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Short-term subsidies and seller type: A health products experiment in Uganda☆

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ABSTRACT

The way in which a product is distributed can have lasting effects on demand by influencing learning, anchoring price expectations, and shaping perceptions of product value. While these issues apply broadly, they are particularly important for health products in poor countries, where short-term subsidies are common, similar products are often available through both non-profit and for-profit organizations, and expanding access is an important public health goal. We implemented a field experiment in northern Uganda in which three curative health products were distributed door-to-door either free or for sale and by either an NGO or for-profit company. For all three products, subsequent purchase rates were lower after a free distribution. While we see no difference in subsequent purchase rates based on seller type, we find that contemporaneous demand for a newly introduced product is higher when the seller identifies as a not-for-profit organization.

1. Introduction

An extensive theoretical and empirical literature in marketing, psychology, and economics investigates how prices may affect not only contemporaneous but subsequent demand. While the effect of price histories has been studied in a wide range of settings, from home prices in the United States to Belgian chocolates, it plays a central role in policy debates about the distribution of health products in low-income countries.[1]

One of the arguments against the free distribution of health products is that price subsidies may actually discourage future purchase. Give someone an insecticide-treated bed net for free, one story goes, and he will neither use it properly nor buy another one in the future. Offer subsidized water treatment today, and households will not be willing to

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[1] In psychology, there is a long history of studying the effect of reference points in absolute judgments (e.g., Sherif et al., 1958; Doob et al., 1969). A range of studies have demonstrated anchoring effects in estimation tasks (e.g., Tversky and Kahneman, 1974; Jacowitz and Kahneman, 1995; Chapman and Johnson, 1999; Epley and Gilovich, 2001). The role of such anchors in the formulation of individuals' values has since received considerable attention in classroom and lab experiments as well as scanner data (Ariely et al., 2003; Mazar et al., 2014; Winer, 1986; Kalwani and Yim, 1992; Raghubir and Corfman, 1999; Rao and Monroe, 1989; Mayhew and Winer, 1992; Dekimpe et al., 1998; Kalyanaram and Little, 1994), although the robustness of such non-budget-constraint effects of prices on demand has recently been called into question (Fudenberg et al., 2012; Maniadis et al., 2014). Nunes and Boatwright (2004) provide evidence for the role of incidental prices in a range of settings, and Simonsohn and Loewenstein (2006) demonstrate behavior consistent with price anchors in the apartment rental decisions of individuals moving to new cities.

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pay for it tomorrow. Such effects, if true, can prevent the development of sustainable, functioning markets for health goods, the kind we take for granted in most developed countries.

Cohen and Dupas (2010) refutes these claims for insecticide-treated bed nets. Free bed nets given to pregnant women in Kenya are used as intended. Moreover, free distribution of bed nets actually encourages future purchases (Dupas, 2014). Building an evidence base for policy-making requires understanding the extent to which these results generalize.

Dupas (2014) illuminates a key input in the policy debate over short-term, free distributions: the tension between learning and price anchors. Seen through this lens, it is easy to imagine the effect of free distributions on future demand going in either direction. On one hand, distributing a health product, or any other experience good, gives the recipient the chance to learn something about the good. Does it work? Does she like it? If the experience proves better than she expected, all else equal, she will be more likely to buy the product in the future than had she not received the free distribution.

But all else is not necessarily equal. Our past purchase experiences shape future demand. As described in Kőszegi and Rabin (2006), past prices can anchor our perceptions of value. We hate paying more than we did the last time, we form expectations about future prices and a product's intrinsic value based on prices we have observed in the past, and we do not want to feel foolish when something is given away tomorrow after we paid good money for it today.

Most of the recent work on pricing for health products in low-income countries focuses on distributions by NGOs or governments, but for-profit firms often use free samples or steep introductory discounts to encourage adoption of new products (Schultz et al., 1998; Seetharaman, 2004; Bawa and Shoemaker, 2004; Villas-Boas, 2004). This raises the question: does the seller's identity matter? For-profit firms do not give consumable products away for free in perpetuity. On the other hand, an NGO might make regular, free distributions of health products. Do consumers interpret price signals differently depending on the source? We hypothesized that free distribution by a for-profit firm would shift reference points or affect price expectations by less than a free distribution by an NGO, from whom individuals could reasonably expect some chance of the free distributions persisting. Moreover, individuals may impute different motives for not-for-profits and for-profits, even when their actions are otherwise identical (Aaker et al., 2010).

To study this set of questions, we implemented a field experiment with 120 villages in northern Uganda in which three curative health products were distributed door-to-door either free or for sale and either by an NGO or a for-profit company (Wave 1). Our key outcome measure is the purchase rate for these products during a subsequent (Wave 2) door-to-door distribution ten weeks later by an unrelated, for-profit firm. As detailed in Section 2, we attempted to adhere to natural marketing processes, with the hope that observed reactions from respondents would be characteristic of what would happen outside a research setting, e.g., we wanted to avoid Hawthorne, John Henry, and measurement effects.

We chose products—Panadol, Elyzole, and Zinkid—for variation in the scope for learning, not for policy relevance. Panadol, a branded version of a pain reliever widely known to consumers, provides the most direct test of price anchoring. It is relatively free from potentially conflating effects of a free distribution on subsequent demand: there are no positive externalities, little scope for learning, and small if any income effects. Thus the main mechanism through which current prices can affect future demand is negative anchoring effects.[2] Experience with Elyzole, a moderately well-known deworming drug, likely produces negative learning due to unpleasant side effects (Miguel and Kremer, 2007). In contrast, experience with Zinkid, an improved but largely unknown treatment for childhood diarrhea that was recently recommended by the World Health Organization at the time of our study, likely produces positive learning.

The three products are quite different from insecticide-treated bed nets, the main product for which this question has been studied. They are curative rather than preventive, consumable rather than durable, and unlikely to have meaningful income effects.[3] We have four notable findings.

First, we find suggestive evidence of price anchoring or other direct effects of prices. For all three products, observed purchase rates when we return to households ten weeks later are 5–12 percentage points lower after a free distribution. We discuss several alternative mechanisms that could explain the difference. Additional data allow us to rule out many of these, including the mechanical effect of having more of the product on hand if it had been previously distributed for free. Households' survey responses are also suggestive of price anchors: those who received free distribution are more likely to report that they do not want to purchase the product because they or someone in their community had received it for free in the past. We note, however, that when we conduct an "intention-to-treat-analysis" that considers all households that were not reached in treatment wave as not purchasing in the subsequent wave, the effect of the free distribution is only statistically significant when pooled across the three products.

An important potential alternative explanation for these results, described in Dupas and Miguel (2017), is that free distribution gave more households an opportunity to experience the good and could have led them to purchase from alternative sources rather than our Wave 2 distribution. We unfortunately do not have data from other potential distribution channels (e.g., drug shops and clinics) for the sample products. Making use of the data we do have, we note that when asked in a post-marketing survey, those in the free treatment who did not purchase in Wave 2 are more likely to report high prices or the presence of free distribution in the area as the reason for their decision. We also find similar treatment effects where the product (either the same brand or a chemically-identical alternative) was available from other sources in the village and where it was not. However, if the free treatment induced households to travel to sources outside the sample to obtain the products and these purchases were not reported in the post-marketing survey, we would expect the pattern of purchases that we observe in Wave 2 even in the absence of price anchors.

Second, our empirical evidence for a model where positive learning can offset negative demand effects from price anchoring is inconclusive. In percentage terms, relative to the effects for Panadol, a branded version of a pain reliever widely known to consumers, the reduction in the share of households purchasing after the free distribution is larger for Elyzole, the product with scope for negative learning. In contrast, the reduction is smaller for Zinkid, where there was scope for positive learning and we would expect learning and price anchors to work in opposite directions. However, none of the differences across products in the effect of prior free distributions is statistically significant at conventional levels. Moreover, only two of the three pairwise comparisons conform to the theoretical predictions when reductions are specified in percent terms rather than percentage points.

[2] Panadol is not unique in its ability to isolate potential anchoring effects. One could use any well-known product with potential for repeat purchase and free from confounding effects (e.g., croissants). Panadol has the advantage of sharing characteristics common to the class of health goods, such as being distributed through drug shops and health centers.

[3] The use of ITNs reduces the incidence of malaria and may thereby increase households' income and, in turn, future demand for additional ITNs (see footnote 27 of Dupas, 2014, for more discussion). In our context, as discussed in Section 4, we do not believe any income effects would be substantial.

Third, contrary to our hypothesis, we find little evidence that the effect of free distributions on subsequent purchase rates depends on distributor type. However, distributor identity does matter for the contemporaneous sale of the relatively unknown product. Households are 14 percentage points (50 percent) more likely to purchase Zinkid from the non-profit than from the for-profit firm selling at the same price and providing the same product information. We find no difference for the more well-known products. The finding that NGOs are more effective at stimulating demand for unknown products has important policy implications but was not one of our ex ante hypotheses. Furthermore, this difference does not persist: there is no discernible difference in the subsequent purchase decisions from an unrelated, for-profit firm between those who were originally offered the product for sale by the NGO or for-profit marketers. We note that all distribution in Wave 2 was conducted by a for-profit organization.

Fourth, we find no evidence that the price anchoring effect of free distributions for one product spills over to the demand for other health products. There is no discernible effect of having received a product for free in the first wave on the purchase rates of Aquasafe, a new product offered only in Wave 2. However, we note that confidence intervals for the cross-product effects are large.

We emphasize that our aim is to contribute to understanding how pricing and distribution strategies affect future purchases. We are not attempting to conduct a cost-benefit analysis of one-time free distributions. Optimal policy, from a social planner's perspective, depends on a number of other potentially important factors such as income effects, externalities, and habit formation. It also depends on the welfare gains from one-time subsidies and the implied welfare losses from anchoring. We do not measure these. Even substantial anchoring effects may not matter from a public health standpoint if there are large welfare gains from massive increases in coverage in the short run.

As a final caveat, we note that our evidence, like other work on the impact of short-term subsidies or free distributions, does not speak to questions about permanently free distribution, a common issue for health goods such as immunizations or maternal care. But as noted in Dupas (2014), short-term, subsidized distributions are an important policy tool for products such as antimalarial bed nets, water-treatment kits and condoms. They are also common after shocks, such as conflict or natural disasters, and may characterize a wide variety of donor programs that last a single funding cycle.

2. Experimental design & data

2.1. Experimental design

Setting and sampling. We conducted our experiment in Gulu District in northern Uganda, an area with a large NGO presence and a history of free distributions.[4] We selected 120 villages for the study and from each of these villages randomly selected approximately 50 households from the household list kept by the village leaders.[5]

First wave of marketing. The first wave of marketing (Wave 1), conducted in October–November 2011, employed a two-level clustered randomization design, with randomization at both the village and individual level. First, villages were randomly assigned to one of four treatment groups in a two-by-two design.[6] The first treatment dimension was the price of the product, either free ("Free") or sold ("Sale"). The second dimension was the type of distributing organization: either a not-for-profit, non-governmental organization ("NGO") or a for-profit business ("For-Profit"). Thirty villages were assigned to each of the four treatment cells. Table 1 illustrates balance across our village treatment assignments.

Within each village, we randomly assigned households to be offered one of three products: Panadol (paracetamol, a painreliever), Elyzole (albendazole, a deworming medication), and a combination pack of Restors and Zinkid (oral rehydration salts, "ORS" and zinc supplements, the World Health Organization's recommended treatment for childhood diarrhea). For the Sale treatment group, we used the same price in each village. We aimed to set the price for the Sale group to approximate a market price (i.e., the perceived price plus a small add-on for the convenience of buying at one's home) and discourage resale.[7] We note, however, that the products we offered were available for sale in local drug shops in only 11%–36% of the study villages. In villages where the experimental product was not available, neither the shop nor market price is well defined.[8]

In order to maximize the likelihood that individuals perceived the various marketing and sales interactions as natural rather than experimental artifacts, we partnered with real Ugandan organizations involved in the provision of health products. For the NGO treatment, we worked with the Uganda Health Marketing Group ("UHMG"), a large Kampala-based NGO largely funded by USAID and focused on

[4] Gulu District was destabilized by an insurgency from 1987 until 2006. In the wake of the insurgency, the area received a large amount of NGO and government attention. Many NGOs were active in reconstruction and service provision, including providing free health care and health products. Relative to other regions in Uganda, Gulu District is likely at the upper end of the distribution in terms of prior exposure to free or heavily-subsidized distributions of health goods.

[5] Of these 120 villages, 72 were participating in a contemporaneous methodological study, for which villages were selected based on availability of certain administrative data. We selected the remaining 48 villages randomly from an administrative government list of villages in Gulu. We selected a total of 859 households based on the availability of institutional data, a requirement for the other study. We randomly selected the remaining households from village household lists.

[6] Village assignment to treatment was block randomized according to two variables. The first, price environment, included information about pricing and drug availability with three possible categories: (1) no drug outlets or none of our drugs; (2) no prices above the median or distributed for free; and (3) at least one price above the median. The second, remoteness, also had three categories: (1) easy to travel and close to health center; (2) difficult travel or far from health center; and (3) difficult travel and far from health center.

[7] Note we randomized the presence of free versus sale distribution, and NGO versus for-profit, at the village level, whereas the exact product received we randomized at the individual level. Thus our estimates are not biased from spillovers if the information flow is about the presence of an organization giving out free health products. The correct interpretation of our estimates includes both the direct effect of the treatment on a household's later behavior and a potential reinforcement effect (those around them also received the same treatment and, through conversations and sharing of information, reinforced any effect). We are estimating the combined effect, which is also the policy-relevant parameter given the typical practice of community-level distribution of health products. However, given that the exact product distributed was randomized at the household level, giving out a particular health product for free may have cross-product spillovers. While this could bias product-specific estimates, we do not see evidence of cross-product effects on purchase rates.

[8] Drug shop prices were collected in a pricing survey conducted prior to Wave 1. In earlier circulated versions of this paper, we referred to this as selling above the market price, but we have changed the language for three reasons. First, as noted, in the majority of villages the products we offered were not available for sale, so the market price is undefined in much of our sample. Second, as the figures in Appendix A demonstrate, drug outlet prices and price perceptions vary widely between and at times even within village. Third, our door-to-door distribution also builds in transport and convenience, which we would expect to influence households' perception of how competitive our prices are relative to other alternatives. The prices set in the first wave were as follows: Panadol: UGX 500 ($0.20) for a strip of ten tablets, Elyzole: UGX 1800 ($0.71) for three packs with two tablets each, Restors/Zinkid combination pack: UGX 2000 ($0.79) for one sachet of Restors and ten tablets of Zinkid.

Table 1
Baseline summary statistics means & standard deviations.

	Wave 1 Treatment Assignment				p-value of		
	Free (1)	Sale (2)	NGO (3)	For-Profit (4)	(1) vs (2) (5)	(3) vs (4) (6)	N (7)
Panel A: Wave 1 Respondents							
Individual Level							
Female	0.529	0.538	0.516	0.550	0.661	0.090	3879
	(0.499)	(0.499)	(0.500)	(0.498)			
Respondent age	42.984	42.781	43.214	42.545	0.873	0.595	1016[a]
	(14.579)	(14.762)	(14.511)	(14.813)			
Number of children under 16	4.475	4.339	4.378	4.457	0.403	0.628	1016[a]
	(2.417)	(2.287)	(2.368)	(2.356)			
Wealth proxy (cows owned)	1.058	0.874	0.893	1.070	0.271	0.297	1016[a]
	(2.601)	(2.137)	(2.365)	(2.459)			
Visited for usage check	0.080	0.090	0.085	0.085	0.060	0.968	3879
	(0.271)	(0.287)	(0.279)	(0.279)			
Found in Wave 2	0.747	0.742	0.765	0.723	0.856	0.107	3879
	(0.435)	(0.438)	(0.424)	(0.447)			
N	2016	1863	1948	1931			
Village Level							
Number of drug outlets	1.167	1.367	1.167	1.367	0.483	0.483	120
	(1.452)	(1.657)	(1.520)	(1.594)			
Panadol available[b]	0.383	0.333	0.333	0.383	0.572	0.572	120
	(0.490)	(0.475)	(0.475)	(0.490)			
Elyzole available[b]	0.233	0.250	0.217	0.267	0.833	0.526	120
	(0.427)	(0.437)	(0.415)	(0.446)			
Zinkid available[b]	0.117	0.100	0.100	0.117	0.771	0.771	120
	(0.324)	(0.303)	(0.303)	(0.324)			
Reports free distribution of any drug in last 3 mo.[c]	0.500	0.483	0.433	0.550	0.857	0.204	120
	(0.504)	(0.504)	(0.500)	(0.502)			
Reports free distribution of any deworming drug in last 3 mo.[c]	0.467	0.450	0.383	0.533	0.856	0.101	120
	(0.503)	(0.502)	(0.490)	(0.503)			
Reports free distribution of Elyzole in last 3 mo.[c]	0.050	0.050	0.033	0.067	1.000	0.406	120
	(0.220)	(0.220)	(0.181)	(0.252)			
N	60	60	60	60			
Panel B: Wave 2 Respondents							
Female	0.509	0.509	0.489	0.530	0.990	0.066	2887
	(0.500)	(0.500)	(0.500)	(0.499)			
Respondent age	43.507	42.979	43.685	42.783	0.699	0.516	779[a]
	(14.783)	(14.601)	(14.184)	(15.307)			
Number of children under 16	4.523	4.383	4.456	4.470	0.461	0.940	779[a]
	(2.461)	(2.346)	(2.413)	(2.413)			
Wealth proxy (cows owned)	1.097	0.896	1.000	1.023	0.314	0.911	779[a]
	(2.628)	(2.273)	(2.577)	(2.361)			
Visited for usage check	0.083	0.091	0.091	0.083	0.246	0.278	2887
	(0.276)	(0.288)	(0.287)	(0.276)			
N	1505	1382	1490	1397			

Standard deviations reported in parentheses.
[a] Variable available only for participants in accompanying methodological study.
[b] A product is "available" in a village if it is "mostly" or "always" available in at least one outlet/drugshop of the village.
[c] Reports of free distribution based on village chief's (LC1's) answer to the questions "Has [the product] been distributed for free in the past in this village?" and, if so, "When was the product last distributed for free in this village?", where "yes" is coded as 1 and "no" or "I do not know" are coded 0. P-values for individual regressions adjusted for clustering at the village level.

the distribution and promotion of health products. For the For-Profit distribution, we worked with Star Pharmaceuticals Ltd ("Star"), a large, Kampala-based company that imports, distributes and markets medicines and other products for sale throughout Uganda. Although the marketers were employed by UHMG and Star, we recruited, trained and monitored the marketers using the same protocols for both NGO and For-Profit distribution. Marketers wore branded t-shirts and displayed ID-cards from the relevant partner organization. The field marketers were all locally recruited, reducing communication barriers.

To mitigate potential liquidity constraints in the Sale treatment arm, several days beforehand the marketers distributed flyers throughout the village to announce the upcoming marketing visit. The aim was to reduce short-term liquidity constraints. In order to minimize potential for differential response rates, a similar flyer was distributed in the Free treatment arm announcing a distribution but not detailing whether

products would be free or sold.[9]

Throughout the study, we attempted to adhere to a natural marketing process. We wanted to avoid marketing procedures that deviated considerably from normal operating practices of NGOs or firms, so that the observed reactions of respondents would be more natural. In particular, we expected that returning on several consecutive days to a remote village to search for a specific respondent by name would be perceived as atypical behavior for an NGO or ostensibly profit-maximizing firm with the aim of sustainable product delivery. This in turn could generate experimenter effects and mask the true effect of price anchors. In practice, marketers made a single attempt to locate the head of household, as identified on the household list kept by the village leaders, or their spouse. While this methodological choice gives us greater confidence that our findings accurately reflect the effect of free distributions in non-experimental settings, it is not without costs. Out of the original 5667 households identified to be in the study, 3879 were found in the first wave of marketing. This is a lower level of entrance into the sample frame than often found in developing country studies. As discussed more fully below, this pattern reappears when looking at attrition in our second wave. In each attempt to locate specific respondents, we found approximately 75 percent of targeted individuals.

Marketers delivered sales pitches specific to each product, price treatment (Free or Sale) and entity (NGO or For-Profit). A pharmacist trained the marketers on how to explain usage and dosage guidelines and other questions about the products.[10] The script for the Sale treatment by an NGO explained the sale price by saying “pay a small amount to share” in the cost, whereas the Sale script for the for-profit said “at great prices”. The aim was to reflect marketing practices typical of the distributing entity. The phrase “at great prices” is common for marketing by for-profit entities such as UHMG and Star and would have been unusual for an NGO. This difference in wording may have generated differences in how the experimental prices were perceived. We discuss the implications of this in Section 3; details on the marketing scripts appear in Appendix B.1 and B.2.

In Wave 1, we offered one unit of the assigned product to households in the Free treatment arm and five units to those in the Sale treatment.[11] Prices were non-negotiable. Once this transaction had been completed, marketers administered a questionnaire to respondents in the Sale treatment group about why they decided to buy or not to buy and who might use the product.[12] In all cases, marketers had only one day to reach all respondents in each village. Marketing was not continued on a second day in order to reduce the possibility of spillovers of information or expectations across respondents.

The three products were chosen deliberately to capture a range of potential learning effects that could influence purchase decisions. Panadol, a branded version of the common painreliever paracetamol, serves as a benchmark. Nearly all respondents were likely to have been familiar with paracetamol, and we expect little scope for learning.[13] For Elyzole, a deworming medication, we expected that based on the relative salience of immediate side effects learning effects would tend to be negative despite potential for long-run benefits (Miguel and Kremer, 2004).[14] Zinkid was sold in combination with Restors, an oral-rehydration salt, following clinical recommendations (World Health Organization, 2005). While generic oral-rehydration is widely used and freely available from health centers, the importance of zinc supplements in combating diarrhea had only recently been established in the global health literature. As such, Zinkid represents a new brand and product for which we expect there to be scope for positive learning.[15] Table 2 presents descriptive results from the price perception and product awareness survey. While our three products were intended to encompass a range of potential learning about the effectiveness of types of health products, these distinctions abstract away from other potential sources of learning such as learning about brand or price. We consider the implications of alternative versions of learning in Section 4.[16]

Second wave of marketing. We conducted the second wave of marketing (Wave 2) on average ten weeks after Wave 1, in December of 2011.[17] The sole purpose of Wave 2 was to get an outcome measure of respondents’ willingness to pay for health goods. Marketers made a single attempt to locate the person interviewed in Wave 1 or, if that person was not available, their spouse. In order to avoid reputation effects from the first stage, we partnered with a different for-profit firm, Surgipharm Uganda Ltd (“Surgipharm”). Again, marketers were employed by the partner, but recruited, trained and monitored by the study team. In order to reduce association between the two waves, we changed the wording of all scripts without significantly affecting the content. In order to reduce the probability that respondents associated Wave 2 with Wave 1, we also

[9] The flyers differed slightly by referring to an “upcoming distribution” in the Free treatment villages and an upcoming “sale at a good price” in the sale villages. This may have induced different average rates for entering into the Wave 1 sample frame between the Sale and Free treatments (65.4% versus 70.5%, respectively). As shown in Panel A of Appendix Table A1, the differential entry rate is larger for households assigned Panadol. However, the flyers were distributed widely in the village and made no mention of the product that was going to be offered, which was randomized at the household level and only revealed to households once they entered the sample frame. We therefore conjecture that the imbalance for Panadol was due to bad luck and not selection on unobserved willingness-to-pay for the product. In hindsight, we believe this design decision was a mistake. For clarity of interpretation, the challenges this design poses for interpretation outweigh the potential gains from more natural marketing efforts, and we should have used the same flyers for all treatment arms.

[10] Marketers gave respondents information on dosage, storage and recommended use of the respective product both verbally and in writing in Acholi, the local language. This information was based on the instruction sheet of the drug and formulated in consultation with a pharmacist and board member of the Ugandan National Drug Authority.

[11] One unit corresponds to the smallest amount of each product that could be sold separately. For Panadol this was 10 tablets, for Elyzole this was 6 tablets, for Restors/Zinkid this was 1 sachet of Restors and 10 tablets of Zinkid, and for Aquasafe this was 8 tablets. We provided only one unit for free in order to reflect realistic distribution practices that would be observed outside of the experimental setting. Free distributions tend to be limited to a specific number of units. This is less likely for a distribution where products are sold, where we imposed a cap for logistical reasons. Only 2.5 percent of households in the Sale treatment purchased five units, suggesting that the cap on the quantity of units for sale was only rarely, if ever, binding.

[12] This survey was not conducted in the Free group in order to keep the interaction more natural.

[13] It is, however possible, that households that had not tried the branded version may have learned about differences between branded and generic paracetamol.

[14] While the immediate side effects of deworming medication are negative, the pill works and alleviates symptoms such as lethargy in individuals with symptomatic worm infections. Whether positive or negative learning dominates depends on both the relative size of the asymptomatic and symptomatic populations as well as the specific learning process.

[15] Zinc became part of the WHO guidelines for the treatment of diarrhea in 2006. In a study carried out with Zinkid users by our partner the Ugandan Health Marketing Group in 2012, 93 percent of zinc users believed that the product was an effective treatment for diarrhea, citing a quick end to diarrhea and fast recovery by the child as primary reasons for this belief.

[16] The three products also differ in terms of who would be the target user, which could affect the scope for learning. The type of Panadol used was aimed at adults only; children under 12 were not allowed to use it. Although Elyzole could be used by people of any age (except babies), parasitic infestations are most acute amongst children. Zinkid was a product specifically aimed at children, with a target age group of six months to five years.

[17] The minimum number of weeks between marketing waves was 6, the maximum 12 weeks, and the median is 10 weeks. Timing varied for logistical reasons, such as weather and holidays. We do not find any evidence that observed effects are correlated with differences in the number of days between waves.

Table 2
Summary statistics of respondents' familiarity with products.

Drug	Percent reporting they recognize a shown drug (1)	Percent of respondents who say they recognize the brand (2)	Percent giving a price estimate (any brand) (3)	Percent giving a price estimate (same brand) (4)	N (5)
Panadol	95.5%	10.2%	87.7%	9.4%	1282
Elyzole	64.4%	7.7%	58.4%	6.5%	1191
Zinkid/ORS	51.4%	5.9%	45.6%	4.5%	1275
Zinkid (lower & upper bound)[a]			16.3%-45.6%	1.3%-4.5%	1275
Aquasafe	71.4%	15.8%	65.7%	14.3%	2019

These data were collected during the Wave 1 by a marketer. Prior to marketing, we asked respondents about the two products that would not later be marketed to them. Column 1 reports answers to the question "Do you recognize this product that I have here? (Briefly describe what the product is, what it does)". Column 3 reports answers to the question, "How much would you expect to pay for this product [there]?". The available choices were: (a) Don't know, (b) It is free, (c) It is sold at this price: UGX___ (enter amount), (d) I am not certain, but I would estimate this price: UGX___.

[a] Zinkid and ORS were shown as bundle. In order to unbundle familiarity with the two products, we exploited whether respondents gave the price estimate in the unit of sachets or tablets. A respondent giving a price in the unit of sachets is taken to refer to ORS, since Zinkid is distributed in tablets. Since we cannot rule out that people knew both drugs but only reported their perceived price of ORS, this estimate is a lower bound. The upper bounds for familiarity levels with Zinkid are the joint levels presented for Zinkid/ORS.

assigned marketers to villages such that individual marketers did not visit the same village twice. While there may be time trends in the demand for health products, we do not believe there is any reason to expect seasonal fluctuations in demand to vary according to treatment status.[18]

We then investigate whether having received any product for free affects the purchase rate of *other* health products. We therefore assigned 25 percent of households to be offered a fourth product not offered in Wave 1, Aquasafe, a product designed for home water purification. Since no learning about specific product characteristics takes place across products, the cross-product test allows us to assess whether price anchoring will occur for broadly construed product categories, such as "health products".[19] In the second marketing wave, the only randomization was the household-level assignment of the product: 25 percent of households were marketed the new product, Aquasafe, and 75 percent the same product from Wave 1. Figure A1 summarizes the experimental design.

Attrition. As shown in Table 1, in Wave 2 we found 2887 of the 3879 individuals treated in Wave 1. This attrition rate of 25.6% resulted from a deliberate methodological decision to adhere to a natural marketing process, which precluded tracking specific households over multiple days. As shown in Appendix Table A1, attrition between waves is uncorrelated with individual characteristics (other than gender), including whether or not the subject received the product in Wave 1. See Panel B of Appendix Table A1 for details. Attrition is not correlated with assignment to the Free or Sale treatments. We found 74.7% and 74.2% of Wave 1 subjects, respectively (p-value: 0.856). Attrition is, however, marginally correlated with assignment to the NGO vs. for-profit treatments, where we found 76.5% vs. 72.3% of subjects, respectively (p-value: 0.110).

[18] Panadol is a painkiller that is used frequently to treat a variety of illnesses year-round, especially as it often means avoiding a visit to the health center. The Ugandan Ministry of Health suggests preventive deworming of children every three to six months, so we would expect participants to demand more deworming medication at the time of our second visit (Ministry of Health; Republic of Uganda, 2012). Childhood diarrhea is more common during the rainy season (Ahmed et al., 2008), therefore we might expect higher demand for Zinkid to treat diarrhea in Wave 1 when rains were more common.

[19] The mechanisms of any such cross-product effects could include beliefs about the general quality of products marketed in a particular way (i.e., door-to-door or by a for-profit entity) or categorical price judgments, whereby individuals judge utility of purchase by comparing the price of product to endpoints or distributions within the product category. For discussions of the latter mechanism, see, for example, Alba et al. (1999) and Mazar et al. (2014).

2.2. Data

Village and drug outlet data. Before Wave 1, we surveyed community leaders and drug outlets. We first asked the village chief about the number and type of drug outlets (including drug shops, clinics and hospitals) in each village, the distance (in time and kilometers) to the most popular and nearest facilities and any recent free distributions of health products. We then visited every drug outlet (including both private drug shops and local health clinics) in each village and asked about the price, availability and preferred brand for a list of common drugs. There were drug outlets in 64 of the 120 villages and, when a drug outlet was present, an average of 2.4 outlets per village. We used these data to determine the relevant "shop price" for the drugs we were offering, stratification, and to test for treatment effect heterogeneity.

Price perception survey. Immediately prior to offering the product, marketers administered a price perception survey to 50 percent of respondents in Wave 1. After introducing themselves, marketers showed respondents the two products *other than the one assigned to that individual* to avoid potential anchoring effects on the product about to be offered for sale or gift. After a brief description of the use of the product in general, respondents were asked about their familiarity with the product and brand. If they were familiar with the product, they were asked where they could purchase it and what price they would expect to pay. In Wave 1, we solicited price perceptions of the three goods distributed in the wave. In Wave 2, individuals were asked only about the new product, Aquasafe.

Post marketing survey. In order to understand the mechanisms influencing purchase decisions, we conducted a short survey (Appendix C) of all individuals who were offered products for sale (those assigned to the Sale group in the Wave 1 and all individuals in Wave 2). The survey was designed to mimic traditional marketing research in order to ensure that participants' experience was natural. The survey asked respondents in an unprompted way to explain why they did or did not purchase the product.

Observational usage data from physical observation of packaging. During Wave 1, all respondents who had received a product, whether for free or purchased, were informed that that they had also been entered into a lottery. If selected, they would need to present the product packaging (blister packs) in order to claim their prize. It was clearly stated that the prize did not depend on how much of the product was used, only on whether they presented the blister packs. Six to eight weeks after Wave 1 (two to four weeks before Wave 2), surveyors made unannounced visits to a sample of 329 households that received a product in Wave 1 and recorded how many tablets were remaining in

the blister packs.[20]

3. Results

In our setting, free health goods can affect demand through two different mechanisms: price anchoring and learning. We generated exogenous variation along three dimensions: whether a product was offered for free or for sale in Wave 1, whether it was offered by an NGO or a for-profit company in Wave 1, and the product a household was offered. The product price and the type of distributing organization were randomly assigned at the village level, while the product type was assigned at the household level. To estimate our treatment effects, we run the following basic specification for each product k

$$y_{ijkt} = \beta_{k0} + \beta_{k1} NGO_j + \beta_{k2} Free_{ij} + \beta_{k3} Free_j \times NGO_j + \gamma_k X_j + \varepsilon_{ijkt}, \quad (1)$$

where y is a measure of purchase rates (either a binary indicator of take-up or the total quantity purchased/received), i represents households, j represents villages, and t represents time (Wave 1 or Wave 2). *NGO* is a dummy variable that takes the value 1 if a household was approached by a representative of an NGO in Wave 1 and 0 if approached by a for-profit. The dummy variable *Free* takes the value 1 if a household was offered the product for free in Wave 1 and 0 otherwise. Coefficients of interest are the betas. β_1 captures the effect of an NGO being the distributing organization in Wave 1, β_2 the effect of being offered a product for free in Wave 1, and β_3 the effect of the interaction, i.e., being offered a free product by an NGO in Wave 1. X_j is the vector cross product of the two stratification variables: a price index and a remoteness index. ε_{ijkt} represents the idiosyncratic error, which we cluster at the village, the level of randomization.[21] We estimate equation (1) for the pooled sample and for each product individually.

To facilitate interpretation, we also estimate for each product k a specification that excludes the NGO terms

$$y_{ijkt} = \beta_{k0} + \beta_{k4} Free_j + \gamma_k X_j + \varepsilon_{ijkt} \quad (2)$$

although we note that to the extent there is an interaction between the effect of free distributions and the identity of the distributor ($\beta_3 \neq 0$) this regression will not provide an unbiased estimate of the Free treatment effect.

3.1. Take-up in wave 1

Table 3 shows the results, by product, from estimating equation (1) for Wave 1. The odd numbered columns show the effects of treatment assignment on take-up defined as a binary variable equal to 1 if a household purchased or accepted any quantity of the offered product and 0 otherwise. The even numbered columns report the quantity effects as measured in units of the product. We focus our discussion on Panel B, which reports results for all households found in Wave 1. In order to address the potential for differential selection into our sample described in Section 2.1, Panel A reports the results of an "intention-to-treat" analysis using the full sample of 5667 households irrespective of whether they were reached by marketers. For households that were not reached, we code them as not purchasing. This reflects the possibility that, given a preannouncement of the marketing, failure to be present may reflect a particularly low willingness-to-pay for the product.[22] Panel C reports the Wave 1 results for just those households that were found in both waves.

Unsurprisingly, take-up was much higher among those who were offered health products for free compared to those offered them for sale. As the odd-numbered columns show, among households in the for-profit group, being offered the product for free increased binary take-up by 46.3 percentage points for Elyzole, 23.7 percentage points for Panadol, and 69.9 percentage points for Zinkid. All coefficients are statistically significant with p-values below 0.01.[23]

The effect of free distribution on the quantity received follows a similar pattern for Elyzole and Zinkid: those in the Free treatment were not only more likely to receive any of the assigned product but also received more of the product on average. However, the Sale treatment increased the average quantity of Panadol obtained by 0.732 units (or 73.2 percent) relative to the Free treatment. As described above, households in the Sale treatment could purchase up to five units of the assigned product while distribution in the Free treatment was limited to one unit per household. In the case of Panadol, this leads to a reversal in the sign of the treatment effect between the binary and quantity regressions. While not all of the households in the Sale treatment purchased the product, those who did so purchased more than one unit on average. Surprisingly, the purchase rate was similar in those villages where Panadol or its equivalent was available at drug shops and in those where it was not. The intention-to-treat analysis in Panel A, which treats households that were not reached as not purchasing, preserves the pattern and statistical significance of coefficients reported in the Wave 1 sample.

Table 3 also shows that in the case of the unknown product (Zinkid), households were substantially more likely to purchase the product when it was offered for sale by an NGO rather than a for-profit entity. This difference is both statistically and economically significant: a 15.9 percentage point increase in take-up and a 50.7 percent increase in total quantity purchased.[24] Recall that the marketing scripts differed only in their description of the seller's identity and

[20] Surveyors were given details about how many units of the product each respondent had received, and so were able to verify whether all packaging was present. Furthermore, all blister packs distributed by marketers in Wave 1 had been discretely marked so that they could be identified as packaging distributed by our marketers, rather than the same product obtained from elsewhere. Here we deviated from our overall strategy of "naturalness." In this instance, we felt that acquiring some data on usage was important enough to deviate, but to roll it out in a promotional way so that it still was implemented under the pretense of a market introduction of goods. Those who did "win" the lottery (about 10%) do not behave differently in Wave 2.

[21] Stratification was primarily done to ensure balance. Although power is limited for subsample analyses, we do examine whether results are heterogeneous regarding remoteness and price levels. The results do not exhibit any significant heterogeneity along these dimensions.

[22] We were unable to verify the initial product assignments for 537 of the 1788 households that were not reached for our original treatment. In order to estimate the intention-to-treat specification, we imputed the initial treatment assignment using the same assignment protocols in the implementation. Because we do not have individual data on those not reached beyond the summary data described in Appendix Table A1, we have limited ability to empirically examine how those who selected in may be different ex ante to those who did not. While the results are qualitatively similar across sample frames and this imputation, they are not exactly the same. This highlights the methodological importance of paying attention to the message and means of soliciting individuals into experiments. In situations when individuals may have advance information about an upcoming treatment (in this case, the distribution of health products), it is important to keep accurate tracking sheets so that the initial entry into the sample frame can be understood well. In this case, we did not do that, and we would have learned more had we done so (Gazalle et al., 2011).

[23] The results in Table 3 for "any purchase" (the odd columns) are robust to using a Probit specification for the binary outcome variable. Those for the quantity purchased (the even columns) are robust to the Tobit specification, which accounts for left censoring of the dependent variable at zero and right censoring at one or five units, depending on the treatment group.

[24] The calculated percentage change is equal to NGO effect on quantity, 0.173, divided by the mean number of units purchased by those in the For-Profit-Sale treatment, 0.341. The latter is not equal to the constant in the regression equations because the regression includes controls for stratification variables.

Table 3
Demand in wave 1.

Product Offered : Dependent Variables:	Pooled		Panadol[a]		Elyzole[a]		Zinkid[a]	
	Take up (1)	Quantity[b] (2)	Take up (3)	Quantity[b] (4)	Take up (5)	Quantity[b] (6)	Take up (7)	Quantity[b] (8)
Panel A. Sample: All households on tracking sheets								
NGO in Wave 1	0.038	0.025	0.011	-0.040	0.002	0.002	0.101***	0.110***
	(0.031)	(0.051)	(0.041)	(0.109)	(0.037)	(0.047)	(0.035)	(0.039)
Free in Wave 1	0.360***	0.101**	0.240***	-0.369***	0.352***	0.194***	0.485***	0.463***
	(0.028)	(0.041)	(0.034)	(0.076)	(0.034)	(0.038)	(0.036)	(0.038)
Free∗NGO	-0.022	-0.002	0.003	0.063	0.010	0.010	-0.085	-0.096∗
	(0.044)	(0.060)	(0.055)	(0.118)	(0.054)	(0.061)	(0.052)	(0.055)
Constant	N/A[c]	N/A[c]	0.450***	1.132***	0.266***	0.399***	0.175***	0.199***
			(0.039)	(0.105)	(0.042)	(0.045)	(0.039)	(0.042)
Observations	5667	5667	1802	1802	2005	2005	1860	1860
Mean of NGO∗Sale	0.384	0.625	0.491	1.042	0.365	0.524	0.301	0.333
Mean of For-Profit∗Free	0.708	0.708	0.725	0.725	0.713	0.713	0.685	0.685
Panel B. Sample: All households found in Wave 1								
NGO in Wave 1	0.052	0.022	-0.007	-0.137	0.000	0.000	0.159***	0.174***
	(0.033)	(0.064)	(0.038)	(0.146)	(0.045)	(0.061)	(0.044)	(0.049)
Free in Wave 1	0.468***	0.068	0.237***	-0.733***	0.463***	0.233***	0.700***	0.666***
	(0.023)	(0.045)	(0.024)	(0.097)	(0.029)	(0.040)	(0.033)	(0.038)
Free∗NGO	-0.054	-0.019	0.011	0.142	-0.007	-0.014	-0.170***	-0.189***
	(0.034)	(0.066)	(0.039)	(0.151)	(0.047)	(0.062)	(0.046)	(0.050)
Constant	N/A[c]	N/A[c]	0.763***	1.847***	0.466***	0.695***	0.281***	0.317***
			(0.028)	(0.128)	(0.046)	(0.062)	(0.043)	(0.051)
Observations	3879	3879	1228	1228	1394	1394	1257	1257
Mean of NGO∗Sale	0.580	0.945	0.754	1.599	0.539	0.774	0.460	0.508
Mean of For-Profit∗Free	0.999	0.999	0.997	0.997	1.000	1.000	1.000	1.000
Panel C. Sample: All households found in Wave 2								
NGO in Wave 1	0.060∗	0.076	0.011	-0.034	-0.008	0.021	0.170***	0.201***
	(0.035)	(0.072)	(0.043)	(0.169)	(0.052)	(0.079)	(0.052)	(0.054)
Free in Wave 1	0.461***	0.070	0.231***	-0.716***	0.442***	0.204***	0.702***	0.687***
	(0.023)	(0.050)	(0.028)	(0.115)	(0.034)	(0.049)	(0.036)	(0.035)
Free∗NGO	-0.063∗	-0.072	-0.007	0.046	0.002	-0.034	-0.180***	-0.213***
	(0.036)	(0.076)	(0.044)	(0.178)	(0.053)	(0.080)	(0.053)	(0.056)
Constant	N/A[c]	N/A[c]	0.761***	1.830***	0.499***	0.768***	0.265***	0.292***
			(0.047)	(0.185)	(0.053)	(0.082)	(0.051)	(0.056)
Observations	2887	2887	926	926	1027	1027	934	934
Mean of NGO∗Sale	0.595	0.996	0.777	1.688	0.548	0.812	0.467	0.515
Mean of For-Profit*Free	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000

Village assignment to treatment was block randomized according to two variables. The first, price environment, included information about pricing and drug availability with three possible categories: (1) no drug outlets or none of our drugs; (2) no prices above the median or distributed for free; and (3) at least one price above the median. The second, remoteness, also had three categories: (1) easy to travel and close to health center; (2) difficult travel or far from health center; and (3) difficult travel and far from health center. All regressions include controls for stratification cell. Regressions in Panel B include control for Wave 1 respondent gender; regressions in Panel C include controls for Wave 1 respondent gender and whether Wave 2 respondent was same gender. Standard errors clustered by village in parentheses. * Denotes significance at the 10-percent level; ** at the 5-percent level; and *** at the 1-percent level.

[a] The generic names for the three drugs are: *paracetamol* for Panadol, *albendazole* for Elyzole, *zinc* for Zinkid.

[b] The "quantity" dependent variable is the number of units (defined as doses) received or purchased. Respondents in the Free group were offered one unit, respondents in the Sale group were able to purchase up to five units.

[c] Includes product-specific intercept.

motives. All information presented about the product itself was identical across the four treatment arms. Differences in the take-up rate could result either from differences in how households interpreted marketing information about product quality (e.g., the NGO was considered more accurate or trustworthy) or from how they perceived the offer prices (e.g., when offered by the NGO a price was considered a "better deal"). For the more well-known products, no such difference is evident.

Qualitative results from the post-marketing survey suggest a potential mechanism. Those offered Zinkid for sale by the NGO were more likely than those in the for-profit treatment to cite the product's health benefits as a reason for purchase (p-value: 0.059); however, they were no more likely to state "I purchased this because I trust you." We speculate that the results may still reflect a greater trust in the NGO when considering new products, but individuals are not explicitly aware of the NGO's role in forming their impressions. The magnitude of this effect is large: take-up increases from 30 percent to 46 percent.[25] This is consistent with other emerging work that points to the potential role of non-profit organizations as trust builders and may have important policy implications for organizations seeking to encourage the adoption of new technologies (Cole et al., 2013; Karlan, 2014). While our study design does not allow us to speak further to the mechanisms behind this effect, we believe future research into the role played by NGOs in stimulating demand for new products would be valuable.

3.2. Purchase rates in wave 2

Next, we turn to the question of what is the impact on future purchase rates of distributing the products for free. As previously

[25] These are the take-up rates in the For-Profit-Sale and NGO-Sale treatment arms, respectively. As noted above, the constant terms in the regression results reflect the inclusion of controls for stratification variables.

Table 4
Demand in wave 2.

Product Offered in Wave 2 Same As Wave 1?	Pooled Same (1)	Panadol[a] Same (2)	Elyzole[a] Same (3)	Zinkid[a] Same (4)	Aquasafe[a] Different (5)
Panel A: Take-up					
NGO in Wave 1	0.019	0.035	0.034	-0.002	0.055
	(0.032)	(0.040)	(0.056)	(0.052)	(0.059)
Free in Wave 1	-0.100***	-0.116***	-0.118*	-0.053	0.044
	(0.037)	(0.036)	(0.061)	(0.054)	(0.059)
Free*NGO	0.017	0.050	-0.006	-0.002	-0.109
	(0.051)	(0.058)	(0.086)	(0.074)	(0.078)
Constant	N/A[c]	0.838***	0.306***	0.241***	0.436***
		(0.074)	(0.068)	(0.067)	(0.069)
Observations	2150	687	786	677	737
Test of equality of Free coefficient w.r.t.					
Panadol	0.094	N/A	0.982	0.243	0.003
Elyzole	0.208	0.982	N/A	0.336	0.034
Zinkid	0.820	0.243	0.336	N/A	0.169
Mean of NGO∗Sale	0.555	0.866	0.521	0.276	0.571
Mean of For-Profit∗Free	0.480	0.709	0.379	0.233	0.566
p-value of Free + Free∗NGO =0	0.016	0.126	0.034	0.281	0.198
Effect of Free in specification excluding NGO terms	-0.091***	-0.091***	-0.120***	-0.053	-0.010
	(0.025)	(0.028)	(0.041)	(0.037)	(0.039)
Panel B: Quantity [b]					
NGO in Wave 1	-0.003	-0.086	0.048	0.022	0.052
	(0.069)	(0.172)	(0.096)	(0.059)	(0.092)
Free in Wave 1	-0.213***	-0.429***	-0.154	-0.060	0.101
	(0.073)	(0.151)	(0.097)	(0.057)	(0.111)
Free∗NGO	0.111	0.376	-0.056	0.021	-0.176
	(0.114)	(0.237)	(0.138)	(0.095)	(0.142)
Constant	N/Ac	1.813***	0.510***	0.216***	0.512***
		(0.181)	(0.108)	(0.067)	(0.098)
Observations	2150	687	786	677	737
Test of equality of Free coefficient w.r.t.					
Panadol	0.007	N/A	0.100	0.011	0.000
Elyzole	0.795	0.100	N/A	0.338	0.025
Zinkid	0.262	0.011	0.338	N/A	0.117
Mean of NGO∗Sale	0.845	1.720	0.688	0.312	0.714
Mean of For-Profit∗Free	0.729	1.363	0.495	0.240	0.762
p-value of Free + Free∗NGO = 0	0.207	0.754	0.031	0.607	0.370
Effect of Free in specification excluding NGO terms	-0.157***	-0.240**	-0.182***	-0.047	0.011
	(0.052)	(0.110)	(0.067)	(0.047)	(0.069)

Village assignment to treatment was block randomized according to two variables. The first, *price environment,* included information about pricing and drug availability with three possible categories: (1) no drug outlets or none of our drugs; (2) no prices above the median or distributed for free; and (3) at least one price above the median. The second, *remoteness,* also had three categories: (1) easy to travel and close to health center; (2) difficult travel or far from health center; and (3) difficult travel and far from health center. All regressions include controls for stratification cell. Standard errors clustered by village in parentheses. * Denotes significance at the 10-percent level; ** at the 5-percent level; and *** at the 1-percent level.

[a] The generic names for the three drugs are: *paracetamol* for Panadol, *albendazole* for Elyzole, *zinc* for Zinkid, and *sodium dichloroisocyanurate* for Aquasafe.

[b] The "quantity" dependent variable is the number of units purchased.

[c] Includes product-specific intercept.

described, in our setting, the impact of free distribution consists of two basic effects: a price anchoring effect that may depress demand and an information effect whose direction depends on whether the potential for learning is primarily positive or negative. Table 4 shows the results, by product, from estimating equation (1) for Wave 2 purchase behavior on the sample of all households found in Wave 2.

First, we examine the results pooled across all three products. Column 1 of Table 4 presents the effect on the extensive (Panel A) and intensive (Panel B) margins. In both cases, we find that the provision of free products leads to lower purchase rates approximately ten weeks later, with take-up after a free distribution 10 percentage points lower than after the for-sale distribution. However, note that this result pools three products that we deliberately chose for the experiment, not some naturally occurring set of products.

For each of the three products offered in Wave 1, subsequent purchase rates are lower in Wave 2 if the product was initially offered for free. For Panadol and Elyzole, the results are substantial and statistically significant. The bottom of panel A reports results from estimating the simplified equation (2), without the NGO terms. As shown in columns 2 and 3, those previously receiving the product for free are 9.1 percentage points (s.e.: 2.8) and 12.0 percentage points (s.e.: 4.1) less likely to purchase any of the product in Wave 2. In the case of Zinkid, for which there is scope for positive learning, the effect is muted. The purchase rate of Zinkid in the Free treatment group is 5.3 percentage points (s.e.: 3.7) lower than in the Sale treatment, but the difference is not statistically significant (column 4). Panel B displays results for the quantity of units purchased. Again, the effect of prior free distribution is negative and substantial for both Panadol and Elyzole, a reduction in the quantity purchased of 0.24 and 0.18 units, respectively. The effect of prior free distribution of Zinkid, the product with scope for positive

Table 5
Demand in wave 2, intention to treat samples.

Product Offered in Wave 2 Same As Wave 1?	Pooled Same (1)	Panadol[a] Same (2)	Elyzole[a] Same (3)	Zinkid[a] Same (4)	Aquasafe[a] Different (5)
Panel A. Sample: All households on tracking sheets					
NGO in Wave 1	0.012	0.044	0.005	-0.010	0.013
	(0.022)	(0.042)	(0.031)	(0.024)	(0.033)
Free in Wave 1	-0.048**	-0.053	-0.051	-0.040	0.046
	(0.024)	(0.037)	(0.035)	(0.027)	(0.036)
Free∗NGO	0.048	0.092	0.026	0.031	0.002
	(0.035)	(0.059)	(0.052)	(0.039)	(0.046)
Constant	N/A[c]	0.426***	0.178***	0.123***	0.236***
		(0.048)	(0.030)	(0.032)	(0.040)
Observations	4242	1333	1526	1383	1425
p-value of Free = 0	0.046	0.161	0.147	0.148	0.201
p-value of Free + Free∗NGO = 0	0.994	0.374	0.469	0.754	0.133
Panel B. Sample: All households found in Wave 1					
NGO in Wave 1	0.013	0.034	0.019	-0.010	0.001
	(0.030)	(0.048)	(0.048)	(0.037)	(0.046)
Free in Wave 1	-0.098***	-0.130***	-0.096*	-0.068*	0.032
	(0.033)	(0.043)	(0.052)	(0.039)	(0.048)
Free∗NGO	0.057	0.119*	0.017	0.043	0.003
	(0.046)	(0.065)	(0.073)	(0.056)	(0.062)
Constant	N/A[c]	0.715***	0.266***	0.194***	0.400***
		(0.065)	(0.054)	(0.052)	(0.054)
Observations	2897	910	1064	923	982
p-value of Free = 0	0.003	0.003	0.067	0.084	0.508
p-value of Free + Free∗NGO = 0	0.196	0.817	0.103	0.543	0.409
Panel C. Sample: All households found in Wave 2					
NGO in Wave 1	0.019	0.035	0.034	-0.002	0.055
	(0.032)	(0.040)	(0.056)	(0.052)	(0.059)
Free in Wave 1	-0.100***	-0.116***	-0.118*	-0.053	0.044
	(0.037)	(0.036)	(0.061)	(0.054)	(0.059)
Free∗NGO	0.017	0.050	-0.006	-0.002	-0.109
	(0.051)	(0.058)	(0.086)	(0.074)	(0.078)
Constant	N/A[c]	0.838***	0.306***	0.241***	0.436***
		(0.074)	(0.068)	(0.067)	(0.069)
Observations	2150	687	786	677 737	
p-value of Free = 0	0.007	0.002	0.056	0.325	0.458
p-value of Free + Free∗NGO = 0	0.016	0.126	0.034	0.281	0.198

Village assignment to treatment was block randomized according to two variables. The first, *price environment,* included information about pricing and drug availability with three possible categories: (1) no drug outlets or none of our drugs; (2) no prices above the median or distributed for free; and (3) at least one price above the median. The second, *remoteness,* also had three categories: (1) easy to travel and close to health center; (2) difficult travel or far from health center; and (3) difficult travel and far from health center. All regressions include controls for stratification cell. Standard errors clustered by village in parentheses. * Denotes significance at the 10-percent level; ** at the 5-percent level; and *** at the 1-percent level. (a) The generic names for the three drugs are: *paracetamol* for Panadol, *albendazole* for Elyzole, *zinc* for Zinkid, and *sodium dichloroisocyanurate* for Aquasafe. (b) The "quantity" dependent variable is the number of units purchased. (c) Includes product-specific intercept.

learning, is negligible.[26]

We cannot compare the purchase rate across time in order to determine whether the free distribution reduced purchase rates in absolute terms or merely relative to a sales distribution. Unfortunately, such an analysis would not be valid as the two distribution waves occurred at different times in the year and demand is subject to seasonal variation.

We do not find evidence that the anchoring effect of free distributions spills over to other health products. Column 4 reports the effect of Wave 1 treatment status on the Wave 2 purchase decisions for a new product, Aquasafe. Note that because there is no reason to suspect cross-product learning, this is a test of whether free distribution of one health product moves the reference point for another. Naturally, this is not dispositive. We are testing potential cross-product spillovers from one of three particular products to another product offered by a different organization. We cannot reject the null of no effect. While the 95%-confidence interval rules out a cross-product effect as large as the own effect of free distribution for Panadol or Elyzole, it remains quite large. The 95%-confidence interval spans from −8.7 to +6.7 percentage points. We also do not see statistically significant differences between prior distribution by an NGO and prior distribution by a for-profit.

As with the Wave 1 results, we also conduct an "intention-to-treat" analysis using both the full sample of 5667 households irrespective of whether they were ever reached by marketers and then for the sample of all 3879 households reached in Wave 1. As described above, we code all households that were not reached as not purchasing. Panels A and

[26] Although the treatments were randomly assigned at the village level, there is a slightly higher share of women in the for-profit sample versus the NGO sample (Table 1). Therefore, as a robustness check, we also estimate the regression specification for Table 4 including controls for the respondent's gender and gender interacted with treatment. Although female respondents are slightly more likely to purchase each of the health products than men, none of the differences are significant, and the estimates of the primary treatment effects are substantially unchanged when these individual controls are included. Table A7 reports these results. Note also that reported percentage changes are relative to the Sale treatment mean; the regression constant reflects inclusion of controls for stratification variables.

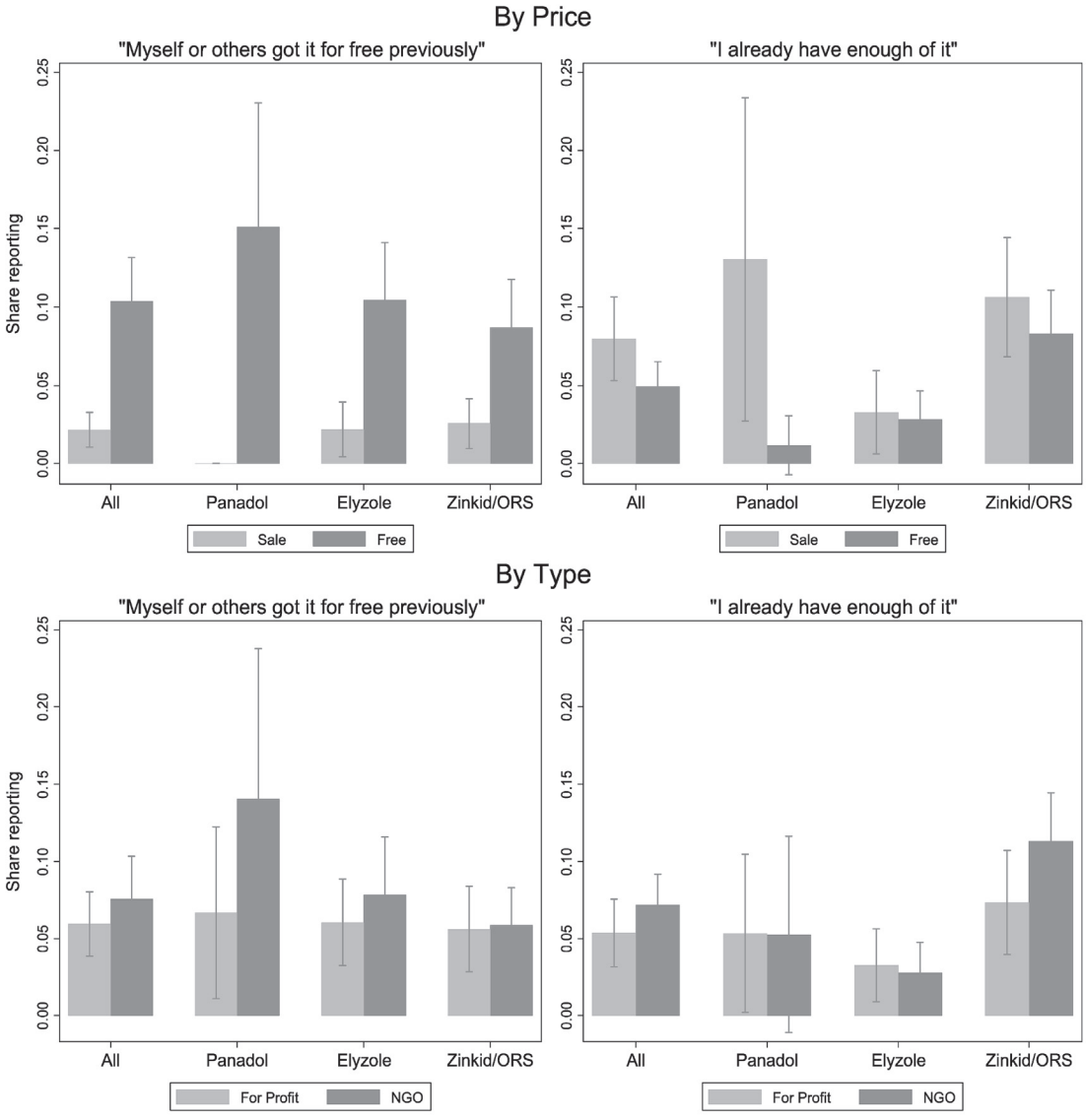
B of Table 5 report the results from the intention-to-treat analysis. For ease of comparison, Panel C replicates the take-up results reported in Table 4. When we include all observations in the initial sample frame none of the individual product coefficients remains statistically significant. The anchoring effect remains significant in the pooled sample, albeit with a reduced magnitude due to the attrition rate of approximately 25% across rounds that we discuss in Section 2.1.

We note that in the case of Panadol, there is some evidence that free distributions by an NGO suffer less of an anchoring effect than those by for-profits. This is contrary to the hypothesis we had in the design of the experiment that a free distribution by an NGO may shift expectations about future prices whereas a for-profit may be perceived as simply trying to improve its brand image or providing free samples for learning or habit formation. The result is not robust to different sample frames and sample-entrance assumptions, and thus we caution against making any substantive inference.

4. Discussion and alternative explanations

The empirical results show that purchase rates following a free distribution can be lower than following a for-sale distribution. Here we first consider the qualitative evidence in support of price anchors and then consider alternative mechanisms. We also discuss potential limitations in our interpretation of the scope for learning about different products and some methodological suggestions for future work.

Qualitative evidence from the post-marketing questionnaire supports the role of price anchors in reducing relative demand following a free distribution. After the Wave 2 distribution, the marketers asked all respondents why they made their purchase decisions. The question was asked in an open-ended way without prompting, and surveyors coded the responses into predetermined categories based on piloting of survey questions. As is shown in Fig. 1, among those who decided not to purchase the offered good in Wave 2, 10.4 percent of respondents in the



Note. Share of respondents reporting a specific reason for not purchasing the offered product in Wave 2 conditional on not purchasing. Multiple responses were allowed. Whisker bars represent 90%-confidence intervals

Fig. 1. Reasons for not purchasing, wave 2.

Free treatment stated that they did not purchase the product because either they or others whom they knew had previously been given it for free. In contrast, only 2.2 percent of those in the Sale treatment responded similarly (p-value: 0.000). A further 4.1 percent of the Free treatment group stated that the product was too expensive versus 1.7 percent in the Sale group (p-value: 0.027). While these responses are subject to the qualification that these responses are participants' self-reported explanations for own behavior, the Wave 2 distributors were affiliated with a different entity than either of those seen in Wave 1, ameliorating concerns over experimenter demand effects. Furthermore, in the supplemental survey of 1069 households in 84 of the 120 study villages between the Free and Sale treatments, households in the Free treatment were 13 percentage points more likely to expect distribution of similar health products to be free in the future (p-value: 0.022).

Next, we assess the plausibility of eight alternative mechanisms that could explain differential effects between free and priced distributions. These include (i) stock on hand, (ii) expectations of a pricing regime change, (iii) income effects, (iv) liquidity constraints, (v) externalities, (vi) habit formation, (vii) prices as a signal of quality, and (viii) cognitive costs. Below we consider each in turn.

First, we consider what is perhaps the most obvious alternative mechanism through which free distribution could reduce future purchase rates: stock. A central limitation of this paper is that data on demand for health products comes exclusively from purchases made through our door-to-door marketing campaigns and no data is collected on purchases made outside of the experimental distribution from local health suppliers. If, for example, participants in the free distribution arm were more likely to purchase or acquire products after our distribution, this would mean that we underestimate demand in the free arm by measuring purchase rates only through our experimental data collection. While we cannot fully rule out unobserved differences in arms that come from purchase rates outside of our data collection, we provide several sources of data to calibrate the extent to which these effects are likely to affect the interpretation of our findings. We first consider whether those in the free distribution arm may not purchase in Wave 2 simply because they still have a stock of the relevant product at home, using data collected from collecting data on usage of experimentally distributed products through household visits and from further qualitative data on reasons for purchase or not purchase of experimental products.

Table 6 reports measures of experimentally-provided stock on hand before Wave 2. For Panadol and Elyzole, the two products for which we saw a significant negative effect from prior free distribution, stock in the Free treatment group is no higher than in the Sale group. In fact, due to differences across treatments in the maximum quantity available per household (see Section 3.1 for details), average experimentally-provided stock-on-hand in the Sale treatment of the Panadol group was actually larger than in the Free treatment. To the extent that stock-on-hand did affect demand, it would have made households who were offered Panadol for free in Wave 1 slightly more—not less—likely to purchase in Wave 2. In the case of Zinkid, those in the Free treatment did have more tablets remaining. To the extent that stock affects demand, this should lower relative purchase rates for those in the Free treatment. In contrast to the other two products, this suggests that our estimates would be an upper bound on the magnitude of the effect. However, Zinkid, the product for which we expected some scope for positive learning, is the product for which we do not find a statistically significant negative effect of free distribution on Wave 2 purchase rates.

The preceding results examine only the remaining experimental stock and do not consider the household's overall stock, which could be obtained from other sources. To address this, we asked respondents in a post-marketing survey why they did not purchase products in Wave 2. As Fig. 1 shows, we do not find a higher share of respondents in the Free group giving "I already have enough of it" as reason for not purchasing. If anything, the share is higher in the Sale group, but the differences are not statistically significant. These findings suggest that stock is not the major driver of lower purchase rates following a free distribution compared to sale. Nonetheless, it may be that other factors, such as differences in the perception of the relative quality of our products

Table 6
Observed usage summary statistics.

	Conditional on receiving any in Wave 1		p-value	N	% in Sale receiving any in Wave 1	Scaled to include nontakeup in Wave 1		p-value
	Sale	Free			Wave 1	Sale	Free	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
NumNumber of tablets distributed in Wave 1								
Panadol	21.36	10.00	0.00	99	75.5%	16.13	9.98	0.00
Elyzole	8.68	6.00	0.00	85	54.1%	4.69	5.99	0.00
Zinkid & ORS	11.03	10.00	0.12	67	38.3%	4.22	9.97	0.00
Mean tablets remaining from experimental stock								
Panadol	2.56	1.03	0.04	99	75.5%	1.93	1.02	0.16
Elyzole	0.16	0.04	0.44	85	54.1%	0.09	0.04	0.65
Zinkid & ORS	5.34	4.86	0.69	67	38.3%	2.05	4.84	0.00
Proportion of tablets used								
Panadol	0.90	0.90	0.91	99	75.5%	N/A	N/A	N/A
Elyzole	0.99	0.99	0.67	85	54.1%	N/A	N/A	N/A
Zinkid & ORS	0.53	0.51	0.90	67	38.3%	N/A	N/A	N/A
Share of respondents who have positive experimentally provided stock								
Panadol	0.37	0.18	0.04	99	75.5%	0.28	0.17	0.20
Elyzole	0.03	0.02	0.88	85	54.1%	0.01	0.02	0.78
Zinkid & ORS	0.59	0.61	0.88	67	38.3%	0.22	0.60	0.00

Households that did not receive the a product in Wave 1 were not included in the sample for usage checks of experimentally provided product. The share receiving the product in Wave 1 for the Free treatment is approximately 100% for all products. In a previous version of this paper we misreported that 329 individuals were "selected" for usage checks and 251 were "found," implying that "found" refereed to the usage checks. The variable "found" should have indicated "found in Wave 2" and the variable "selected" should have indicated "contacted for usage checks". Because our interest in usage checks is to understand the mechanism behind the Wave 2 results, we restrict the sample frame for analysis to only those individuals reached in Wave 2. Results on the full sample of 329 households reached in the usage checks are statistically identical (results available from the authors on request). We note that the attrition rate of 24% from the usage check to Wave 2 is higher than often found in developing country studies and reflects a deliberate methodological decision to adhere to a more "natural" marketing process, rather than persistently return to households to, in this case, adjudicate their eligibility for a marketing prize. See Section 2 for more discussion of study design and attrition.

or competitiveness of our prices compared to other health suppliers due to differential purchase rates outside of our experimental set-up across treatments may play a role in explaining our results.[27]

A second potential mechanism is a pricing regime change story. Seeing a free distribution could generate the expectation of future free distributions. Indeed, in the Kőszegi and Rabin (2006) model, reference points are set in part by rational expectations about future prices. This is a plausible mechanism for generating price anchors in our setting as well. We would have expected it to be more of a factor for free distributions by an NGO (from which one might reasonably expect free distributions to continue) than from for-profits (from which they are unlikely). As described earlier, this, in part, motivated our interest in distributor identity; however, the effect of prior free distributions on subsequent purchases does not vary along this dimension.

A third potential mechanism is income effects. People who received the health products may have lost fewer workdays due to illness during the ten weeks between the two waves and thus may have had more disposable funds to purchase products in the second wave of marketing. If an income effect existed, this would have increased relative demand in the Free group and would therefore imply that we are underestimating the price anchoring effect. It is worth noting that in contrast to insecticide-treated bed nets, where income effects could exist, we expect any income effects of the products in this study to be relatively modest.

Fourth, liquidity may have affected demand. Since households who received the product for free effectively received a transfer, they may have had more money available when marketers appeared in Wave 2. However, any effect along this dimension would tend to increase demand in the Free treatment. We would also expect any effects to be quite small. The magnitude of the transfer was low—about $0.80 per household. Moreover, villages were revisited approximately ten weeks later and this future visit was not announced at the time of the first. It seems implausible that people kept the funds they would have otherwise spent on drugs in Wave 1 for a full ten weeks. Finally, to mitigate liquidity constraints, flyers were distributed a few days prior to each marketing visit to allow respondents to get money ready.

A fifth possible mechanism affecting demand is positive externalities. The argument here would be that higher take-up in Wave 1 reduced disease prevalence and hence the utility from purchasing the product in Wave 2. However, an externality argument cannot explain the negative effect on purchase rates in Wave 2 from free distribution for Panadol, since it is implausible that pain relievers have meaningful externalities. In contrast, the deworming medicine Elyzole does have positive externalities. Dewormed children are less likely to transmit worms to their siblings and peers (Miguel and Kremer, 2004; Ozier, 2011), which could explain a negative effect of free distribution on later purchase rates. However, to the extent that such effects were present in our study, we expect that they were quite small. On average, we distributed Elyzole to only about five percent of households per village in Wave 1. As such, any reduction in disease loads and hence the utility of purchase in Wave 2 would have been quite small.

Sixth, habit formation may have influenced demand. Suppose that upon receiving the health products, households become habituated to using them. Habit formation would make it more likely that households who received the product in Wave 1 then purchase the product in Wave 2, regardless of the direction of learning effects. Since a higher share of households received the products in the villages assigned to the Free treatment, habit formation should have a positive effect on demand there. In contrast, our results move in the opposite direction.

Seventh, higher prices may signal higher quality (Milgrom and Roberts, 1986; Heffetz and Shayo, 2009; Ashraf et al., 2013). All else equal, being offered a product for a higher price should then increase later demand just as we would expect from the price anchoring model. However, the signaling mechanism should have a larger effect for products with more uncertainty about the benefits and would have the exact opposite effect of our model of experience learning, i.e., positive prices should increase relative demand for the least well-known products. Furthermore, we would expect that the price signal would be likely to be relatively less important for a product like Panadol where the majority of respondents in both sale and free distribution personally experienced whether the product alleviated their pain (since the high percentage of the sample who received Panadol are overwhelming likely to have consumed some of the product). While our point estimates across products are in line with the anchoring mechanism rather than the quality signal alternative, we again note that the differences in these estimates are not statistically significant. We cannot rule out the possibility that prices as a signal of quality may explain some of the differences in purchase rates following free and sale distributions. Since these mechanisms have distinct policy implications, we think further research to distinguish their effects would be useful.

Finally, cognitive costs of determining a product's value may influence our results. Suppose that any time individuals are faced with a positive price on a less well established product, they have some probability of being willing to incur the cognitive cost of determining their own valuation for the product. Without first having determined their valuation, they do not buy, since they are uncertain whether the price is above or below their personal valuation of the good. Then, being repeatedly exposed to a purchase decision should increase purchase rates, since in every subsequent interaction fewer and fewer people need to incur the cognitive cost.

While we chose three health products specifically for variation in the scope for learning about the products themselves, we acknowledge that our experiment could have induced learning on other dimensions. For example, households could have updated their beliefs about the relative attractiveness of our experimental prices or the relative value of branded versus generic medicines. The three products also differ along other dimensions, e.g., the target users. We therefore interpret the variation across products as suggestive of the experience good model of learning and its interaction with price anchors, not as a crisp test of theory.

Finally, we note several methodological lessons from our exercise. First, this was a fairly large study but still was hampered by statistical power concerns when testing separately across products. This is often the case when examining heterogeneous treatment effects. For this particular question, having micro-level data on both consumers' willingness-to-pay at baseline (e.g., as done in Berry et al., 2018) or on consumers' knowledge and beliefs would have improved power. The methodological tradeoff is the loss of some of the "naturalness" of the exercise as implemented in this study. Second, while it would entail another tradeoff against naturalness, entrance into the sample frame should be done identically for all treatment groups. Whereas in our setup we did not have differential attrition from Wave 1 to Wave 2, the flyers distributed in anticipation of the study, intended to follow normal marketing procedures, did generate a higher entrance-into-the-sample rate for the free distribution versus the sale. Third, conducting this type of experiment in settings with more diversity of market conditions (e.g., with respect to the presence and pricing of drugs) would provide a valuable opportunity to examine how local market conditions influence the results. Fourth, building this exercise on top of other long-term data collection would produce important economies of scale in operations and also provide access to richer baseline and long-term data. This would allow researchers to examine everything from more nuanced heterogeneous treatment effects to long-term usage, welfare and market responses.

[27] The products we offered were available for sale in local drug shops in 11%–36% of the study villages (see Table 1). There are no distinguishable difference in the effect of prior free distribution on subsequent purchase rates between those villages where the product was available for outside purchase and those where it was not.

5. Conclusion

The way in which a product is distributed today can have lasting effects on demand. Previously observed prices can anchor perceptions of value. Experience with a product informs an individual's beliefs about its intrinsic value. The form of a distribution or identity of a distributor can be interpreted a signal of quality.

This paper studies these issues through a field experiment in northern Uganda in which three curative health products, chosen for variation in the scope for learning and not for policy relevance, were distributed door-to-door either free or for sale and by either an NGO or a for-profit company. Our key outcome measure is the observed purchase rate for these products during a subsequent door-to-door sale ten weeks later by an unrelated, for-profit firm. For all three products, purchase rates are lower after a free distribution. While several alternative mechanism could explain the difference, we can rule out the mechanical effect of having more experimentally-provided stock on hand. Households' qualitative responses are suggestive of price anchors.

While not dispositive, the pattern of subsequent demand across the three products provides suggestive support for the predictions of Dupas (2014) and contributes to the literature on experience goods pricing (Nelson, 1970; Villas-Boas, 2004; Shapiro, 1983; Bergemann and Välimäki, 2006). There is a tension between learning and the potential for prices to directly affect individuals' willingness to pay, irrespective of a product's intrinsic value. This mechanism may be particularly important in the case of pharmaceutical demand (Crawford and Shum, 2005) and health goods in low income countries more generally (Dupas, 2011). It is also applicable to agricultural products and other goods where subsidies or discounts are common policy instruments. While we do not find evidence of price anchors for one product spilling over to another, the potential for categorical price judgments is high and could affect supply through market entry decisions. This remains an important area for future research.

Surprisingly, purchase rates after a free distribution do not vary with the distributor's identity. If households perceive price signals from an NGO (from whom they could reasonably expect some chance of future free distributions) differently than those from a for-profit company, it does not affect subsequent purchase rates in our study. However, distributor identity does matter for the contemporaneous sale of the relatively unknown product. Households are 50 percent more likely to purchase Zinkid, an improved but largely unknown treatment for childhood diarrhea, from the non-profit than from the for-profit firm selling at the same price and providing the same product information. The finding that NGOs are more effective at stimulating demand for unknown products has important policy implications and merits further attention (Cole et al., 2013; Karlan, 2014).

Ultimately, the answer to the question "how will one-time subsides affect future demand" is simple: it depends. We studied, in one context, a few important factors. Building a robust model that both informs policy and extends our knowledge of markets requires further research that replicates prior tests in other contexts and explores additional potentially important factors.

Appendix A. Supplementary data

Supplementary data related to this article can be found at https://doi.org/10.1016/j.jdeveco.2018.07.013.

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