applicable).

E. *Outcomes of Interest*

Chlorine Usage: Chlorine Tests.—Household chlorine use was measured using colorimetric tests. Respondents were asked to provide a cup of water from their drinking water reserve, and enumerators added reagent powder, which turned a shade of pink if residual chlorine was present. Enumerators compared this shade

[10] It is possible experimenter demand effects influenced coupon redemption in the first couple of months, but we would not expect survey demand effects to influence redemption in the long term.

against a provided color wheel to determine the concentration of chlorine in the water. Two tests were done: one with free chlorine, which measures the concentration of available chlorine (i.e., whether there is any chlorine left in the water to keep it safe) and one for residual measures (i.e., whether there are any byproducts of a chlorine reaction). The two measures differed less than 1 percent of the time, so the study results are identical across the two measures and, for simplicity, we focus on just one for the analysis (residual chlorine). Households with a nonzero concentration of residual chlorine are considered to have treated their water. Households with no drinking water reserve at the time of the visit are coded as not having treated water (158 observations, 5 percent of total).[11]

Child Health.—We measured three common illnesses in children that can be caused by consumption of contaminated water: diarrhea, fever, and vomiting. Enumerators asked caretakers about illness in all of their children under ten years old that occurred within the last four weeks. We analyze the health data at the child level. We create a dummy for "any illness" and also consider the total number of illnesses experienced. Online Appendix B shows the results for each illness type separately, as well as for different recall periods and different age groups. We discuss potential reporting bias—in particular, the risk of experimenter demand effect—in Section IVE.

We also measured coughing. Waterborne pathogens common in our setting do not have coughing as a symptom, but coughing is a symptom of indoor air pollution, which is high in rural Malawi, where wood burning is the primary source of cooking fuel (91 percent of households in our sample). There are two potential indirect channels through which water treatment can reduce indoor air pollution exposure: by reducing the need to boil water and by reducing illness spells during which children stay indoors. We discuss impacts on coughing in Section IVE.

F. *Estimation Strategy*

We provide results in both graphical and regression formats. The graphics show the raw data month by month—one nice feature of our rolling follow-up surveys is that we can trace out impacts over time. This also allows us to study how treatment effects vary with seasons. The regression results are based on the following fully interacted specification:

$$
\begin{aligned}
(1) \quad y_{it} = {} & \beta + \theta \text{COUPON}_i + \alpha \text{WASH}_{it} + \delta(\text{WASH}_{it} \times \text{COUPON}_i) \\
& + \sigma \text{MWANZA}_i + \rho(\text{MWANZA}_i \times \text{COUPON}_i) + \nu \text{DELIV}_{it} \\
& + \psi(\text{DELIV}_{it} \times \text{RATION}_{it}) + \gamma_t + \epsilon_{it},
\end{aligned}
$$

[11] Table B1 in the online Appendix shows that results are similar when we exclude these households rather than coding them as "no treated water."

where y represents the (follow-up) outcome for household i in month t of the study, COUPON is an indicator for whether the household is in the coupon group, WASH is an indicator for whether the household's CHW has received the WASH promotion intervention (this turns from zero to one on June 15, 2018 for households whose CHW was assigned to training), MWANZA is an indicator for whether a household resides in Mwanza district, and DELIV is an indicator for whether the household's CHW was assigned to the home delivery arm (this turns from zero to one on June 25, 2018 for households with a CHW assigned to home delivery).[12] RATION is an indicator for whether the household's CHW was assigned to one of the two rationing arms (this turns on in February 2019 for households with a CHW assigned to rationing).[13] Finally, γ_t is a vector of indicators for each month of the study (i.e., month fixed effects). Month fixed effects are important because the interventions started at different points in time, and water quality and child health vary across seasons. The interaction terms allow us to estimate separate treatment effects for each treatment arm outlined in Section IIIC and Figure 1. In this setup θ represents the coupon treatment effect in Neno district for the households that did not have a CHW assigned to the WASH promotion or delivery arm, α represents the effect of WASH promotion training in absence of the coupon intervention, δ represents the additional effect of the coupon intervention when the CHW is also assigned to incorporate WASH promotion, σ is the effect of living in Mwanza relative to Neno, ρ is the additional effect of the coupon intervention in Mwanza (such that the total coupon effect in Mwanza is $\theta + \rho$), ν is the effect of the home delivery intervention, and ψ is the effect of the rationing intervention relative to home delivery without rationing. Our main analysis pools all postintervention time periods. Analyses controlling for baseline characteristics—including baseline values of the outcome of interest, when available—yield similar results and are shown in Table B2 in the online Appendix. We cluster standard errors at the CHW level in all analyses because the WASH promotion and home delivery interventions were both assigned at the CHW level in Neno.[14] This produces conservative standard errors for the coupon treatment effect, since the coupons were randomized at the household level.

G. *Balance between Study Arms at Baseline*

We estimate equation (1) using data from the baseline survey to assess balance on our main outcomes prior to the start of the interventions. The results in Table A2 in the online Appendix show some imbalance. Neno households assigned to the coupon arm without WASH promotion were 3.4 percentage points ($p = 0.100$) less likely to use chlorine at baseline than control households. Neno households assigned to a WASH-trained CHW were 2.9 percentage points more likely to use chlorine at baseline if assigned to coupons than no coupons (sum of Coupon and

[12] There was a ten-day delay between WASH training and dispensing of the WaterGuard to CHWs.

[13] In the main specification (Table 2) we pool the rationing arms, but we study the impacts of rationing levels separately in a specification focused on the home delivery group in Table 5.

[14] In Mwanza clusters are households since there is no CHW program and the only intervention (coupons) was randomized at the household level.

Coupon $\times$ WASH coefficients, $p = 0.041$). These differences are statistically significant but economically very small and will be dwarfed by our estimates of the coupon treatment effects below, so the small imbalance does not affect our ability to estimate causal impacts, and controlling for baseline levels in the analysis does not change the results (Table B2 in the online Appendix). Households assigned to coupons and the WASH intervention were balanced with control households on chlorine use (adding the coefficients and interaction terms gives $-0.034 - 0.024 + 0.063 = 0.005$ with $p = 0.808$). Also note that when pooling all arms that received coupons, the coefficient on coupon assignment at baseline was trivial (pooled coupon effect of 0.4 percentage points, $p = 0.717$). Study arm assignment is not associated with the probability of a CHW visit in the four weeks prior to the baseline survey. The likelihood of any child illness is 5 percent lower in the pooled coupon group (-3.5 percentage points, $p = 0.044$) and number of illnesses is 6 percent lower (0.07 illnesses, $p = 0.038$).

IV. Results

A. *Impacts on Chlorine Usage and Child Health*

Table 2 shows the impacts on our primary outcomes of interest: child health and whether we could detect chlorine in the household's drinking water during unannounced follow-up visits. This table also includes results for self-reported WaterGuard (likely an upper bound) and whether the household gave any WaterGuard away (spillover).

Both subsidy interventions had large impacts on water treatment rates. When pooling all arms that received coupons, the coupon intervention increased the likelihood of a positive chlorine test by 26.5 percentage points (see "Pooled Coupon Effect" at the bottom of Table 2), while the home delivery intervention increased it by 19.4 percentage points. These are vast improvements in chlorine use compared to the control group (only 3.8 percent of the control households had a positive chlorine test at follow-up).

The large increase in water treatment rates from the coupon program led to significant improvements in child health as reported by the caretakers: the likelihood of a child under age ten experiencing any of the three illnesses in the past month decreased by 9.0 percentage points (pooled effect) in the coupon group (from a base of 43.8 percent). This corresponds to a 20.5 percent decrease, much larger than the 5 percent imbalance observed at baseline (Table A2 in the online Appendix). The number of illnesses decreases by 0.143 from a base of 0.591, a 24 percent reduction (Table 2, column 5). While there was a significant reduction in child illnesses in Neno, the effect in Mwanza, where child health was substantially worse in all categories at baseline (0.914 illnesses compared to 0.591 in Neno), appears larger (an additional 0.066 decline in number of illnesses), though the confidence interval is large and we cannot reject equality of the effects between the two samples.

Figure A2, panel A in the online Appendix plots coefficient estimates of the health effects of coupons estimated quarter by quarter. The health effects start being noticeable only after the first quarter. This could be because health impacts are cumulative: as young children experience fewer illness episodes, they become stronger and able

Table 2—Impacts on WaterGuard Adoption and Child Health

Variables	Positive chlorine test (1)	Self-reported chlorine use (2)	Gave WaterGuard away (3)	Any child illness (4)	Number of illnesses (5)
Coupon	0.256	0.296	0.121	−0.076	−0.091
	(0.027)	(0.033)	(0.020)	(0.030)	(0.045)
Coupon × Mwanza	0.064	0.049	0.035	−0.018	−0.066
	(0.040)	(0.044)	(0.031)	(0.045)	(0.068)
WASH	0.007	0.022	−0.015	0.019	0.039
	(0.015)	(0.015)	(0.009)	(0.030)	(0.043)
Coupon × WASH	−0.014	−0.008	0.057	−0.021	−0.081
	(0.035)	(0.040)	(0.027)	(0.037)	(0.060)
Home delivery	0.194	0.227	0.005	−0.034	−0.056
	(0.026)	(0.026)	(0.012)	(0.031)	(0.048)
Home delivery × rationing	0.002	0.035	0.008	0.042	0.053
	(0.042)	(0.043)	(0.018)	(0.033)	(0.056)
Mwanza	−0.023	−0.003	−0.016	0.084	0.174
	(0.015)	(0.016)	(0.010)	(0.033)	(0.046)
Child age (years)				−0.032	−0.057
				(0.003)	(0.005)
Observations	3,793	3,576	3,893	6,623	6,623
Controls	No	No	No	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes
Pooled coupon effect	0.265	0.304	0.156	−0.09	−0.143
P-value pooled coupon effect	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
Total rationed HD effect	0.196	0.262	0.013	0.008	−0.004
P-value rationed HD effect	< 0.001	< 0.001	0.414	0.831	0.951
P-value coupons versus HD	0.044	0.009	< 0.001	0.06	0.144
P-value coupons versus rationed HD	0.149	0.227	< 0.001	0.018	0.068
Neno control group mean	0.038	0.034	0.013	0.438	0.591
Number of clusters	847	841	848	844	844

Notes: Data are from follow-up surveys conducted on a rolling basis between May 2018 and July 2019. The order in which households were surveyed was randomized, with stratification at the CHW level. Households were sampled to be surveyed twice, with an average gap of 6.4 months between the two follow-ups. Child illness was measured within the previous month for children under ten years old. Any child illness (column 4) indicates whether the child had any of the illnesses we measure (diarrhea, vomiting, and fever) and was constructed from caretaker reports. Column 5 shows marginal effects from a Poisson regression of the count of illnesses reported (zero to three) with standard errors estimated using the delta method. The pooled coupon effect is the weighted average of the effect in the three arms with coupons: Neno WASH, Neno no WASH, and Mwanza. The Total Rationed HD effect is the effect of home delivery under rationing compared to the control. Standard errors clustered at the cluster level are in parentheses. A cluster is a CHW catchment area in Neno and a household in Mwanza. HD stands for home delivery.

to better fend off future illnesses. Health effects could also be somewhat seasonal, with a reduced waterborne disease burden during the dry season (May–July).

When pooled across all periods, the health impacts of the home delivery intervention are about half the size that of the coupon intervention in Neno and not statistically significant (Table 2). Figure A2, panel B in the online Appendix shows that the effects vary across quarters, however, with significant drops in illnesses during the wet season.

Figure 2 shows the percent change illness by illness for coupon and home delivery interventions based on the coefficient estimates shown in Table A3 in the online Appendix. The coupon intervention significantly reduced episodes of diarrhea (23.1

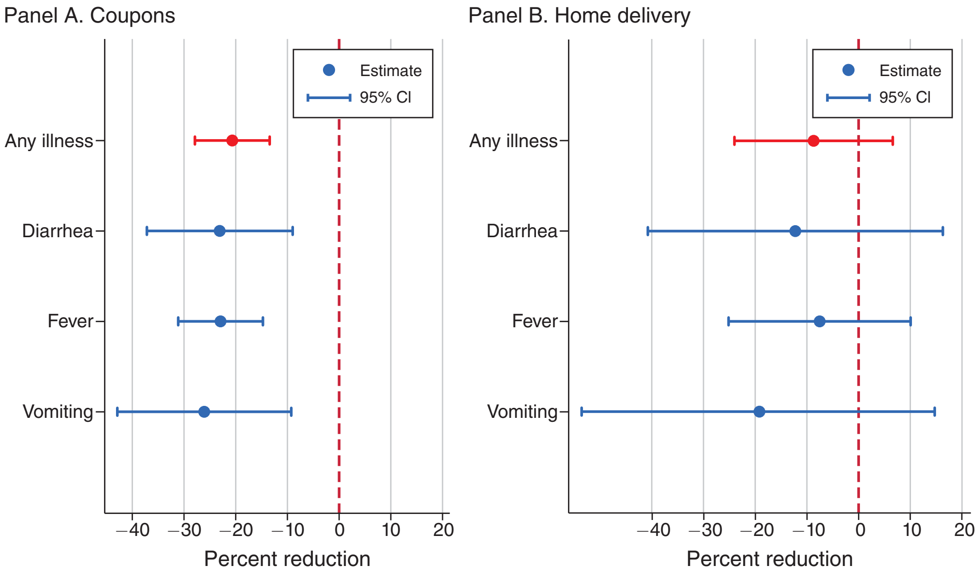

Figure 2. Percent Change in Child Illness

Notes: Percent changes are from regressions reported in Table 2 and Table A3 in the online Appendix. We used the margins command in Stata to estimate the share of children that had each illness with and without the intervention, and each point represents the percent difference in the intervention group relative to the control group. 95 percent confidence intervals are based on the upper and lower ends of the confidence interval of the difference divided by the level in the control group.

percent reduction), fever (22.9 percent reduction), and vomiting (26.1 percent reduction). Child illness reductions from the home delivery intervention were of important magnitudes (an 18 percent decrease in the number of illnesses) but imprecisely estimated, likely due to the heterogeneity across quarters shown in Figure A2 in the online Appendix.[15]

B. *WASH Promotion through CHWs Does Not Increase Usage*

Although the two subsidy schemes (coupons and home delivery by CHWs) generate a considerable increase in water treatment rates compared to baseline levels, the usage rate is far from universal, with at most 40 percent of households using free chlorine at a given point in time. Can the usage rate be increased among subsidy beneficiaries through behavioral change communication? In the terms of Section II, can use_{mar} be increased as much as $take_{mar}$? In contrast to our hypothesis going in, the presence of a CHW program does not seem to increase chlorine usage even when CHWs are specifically trained on WASH promotion. In Mwanza, the region without the CHW program, the impact of the coupon program on chlorine usage

[15] Our main results use a four-week recall for illnesses among children under age ten. Table B3 in the online Appendix shows that our results for diarrhea are robust to different recall periods (14 days and 7 days); if anything, the relative increase in the pooled coupon effect increases with shorter recall. (We cannot do this exercise for vomiting and fever because we did not collect information allowing for different recall periods for them.) Table B4 in the online Appendix provides the results for children under age five. They are similar to results for children under age ten.

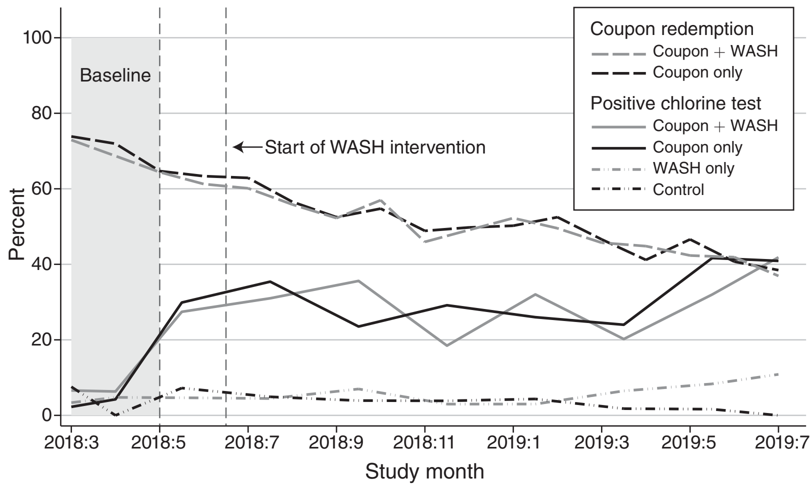

FIGURE 3. COUPON REDEMPTION AND VERIFIED USAGE OVER TIME

Notes: An average of 267 households were surveyed each month at follow-up; chlorine test results are pooled in two-month bins to reduce noise (e.g., May and June 2018 are pooled). All chlorine tests from the baseline period (March and April 2018) were conducted before the households received coupons; however, coupon redemption started for some households before all households received coupons.

is greater by 6.4 percentage points with a standard error of 0.040 ($p = 0.108$), so the 95 percent confidence interval excludes any effect of CHW presence greater than 1.6 percentage points. Within Neno, the region with the CHW program, direct delivery by trained CHWs did not yield higher usage rates than the coupon scheme and training CHWs on WASH did not influence overall chlorine usage in the coupon group (Table 2, column 1, row 4: $p = 0.688$). WASH training did not affect coupon redemption (Figure 3), nor did it influence chlorine usage conditional on having redeemed the coupon.[16]

C. *Targeting*

Increasing the usage rate among takers appears difficult. This suggests that targeting subsidies to those most likely to use the product is important. The main rationale for a coupon scheme is that it can generate self-targeting: since there is essentially no use for a small bottle of diluted chlorine outside of water treatment and very little (if any) resale value, households that have no intention to use the product may not bother redeeming the coupon. We show the patterns of results for the coupon intervention month by month in Figure 3 (coupon redemption and usage) and Figure 4 (child health). We find substantial self-targeting: while coupon redemption decreased over time from 74 percent in month 1 to 40 percent in month 18, chlorine usage (as measured through the water tests) holds steady at

[16] This is true regardless of the CHW's characteristics, which we assessed by interacting the WASH indicator with CHW age, education, number of households, hours spent on CHW activities, whether CHW is their primary source of income, and whether they were a senior CHW.

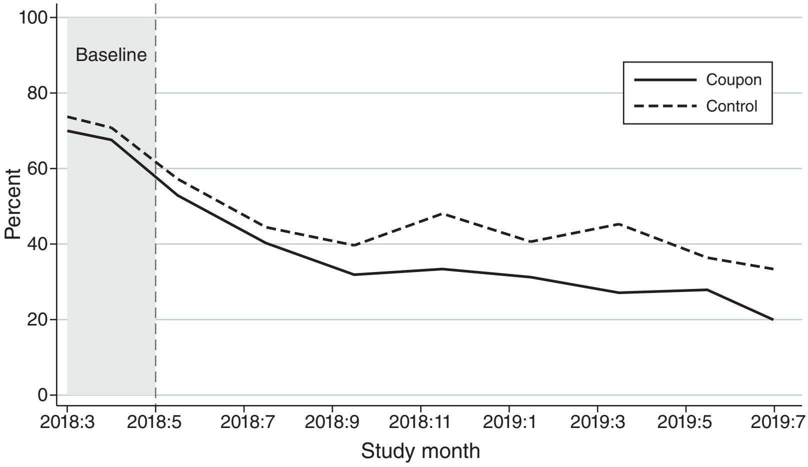

FIGURE 4. SHARE OF CHILDREN WITH ILLNESS IN PREVIOUS FOUR WEEKS OVER TIME

Notes: The lines plot the share of children under ten years old from surveyed households who had at least one illness among diarrhea, fever, and vomiting. Results are pooled in two-month bins to reduce noise. Each group pools respondents from the non-WASH and WASH subgroups.

about 30 percent of coupon households for the duration of the study, with a slight uptick in the last few months. This suggests that the drop in coupon redemption in the first few months is among people who had weaker preferences for chlorine use and that self-targeting improved over time. What's more, as shown in Figure A2, panel A in the online Appendix, we find that meaningful health effects emerge after 6 months of exposure to the subsidy (just as coupon redemption plateaus) and persist throughout the remaining 12 months of observation, suggesting that the health return for self-selected marginal users under the coupon scheme (b_{mar}) is high. This has promising implications for the efficiency of this program in the long run.

So far we have shown that about 30 percent of households that received coupons treat their water in a given month. However, it is unclear if it is the same 30 percent of households each month or if households treat some months but not others. This has implications for chlorine promotion programs, targeting chlorine to users, and for understanding chlorine preferences. We provide insight into these issues by analyzing the distribution of the number of coupons redeemed and by analyzing consistency in their use across the waves of water testing.

Figure A4 in the online Appendix shows that there is bunching in the distribution of the number of coupons redeemed around 0 and 1 (15 percent of households) and around 16, 17, and 18 (25 percent of households).[17] To assess consistency in chlorine use, Table 3 estimates treatment effects on whether households had a positive chlorine test at any of the two follow-up visits (column 1) and on whether they had a positive test at all follow-up visits (column 2). The pooled coupon effect shows that

[17] Most households had the opportunity to redeem 18 coupons, but about 10 percent did not receive their coupons until the first month had already passed. Some received their coupons toward the end of the first month and therefore might not have had time to redeem the first coupon.

Table 3—Consistency in WaterGuard Use

Variables	At least one positive chlorine test (1)	Always positive for chlorine (2)
Coupon	0.402	0.155
	(0.0399)	(0.0290)
Coupon × Mwanza	0.0533	0.0402
	(0.0589)	(0.0422)
WASH	0.0273	0.0139
	(0.0258)	(0.0125)
Coupon × WASH	−0.0579	−0.0436
	(0.0552)	(0.0367)
Home delivery	0.348	0.122
	(0.0401)	(0.0260)
Mwanza	0.00784	−0.0116
	(0.0269)	(0.00820)
Observations	1,499	1,499
Pooled coupon effect	0.39	0.146
P-value pooled coupon effect	<0.001	<0.001
P-value of coupons versus home delivery	0.417	0.612
Neno control group mean	0.064	0.012
Number of clusters	763	763

Notes: Data are collapsed to the household level and include only households that had at least two follow-up surveys after their assigned intervention started. Follow-up surveys were conducted on a rolling basis between May 2018 and July 2019. The order in which households were surveyed was randomized, with stratification at the CHW level. Households were sampled to be surveyed twice with an average gap of 6.4 months between the two follow-ups. "At least one positive chlorine test" indicates that the household's drinking water had a positive chlorine test during at least one of the follow-up visits. "Always positive for chlorine" indicates that the household had a positive chlorine test at all of the follow-up visits. The pooled coupon effect is the weighted average of the effect in the three arms with coupons: Neno WASH, Neno no WASH, and Mwanza. Standard errors clustered at the cluster level are in parentheses. A cluster is a CHW catchment area in Neno and a household in Mwanza.

coupons increased the likelihood of having at least one positive chlorine test by 39.0 percentage points and of consistent chlorine use by 14.6 percentage points. Thus, more than half of the households that used chlorine as a result of the coupon intervention did not use it consistently, even in the WASH group. This could be because households only use chlorine at specific times—e.g., when a child is ill or when the main water source is suddenly contaminated. Alternatively, this could be because other barriers such as lack of buckets, inconvenience, or cognitive overload prevent people who would prefer to consistently use free chlorine from doing so. This is consistent with a recent experiment in Kenya, which shows that interventions that increase self-efficacy as well as the salience of chlorination can increase chlorine usage by 25 percent (Haushofer et al. 2019).

Targeting on Health Returns: Heterogeneity by Baseline Water Quality.—Not all households may need chlorine all the time to have clean water. At baseline, 71 percent of households report using a protected source (public tap, borehole with handpump, protected well, protected spring, or private tap) and 50 percent believe that their water source is always safe. While households with a protected source could still benefit from chlorine use because contamination could occur during storage at

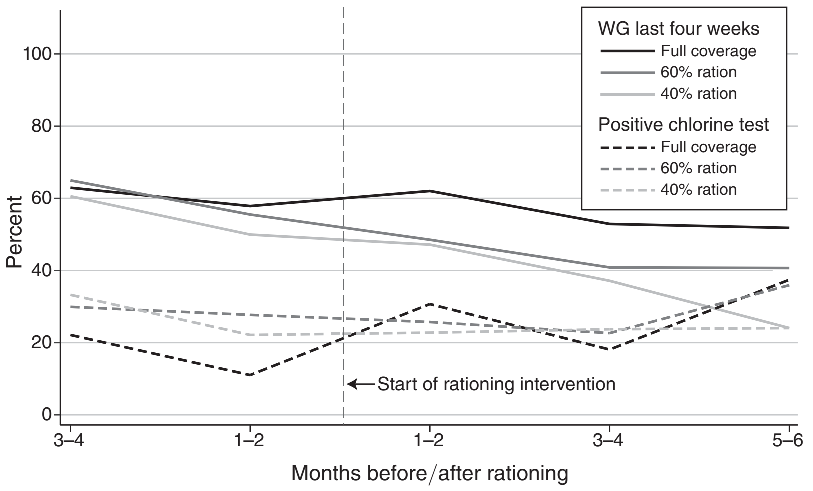

Figure 5. Home Delivery Group: Receipt of Free WaterGuard and Verified Use by Rationing Assignment

Notes: Dashed lines plot the share of surveyed households that had chlorine-treated water (tested by the study team) for the four months prior to the start of rationing (February 2019) and six months while rationing was ongoing. Shares are averaged across two months to reduce noise. Solid lines plot the share of surveyed households that reported receiving a free bottle of WaterGuard in the previous four weeks.

home, the returns to using chlorine may nevertheless be much lower for such households. Table A4 in the online Appendix zooms in on the control group and examines the correlation between beliefs about water cleanliness and child illness. We find a strong negative correlation between whether the respondent believes water is always safe and child illness incidence and, likewise, a strong negative correlation between "protected source" and child illness. This suggests that water sources for these households are truly less likely to be contaminated.

Ideally, the social planner would only target subsidized chlorine to households with contaminated water. We test for how well coupons and home delivery by CHWs target chlorine to households with contaminated water by examining whether baseline water cleanliness predicts chlorine use and child health at follow-up. Figure 6 shows that the coupon effect on chlorine use was significantly larger for households with an unprotected water source (a 36 percentage point increase compared to a 23 percentage point increase; $p < 0.001$ for both). Correspondingly, the impact on child health is also larger for such households, though not significantly ($p = 0.110$). These results imply a nontrivial degree of self-targeting based on the perceived value of chlorine treatment.

In contrast, the effect of the home delivery treatment on chlorine usage is not greater for households with worse water access at baseline (Figure 6). To try to understand why, Table 4 tests whether households that have a less clean water source are those most likely to receive the free chlorine subsidy. Under the status quo, CHWs visit those whose source is unprotected less regularly (column 1, row 1). While the home delivery program increases effort by CHWs (they are 12.7 percentage points more likely to make a visit if given bottles to distribute), it does not close the gap for households with an unprotected source (the coefficient for the interaction between Home

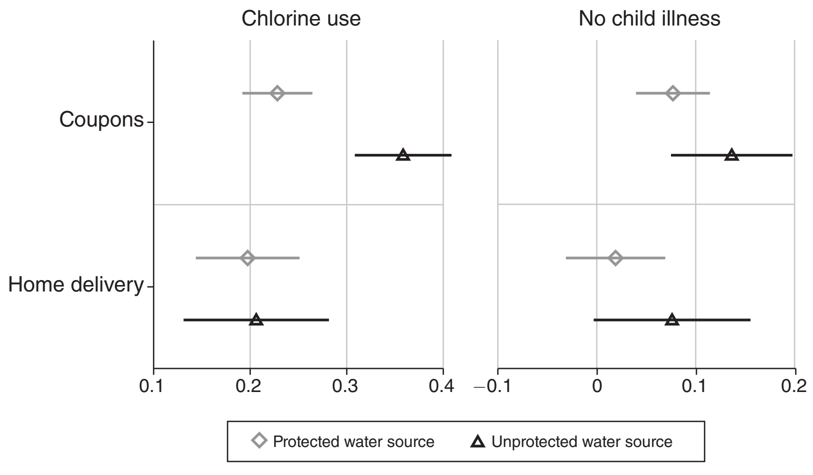

Figure 6. Heterogeneity in Effect Sizes by Whether Water Source is Protected

Notes: Estimates are from separate regressions that subset on households based on whether they had a protected water source. Regressions include indicators for coupon and home delivery assignment. Child illness was measured within the previous month for children under ten years old. The illness effect sizes are the absolute value of any illness coefficients (positive means less illness). Protected sources include piped water, a public tap, a borehole with handpump, a protected well, and protected springs. Points represent the effect size relative to the control group and error bars are 95 percent confidence intervals. Standard errors are clustered at the CHW level in Neno and at the household level in Mwanza. For coupons, differences in effect sizes are significant for chlorine use (tested using interaction terms, $p \leq 0.001$) but not child illness ($p = 0.110$). Differences in effect sizes are not significant for home delivery ($p = 0.785$ for chlorine use and $p = 0.415$ for child illness).

Delivery and Unprotected Source in column 1, row 2 is negative and insignificant). As a result, households in the home delivery group with an unprotected source are not more likely to receive free water treatment and possibly less likely (column 2, row 2; we cannot reject fairly large negative effects, with a coefficient at −9.8 percentage points, $p = 0.197$). In contrast, self-targeting under the coupon program works well: households with an unprotected source redeem 19 percentage points (three and a half) more coupons, on average, than those with a protected source (column 3). Note that average distance to the shop where the coupons could be redeemed was significantly greater for households with an unprotected source: 2.3 kilometers versus 1.9 kilometers, a 400-meter gap ($p < 0.001$).

Why do CHWs fail to appropriately target households who have the greatest need for chlorine? This may be because such households are further away; hence, the costs associated with doing a home visit are higher. Indeed, households whose distance to the trading center is greater than the median in the sample were significantly less likely to receive regular CHW visits, and the home delivery intervention did not change this (Table 4, row 11, column 4). Correspondingly, distance reduced the impact of the home delivery intervention on receipt of free WaterGuard (column 5). Strikingly, it did so possibly more than in the coupon group (the p-value of the difference between the two interaction terms is 0.17). In other words, distance impedes access to the subsidy at least as much when the distance cost is paid by the delivery agent rather than the end user.

Table 4—Targeting on Baseline Needs and Preferences

Variables	CHW visit last month (1)	Free WG last month (2)	Share coupons redeemed (3)	CHW visit last month (4)	Free WG last month (5)
Unprotected water source	−0.088	0.025	18.93		
	(0.037)	(0.024)	(2.45)		
Home delivery × unprotected source	−0.041	−0.098			
	(0.061)	(0.076)			
Rationing × unprotected source	−0.054	0.096			
	(0.109)	(0.094)			
Coupon × unprotected source	0.043	0.064			
	(0.044)	(0.047)			
Home delivery	0.127	0.525		0.128	0.571
	(0.032)	(0.040)		(0.040)	(0.047)
Rationing	−0.033	−0.169		−0.118	−0.267
	(0.049)	(0.053)		(0.057)	(0.062)
Coupon	0.019	0.324		0.017	0.365
	(0.024)	(0.025)		(0.025)	(0.028)
Thinks WG tastes bad (at baseline)	0.011	−0.052	−7.30	0.007	−0.045
	(0.018)	(0.020)	(2.22)	(0.017)	(0.020)
Mwanza			8.59		
			(2.63)		
Far from trading center (TC)			−12.12	−0.059	0.023
			(2.25)	(0.032)	(0.022)
Home delivery × far from TC				−0.011	−0.156
				(0.054)	(0.066)
Rationing × far from TC				0.163	0.265
				(0.089)	(0.090)
Coupon × far from TC				0.034	−0.054
				(0.042)	(0.046)
Constant	0.576	0.069	54.33	0.575	0.066
	(0.025)	(0.022)	(1.91)	(0.027)	(0.023)
Observations	3,138	1,906	872	3,136	1,905
Month fixed effects	Yes	Yes	No	Yes	Yes
Control group mean	0.583	0.0675		0.583	0.0675
Mean of dependent variable			53.36		

Notes: Protected water source includes piped water, a public tap, a borehole with hand pump, a protected well, and protected springs. Columns 1, 2, 4, and 5 are restricted to Neno district and include controls for the presence of an ART patient, TB patient, and the age of the youngest child as well as month fixed effects. Column 3 is restricted to the coupon sample and is at the household level. Free WaterGuard in the last four weeks (columns 2 and 5) is self-reported and available for follow-up 2 only. "Far from trading center" means that distance is greater than the median (1.85km). "Thinks WG tastes bad" indicates whether the household agreed or strongly agreed at baseline with the statement "WaterGuard makes water taste bad." Standard errors clustered at the cluster level are in parentheses. A cluster is a CHW catchment area in Neno and a household in Mwanza.

Targeting on Usage Cost: Heterogeneity by Baseline Taste Concerns.—Another dimension of heterogeneity is in households' distaste for chlorinated water. The minimum amount of chlorine necessary to purify water may not affect the taste of water, but dosing correctly can be difficult for nonstandard water containers, so as households err on the side of caution, the actual quantities used to purify tend to give the water a chlorinated taste. At baseline, while 51 percent of respondents had ever used WaterGuard, 46 percent of respondents mentioned that they think it gives water a bad taste (Table 1), suggesting that this may be a serious barrier to adoption.

Table 4, column 3 shows that households with such beliefs at baseline redeemed 7.3 percentage points (one and one-third) fewer coupons, suggesting self-targeting based on taste concerns. Figure A5 in the online Appendix shows that the effects of the coupon scheme on chlorine use were smaller for households who believe WaterGuard alters the taste.

Targeting through Ex Post Sharing.—To the extent that subsidy recipients who do not care for chlorine give the chlorine away to other households, the overall targeting performance of the subsidy programs may be higher than what we observe when focusing on beneficiary households only. Coupon households are 15.6 percentage points more likely to report having ever given WaterGuard to a neighbor than control households (Table 2, pooled coupon effect). Because we provided coupons to less than 10 percent of households in a village, however, these spillovers are diffuse—chlorine usage in the control group was still very low at only 4 percent on average, not higher than at baseline. Home delivery households were not more likely to report giving WaterGuard away; this is possibly because their neighbors were already receiving WaterGuard from the CHW.[18]

Targeting by CHWs: Results from the Rationing Experiment.—Can CHWs be effective targeting agents when specifically asked to do so? Starting in February 2019, the rationing intervention randomly varied the amount of WaterGuard bottles CHWs had to distribute. This allows us to test whether CHWs can target free chlorine to households that use it when they are in charge of identifying beneficiaries.

Table 5 shows how the rationing arms compare to the 100 percent coverage arm on whether the households received a bottle in the last four weeks, positive chlorine tests, and any child illness. Columns 1–3 analyze each rationing arm separately and columns 4–6 pool them. This analysis only includes the 105 CHWs that were assigned to home delivery and fewer survey waves (261 households). Therefore, we have less power to detect effects than in our previous analyses, and estimates should be interpreted with caution.

We find modest evidence that rationing the supply of WaterGuard bottles was more efficient than 100 percent coverage in terms of targeting users. Despite many fewer bottles being given to CHWs and fewer households receiving a free bottle of WaterGuard (columns 1 and 4), rationing had small and insignificant effects on objectively measured WaterGuard usage among households (column 2, $p = 0.941$ for the 60 percent coverage arm and $p = 0.493$ for the 40 percent one, and column 5, $p = 0.633$). However, confidence intervals are wide and we cannot rule out important reductions in chlorine use. Figure 5 plots the share of households that received WaterGuard and the share with positive chlorine tests by study month. By the last study month, the share of households that receive WaterGuard and the share that have treated water nearly converge in the rationing arms, whereas a large gap remains under 100 percent coverage. Tentatively, these results suggest that CHWs

[18] Differential spillovers between the coupon and home delivery arms will not bias the comparison between these arms because they are compared to the same set of control households (study arm 3 in Figure 1).

Table 5—Home Delivery: Rationing results

Variables	Free WaterGuard last 4 weeks (1)	Positive chlorine test (2)	Any child illness (3)	Free WaterGuard last 4 weeks (4)	Positive chlorine test (5)	Any child illness (6)
Rationing 60 percent	−0.144	−0.006	0.036			
	(0.089)	(0.077)	(0.059)			
Rationing 40 percent	−0.202	−0.051	0.036			
	(0.078)	(0.074)	(0.053)			
Rationing pooled				−0.178	−0.032	0.036
				(0.074)	(0.066)	(0.050)
Observations	261	261	468	261	261	468
Controls	No	No	No	No	No	No
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of CHWs	105	105	105	105	105	105
Nonrationing group mean	0.609	0.178	0.349	0.609	0.178	0.349

Notes: Data are from follow-up surveys. The sample is restricted to Neno households sampled for the home delivery treatment and surveyed after the introduction of rationing (February 1, 2019). Columns 4–6 pool 60 percent and 40 percent rationing levels. Free WaterGuard in the last four weeks (columns 1 and 4) is self-reported, and chlorine tests (columns 2 and 5) were measured using the household's water supply. Child illness analysis is at the child level (columns 3 and 6). Child illness was measured within the previous month for children under ten years old and controls for the child's age. Standard errors clustered at the cluster level are in parentheses. A cluster is a CHW catchment area in Neno and a household in Mwanza.

could have private information about households' preferences for WaterGuard that allows them to target bottles to households that will use them.

The estimates of the health effects under rationing are noisy due to the reduced sample size (fewer households over fewer months) but suggest that targeting by CHWs, while it is based on usage, may not be based on underlying health needs: the health effects under rationing cannot be distinguished from zero (see Figure A3 in the online Appendix and Table 2). Again, confidence intervals are wide, and we cannot rule out important increases in child illness under rationing.

Overall, it seems clear from our results that absent a monitoring or incentive system, targeting through CHWs performs worse than self-targeting through coupon redemption. One prima facie puzzling result is that the self-targeting observed in the coupon scheme cannot be reproduced through the home delivery by CHW scheme. Why do households who would go through the hassle of redeeming their coupons not go through the hassle of asking their CHW to visit them and give them chlorine? Why do households who would not bother to redeem their coupon still accept the free bottle from the CHW? The most likely explanation for the latter is that it is difficult to refuse a free health good from a health worker, likely due to social desirability bias. Concerning the former, CHWs may not have informed households that they were entitled to a free bottle every month, reducing the pressure on themselves to make monthly visits, especially to faraway households. Once they had to ration, CHWs may have asked households directly, "Will you use the chlorine? I need to know because I do not have enough for everyone." This could explain why some targeting of usage was possible under rationing.

D. *Why Is Chlorine Promotion by CHWs Not Making a Difference?*

CHWs employed by PIH have been working in Neno to support the primary care system for over 12 years and are a respected part of the health system in the district. In 2014–2015 CHWs were trained and deployed to identify pregnant women and escort them to prenatal care visits, and a synthetic control method study estimated that this led to an 18 percent increase in enrollment for prenatal care (Kachimanga et al. 2020). Based on this, PIH expected that WASH advice from CHWs would be followed, especially if it came with the hand delivery of a subsidized product. Why, then, did the WASH training not improve household usage of chlorine even when combined with home delivery?

A first possible explanation is that CHWs have a large and growing amount of responsibilities and might not have the time to implement the intervention as designed. This could have led CHWs to forgo visiting some households. Moreover, during household visits, CHWs are tasked with a long checklist of items to complete, which could crowd out chlorine promotion during the visit. We test for these potential explanations by estimating equation (1) to predict the probability of receiving a CHW visit in the previous four weeks and the probability of a CHW talking about chlorine during their last visit (Table 6, columns 1 and 2). Only 58 percent of households received a home visit in the previous four weeks in the control group (the figure increases to 80 percent for any CHW visit in the last two months). There was no significant impact of the WASH intervention (column 1). Interestingly, the home delivery intervention increased CHW effort: home delivery led to a 12.6 percentage point (22 percent) increase in households reporting a home visit from a CHW. This result suggests that the ability to deliver free goods may motivate CHWs to visit households more regularly than they would otherwise. This is consistent with the finding from Wagner et al. (2020) in Uganda, where CHWs asked to deliver free oral rehydration salts (ORS) kits to households did more home visits than when they could sell the treatment kits door to door and keep the revenue. Nevertheless, in our setting compliance with home delivery to all households was far from perfect among CHWs, with only 59 percent of households in the home delivery arm reporting receiving a free bottle of WaterGuard in the past month absent any rationing (Table 4, column 2). In contrast, all households in the coupon arm received coupons. Combined with the result that chlorine usage is higher in the coupon arm, this means that some households that would have used chlorine had they been in the coupon arm did not get a delivery from their CHW.

Table 6, column 2 shows that, conditional on receiving a visit, about a third of control households receive information about chlorine from their CHW, and the increase in exposure to chlorine promotion was much lower than expected in the WASH arm. Assignment to WASH training increased the likelihood of the CHW talking about chlorine during their previous visit by 11.6 percentage points from 34.8 percent in the control arm. Interestingly, this effect is no greater than the effect of coupons alone: households that received coupons were 10.7 percentage points more likely to have their nontrained CHW discuss WaterGuard with them during their last visit. It could be that since households in the coupon arm are more likely

Table 6—Mechanisms of CHW Interventions

	CHW effort		Beliefs about cleanliness of water		Beliefs about WG effectiveness		Knowledge about WG	
Variables	CHW visit last month (1)	CHW talked about chlorine during last visit (2)	Thinks water source is "always" safe (3)	Boiled water (4)	Thinks WG protects against health risks (5)	Thinks WG completely effective against diarrhea (6)	Reported WG as way to make water clean without prompt (7)	Knowledge of correct use (8)
Coupon	0.033	0.107	0.043	−0.040	−0.001	0.015	0.081	0.343
	(0.030)	(0.034)	(0.031)	(0.016)	(0.023)	(0.032)	(0.031)	(0.031)
WASH	−0.001	0.116	0.011	0.012	0.004	−0.040	−0.010	0.049
	(0.034)	(0.033)	(0.042)	(0.018)	(0.020)	(0.030)	(0.034)	(0.033)
Coupon × WASH	−0.001	0.017	−0.007	−0.018	0.018	0.072	0.030	−0.118
	(0.041)	(0.044)	(0.040)	(0.022)	(0.028)	(0.042)	(0.041)	(0.042)
Home delivery	0.126	0.416	0.020	−0.043	0.022	0.038	0.093	0.174
	(0.035)	(0.035)	(0.043)	(0.016)	(0.022)	(0.035)	(0.035)	(0.038)
Home delivery × rationing	−0.050	−0.040	0.003	−0.001	−0.019	0.000	−0.045	0.070
	(0.047)	(0.044)	(0.046)	(0.018)	(0.032)	(0.048)	(0.046)	(0.042)
Observations	3,138	2,846	3,129	3,134	3,138	3,138	3,138	3,138
Controls	No	No	No	No	No	No	No	No
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of clusters (CHWs)	435	435	435	435	435	435	435	435
Neno control group mean	0.583	0.348	0.574	0.0933	0.876	0.395	0.576	0.511

Notes: The table shows the Neno sample only, since there is no CHW program in Mwanza. Data are from follow-up surveys conducted on a rolling basis between May 2018 and July 2019. The order in which households were surveyed was randomized, with stratification at the CHW level. Households were sampled to be surveyed twice, with an average gap of 6.4 months between the two follow-ups. All outcomes were self-reported. Standard errors clustered at the cluster level are in parentheses. A cluster is a CHW catchment area.

to have chlorine on hand they bring up the topic themselves, or that the CHW reacts to the coupons on the wall calendar. Ultimately, the WASH intervention did not lead to a significant change in attitudes about the need to treat water with chlorine (Table 6, column 3), beliefs about the effectiveness of WaterGuard at preventing illness (columns 5 and 6), or knowledge of correct use (column 8). We also do not find evidence that the WASH intervention encouraged households to use boiling as an alternative strategy to purify water (column 4).

E. *Potential Threats to Internal Validity*

Attrition.—About 6 percent of households assessed at baseline received no follow-up visits and 22 percent received only one follow-up visit. (Attrition in round 2 was due in part to torrential floods that cut out access to entire sections of the sample area for a few months.) Table A5 in the online Appendix regresses the number of follow-up visits per household on their treatment assignment. This table shows that the probability of attrition was similar across study arms. However, it is possible that households who missed a follow-up visit had different characteristics across study arms. Table A6 in the online Appendix shows baseline characteristics of attriters (the 608 households that had less than two follow-up visits). Differences among attriters between arms were small and mostly insignificant. Attriters in the home delivery arm had a lower chlorine use rate at baseline, which implies that, if anything, our estimates for this arm are biased upward (and therefore our finding that coupons outperform home delivery is all the more remarkable). Attriters in the coupon arm had a lower likelihood of child illness at baseline, which implies, if anything, our child health results would be biased toward zero. However, it is still possible attriters were different on unobservable characteristics that are correlated with our main outcomes. To bound the bias that could result from this, we include attriters in our main regression models and assume that attriters from coupon- and home delivery–assigned households had bad outcomes and attriters from control households had good outcomes. Specifically, observations in the control arm with missing surveys were assumed to have a chlorinated water rate twice as high as that of the control group with surveys (imputed treatment rate of 10 percent) but only half the infection rate (imputed infection rate of 28 percent). Observations from the treatment arms (coupon and home helivery) were all assumed to have no chlorine in their water (imputed treatment rate of 0 percent) and the average infection rate at baseline (imputed infection rate of 68 percent). We exclude the WASH treatment from this exercise for simplicity. Table A7 in the online Appendix shows that under these rather extreme assumptions chlorine use is still 19 percentage points higher in the coupon arm, but we cannot reject the null for effects on child illness.

Reporting Bias for Health Outcomes.—While our measure of chlorine usage is objectively measured through water tests, our measures for health outcomes are reported by caregivers. Since study participants could obviously not be blinded to their treatment status, one could be concerned that the impacts we observe are due to experimenter demand effects. Wolf et al. (2018) suggest that nonblinding could lead researchers to overestimate diarrhea reductions. In this section, we discuss various pieces of evidence to help allay such concerns. First, we note that the health effects for the coupons do not appear immediately and persist over time (as shown in Figure 4 and Figure A2 in the online Appendix), while experimenter demand effects are typically immediate and short lived. Second, we observe spillover effects on respiratory health (see Table A3, column 4 in the online Appendix). Specifically, the incidence of coughing—very high in our sample due to indoor air pollution (46.4 percent of children under ten experienced coughing in the previous four weeks in the control group)—reduces by 17 percent. Experimenter demand effects are less likely to carry through to respiratory health, an illness clearly not targeted by the water treatment subsidy program. The decrease in coughing is consistent with the observed decrease in water boiling (Table 6, column 4), as well as the fact that children spend more time outdoors when they are not sick with diarrhea or fever. An alternative explanation would be that the reduction in coughing is evidence of a failed placebo test; i.e., caretakers overstated health improvements across the board, even for illnesses not impacted by the intervention. We view this as unlikely. Third, for diarrhea we see larger health impacts in Mwanza than Neno (Table A3, column 1), consistent with greater coupon usage in Mwanza, but there is no reason why experimenter demand effects would be greater in Mwanza. Finally, the treatment that may have been the most likely to generate social desirability bias is the WASH treatment, since it explicitly encouraged households to use chlorine. Yet households in the WASH treatment did not self-report higher chlorine use or fewer illnesses (Table 2, columns 2, 4, and 5). Overall, while we acknowledge that having objective measures of child health would have been a great addition to this study had it been possible, we take our estimated effects on caregiver-reported health outcomes as strongly suggestive.

V. Cost Effectiveness

To estimate and compare cost effectiveness across interventions, we use administrative data on number of bottles distributed and distribution costs combined with our treatment effect estimates. We restrict our analysis to the last 12 months of the interventions (September 2018 onward) to get closer to steady-state cost and effectiveness estimates. We estimate home delivery cost effectiveness for the version with 100 percent coverage only since the rationed versions did not yield visible health impacts. We calculate the cost per additional 30 household days with treated drinking water and the cost per child illness averted. The two analyses call for different time horizons because households with treated water reflect one point in time, whereas child illnesses averted add up over time. Thus, we use monthly costs and effectiveness (averaged over all 12 months) when estimating cost per additional 30 household days with treated drinking water, and we use total costs and effectiveness for the full 12 months when estimating the cost per child illness averted. We use household days because our chlorine tests only verify whether a household had chlorine tests on a given day. Thus, we assume that each additional positive test represents 30 additional household days with treated water in our one-month time horizon. We consider a child as having illness if their caretaker report that they had diarrhea, fever, or vomiting in the previous four weeks. Treatment effects for illnesses averted are from the specification in equation (1), restricting to data collected after September 1, 2018.

Cost estimates are based on the number of WaterGuard bottles distributed and the wholesale price ($0.45 per bottle). We use coupon redemption data to identify the number of bottles redeemed in the coupon arm and administrative records from PIH to identify the number of bottles handed out to CHWs in the home delivery arm. We also include the monthly cost of distribution to shops in the coupon arm and the monthly cost of distribution to CHWs in the home delivery arm.[19] The monthly distribution costs to shops are likely an upper bound because a scaled-up version could deliver to shops less frequently (e.g., every three months). CHWs, however, would have difficulty transporting and storing more than one month's supply.

Table 7, panel A shows that coupons were far more cost effective than home delivery, with 100 percent coverage in terms of increasing chlorine use. Across the 872 households that received coupons an average of 419 coupons were redeemed per month, costing about $258 ($0.30 per household). This led to 6,804 additional

[19] It cost the program roughly $25 per shop to make the deliveries each month, and each shop covers about 313 households ($0.08 per household). Distribution to CHWs is more centralized because CHWs meet monthly at the health center and, hence, it is cheaper (at $0.02 per household per month).

TABLE 7—COST-EFFECTIVENESS

	Observations	Bottles distributed	Cost per HH	Effect	Additional HH days treated	Cost per 30 HH days treated	95% CI
Panel A. Cost per additional household with treated water (one-month time horizon)							
Coupons	872	419	$0.30	0.26	6,804	$1.13	$1.00–$1.31
Home delivery	2,965	2,965	$0.47	0.24	21,410	$1.95	$1.50–$2.60
	Observations	Bottles distributed	Cost Per HH	Effect	Cases averted	Cost per illness averted	95% CI
Panel B. Cost per child illness averted (12 month time horizon)							
Coupons	872	5,027	$3.55	−0.11	1,181	$2.62	$1.96–$3.95
Home delivery	2,965	35,578	$5.64	−0.04	1,575	$10.62	$3.88–N/A
	Observations	Bottles distributed	Cost per HH	Effect	Cases averted	Cost per illness averted	95% CI
Panel C. Cost per diarrhea case averted (12 month time horizon)							
Coupons	872	5,027	$3.55	−0.05	477	$6.50	$4.29–$13.49
Home delivery	2,965	35,578	$5.64	−0.02	727	$22.99	$7.75–N/A

Notes: We use only the last 12 months of data (September 2018 onward) to reflect steady-state costs and effectiveness. Cost effectiveness (far-right columns) is relative to the control group. 95 percent confidence interval estimates use the upper and lower ends of the 95 percent confidence interval of the effect size. "N/A" means that the 95 percent confidence interval of the effect size goes through zero. We convert effect sizes to household days with treated water because we only test a water source on one day. Values in panel A are for one month (averaged across 12 months). The number of households for the coupon arm is all households that received coupons. The number of households in the home delivery arms is the total households assigned to home delivery including those that were not surveyed. "Bottles distributed" is the number of coupons redeemed in the coupon arm and number of bottles given to CHWs in home delivery arms. Bottles cost $0.45. Distribution of bottles costs $0.08 in the coupon arm and $0.02 in the home delivery arm. Effects for coupons and home delivery are from the same regression used in Table 2 (Table A3 in the online Appendix for panel C) but restricting to the last 12 months.

household days with treated drinking water. This gives a cost effectiveness ratio (CER) of $1.13 per 30 household days with treated water. Panel B shows that the coupon intervention cost $2.62 to avert one child illness. The home delivery was also relatively cost effective in terms of averting illness, but less so than coupons. It cost nearly twice as much per household as the coupon intervention and is less cost effective at $10.62 per illness averted. Panel C shows that coupons cost $6.50 per diarrhea case averted, versus $22.99 for home delivery.

VI. Conclusion

Governments of low- and middle-income countries spend relatively large sums on water and sanitation subsidies: between 1.5 and 2 percent of GDP, according to a recent World Bank report (Andres et al. 2019). The incidence of this spending has recently been questioned, however. Careful analysis of the spending in ten countries suggests that 56 percent of subsidies end up in the pockets of the richest 20 percent of households while only 6 percent of subsidies find their way to the poorest 20 percent (Andres et al. 2019). The main cause of such poor targeting is that existing subsidies typically target piped water systems, which are largely nonexistent in rural areas where the majority of the poor live. Even when poor households are in areas with access, most of them are unable to afford a connection to the network (Devoto et al. 2012). Identifying targeting mechanisms that can direct resources to the poor while excluding the more well off is a key priority for governments.

This paper shows that one such possible mechanism—free water treatment solution made available monthly to households with young children, conditional on their coming to pick it up from a central location—can effectively target subsidies to poor households whose drinking water is unclean. Such a program is extremely cost effective and substantially reduces the incidence of waterborne diseases in low-density, unconnected rural areas with no decrease in impact over an 18-month period. It is more cost effective than relying on existing networks of CHWs. CHWs are worse at targeting subsidies to households that need them the most than self-targeting. Moreover, their ability to nudge more households to treat water appears very limited, and asking them to do so may come at the expense of other tasks that they have to do.

As far as we know, no country to date routinely provides free water treatment products to parents of young children through rural clinics. This situation reminds us of the situation regarding malaria prevention in the early 2000s (Cohen and Dupas 2010). Since then, free distribution of insecticide-treated bednets to pregnant women and young children has been adopted by over 30 countries, and malaria incidence has considerably reduced as a result (World Health Organization 2019). Our results suggest that adopting a similar policy for point-of-use water treatment would bring countries closer to achieving the goal of universal access to safe drinking water set forth by the 2030 Sustainable Development Agenda.

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