



Keeping the doctor away: Experimental evidence on investment in preventative health products[☆]



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ABSTRACT

Household investment in preventative health products is low in developing countries even though benefits from these products are very high. What interventions most effectively stimulate demand? In this paper, we experimentally estimate demand curves for health products in Kenya, Guatemala, India, and Uganda and test whether (1) information about health risk, (2) cash liquidity, (3) peer effects, and (4) intra-household differences in preferences affect demand. We find households to be highly sensitive to price and that both liquidity and targeting women increase demand. We find no effect of providing information, although genuine learning occurred, and we find no evidence of peer effects, although subjects discussed the product purchase decision extensively.

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1. Introduction

Over 10 million children in developing countries die each year, many by diseases which could be avoided by simple preventative health investments (Jones et al., 2003). Many studies have shown that investment in preventative health products yields enormous health benefits in developing countries.¹ Despite these benefits, investment in preventative health

products is generally low among poor households and tends to fall off rapidly at even small positive prices (i.e., Ashraf et al., 2010; Cohen and Dupas, 2010; Dupas, 2009, 2013). Why does investment in preventative health remain low when benefits are high? In this paper, we present results from a novel set of field experiments designed to explore the relative importance of various factors that potentially influence the demand for preventative health products.

We consider four main factors which have been identified in other research as possible determinants of either the level or the elasticity of the demand curve for health products. First, households may lack health information: they may not be fully aware of the health risks they face, or of the role that a product can have in mitigating such risks. Indeed several studies reveal positive effects on health behavior from informing households about the benefits of certain types of sickness prevention. For example, Cairncross et al. (2005) and Luby et al. (2004, 2005) show large behavioral effects of intensive education campaigns on hygiene, while Jalan and Somanathan (2008) and Madajewicz et al. (2007) find that informing households about fecal and arsenic water contamination, respectively, influenced them to use alternative water sources and purification technology. Second, households may lack liquidity. This could be because they are credit constrained (i.e., Devoto et al., 2012; Tarozzi et al., 2013), because they lack a secure place to save money (i.e., Dupas and Robinson, 2013a), or because they do not

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¹ A partial list of technologies with high returns includes chlorine for water (Arnold and Colford, 2007; Fewtrell et al., 2005), insecticide-treated bed nets (Lengeler, 2004), and iron supplementation (Bobonis et al., 2006; Thomas et al., 2006).

save as much as they planned to for behavioral reasons (i.e., Ashraf et al., 2006; Duflo et al., 2011). Peer effects may constitute a third influence, creating situations where multiple, Pareto-rankable equilibria may exist, and where sub-optimal levels of health product adoption are possible if there are few early adopters. Depending on the characteristics of the product, such peer effects may tend to increase investment (i.e., Dupas, 2013) or decrease it (i.e., Kremer and Miguel, 2007). Fourth, numerous studies have shown that there may be intra-household conflict in spending on health (particularly for children). For example, Duflo (2003) and Thomas (1990) provide evidence that women are more likely to invest in children's health than men, suggesting that targeting preventative health products at female household heads may be important.²

To test these different hypotheses, we perform a set of field experiments in four countries — three smaller studies in Guatemala, India, and Uganda, conducted in 2008, and a larger study in Kenya conducted in 2010. In each site, we follow recent papers to estimate experimental demand curves by providing households with coupons for randomly selected discounts that could be redeemed in exchange for a given health product.³

Our paper contributes to the existing literature in three significant ways. First, while there have been a number of studies of single factors that may affect health product demand in individual countries, our study in Kenya is among the first to simultaneously test multiple hypotheses to determine which factors have the greatest relative impact on demand. Second, because we carry out tests on three different kinds of preventative health products, we are better able to infer from our results as applying to health products more generally. Finally, our findings on the effect of information and price have an added degree of external validity since we test these factors in all four of our country sites, providing greater confidence that results are not highly context-dependent.

Our main experiment in Kenya was conducted among 999 households in 2010 and focused on a particular health technology which has not been examined in previous studies, but which could have potentially significant health impacts: rubber shoes for children. One important way shoes may improve health is by preventing hookworm infection, which is typically transmitted when a person's skin comes into contact with contaminated soil (usually through bare feet). While worms can be easily treated after infection (see Miguel and Kremer, 2004), initial infection can be avoided by wearing shoes.⁴ Though the effect of deworming is an open question on a global scale, our main study takes place in a region in Kenya in which deworming has been shown to have important short- and long-term impacts (Baird et al., 2011; Miguel and Kremer, 2004), and substantial spillovers for young children (Ozier, 2011).⁵ While we did not perform parasitological tests for worms, self-reported infection was quite high: Respondents reported that 23% of their children had worms in the past year. While much of this is due to poor sanitation, low shoe usage is also a major risk factor. In our sample, only 17% of children owned shoes and a smaller percentage wore them regularly. In this geographical context, preventing infection by wearing shoes is likely to have a substantial direct health effect, as well as a positive spillover effect on others.⁶

² Dupas (2011) provides an excellent and more amplified review of these issues.

³ See, for example, Ashraf et al. (2010), Cohen and Dupas (2010), Dupas (2009), and Kremer and Miguel (2007).

⁴ Though there are no randomized controlled trials on the effect of shoe wearing that we are aware of, several non-experimental studies show that regular shoe usage is associated with reduced hookworm infection when controlling for other risk factors (Erosie et al., 2002; Phiri et al., 2000). This seems plausible given the transmission pathway for the disease.

⁵ Recent work by Taylor-Robinson et al. (2012), suggests that the benefits of deworming campaigns may not be substantial. The authors synthesize 42 randomized control trials of deworming efforts and conclude that there is insufficient evidence of consistent benefit on nutrition, hemoglobin, school attendance or school performance. Nonetheless, worms are likely a major problem in this part of Kenya given these earlier studies.

⁶ While preventing hookworm infection might be the most important health benefit of shoes, it is not the only one. Wearing shoes reduces foot injuries and the chance of infection from such injuries.

Our Kenya study carried out the following set of experimental treatments in conjunction with our basic price treatment. To measure the impact of information, we provided a randomly selected subset of households with an information script on the dangers of worm infection, transmission pathways, and on the importance of wearing shoes in hookworm prevention. To assess the role of liquidity constraints, we gave households a randomly determined amount of cash.⁷ In order to test for peer effects, we geographically stratified the intensity of our low-price treatments to ascertain whether households surrounded by heavy adoption are more likely to purchase the shoes. We are also able to use random variation to examine whether information spilled over to the neighbors and other peers of treated households. To examine whether demand varied by parental gender, we randomly selected either the husband or wife (among married couples) for participation. This person was the one to receive the coupon, cash, and information script.

An important result from our set of experiments is that, despite the importance often given to information dissemination in health campaigns in developing countries, we find that information alone has no impact on the ultimate purchase decision. We show that while the informational script substantially increased knowledge about worms, this did not translate into increased demand. Our estimates are precise enough that we can rule out large effects, and the results are not specific to Kenya: we find no effect of information in Guatemala and Uganda, and some mixed evidence in India. These results suggest that information alone is unlikely to be a panacea for underinvestment in preventative health products. We also do not find any evidence that peer effects play a significant role in household purchases of the shoes.

By contrast, we find strong evidence that liquidity is important. Increasing the cash payment from zero to the mean payout in the experiment (35 Kenyan shillings, or US \$0.47) increases redemption by approximately 8 percentage points. This is roughly equivalent to an 8 Kenyan shilling (Ksh) reduction in the price, or about 9.4% of the 85 Ksh retail price. This result implies that credit or liquidity constraints are an important limiting factor in health investment.

We also find that women are more likely to redeem their coupons (by about 9 percentage points). This corresponds to roughly a 9.5 shilling reduction in price, or 11% of the retail price. This result is closely related to earlier studies on intra-household investment such as that of Thomas (1990), who shows that the propensity to invest in children increases more strongly with female than male income, and Duflo (2003) who uses an exogenous change in pension eligibility in South Africa and finds similar results. However, our study is different because the experiment did not change relative incomes (and by extension, intra-household bargaining power). Instead, the experiment only varied which spouse received the coupon. This result suggests that the flow of information within the household may be limited. In this context, it appears that mothers value health investment in children more than fathers, and that there is intra-household conflict over the allocation of resources between health investment in children and other expenditures. Increasing investment in children appears to increase the mother's welfare, but may increase her husband's welfare by less, or even reduce it. Thus, if the husband receives the coupon, he may not choose to redeem it and withhold knowledge of it from his wife. This result is similar to Ashraf (2009), who finds evidence of intra-household communication barriers in a field experiment on savings in the Philippines.

While credit constraints and targeting women are therefore important, ultimately these effects are limited relative to the effect of price. About 78% of the variation in health-product purchase is explained through price variation alone, overshadowing liquidity and gender

⁷ The cash payment was very small relative to lifetime income. The average payout was 35 Ksh, relative to weekly income of 900 Ksh and asset ownership of around 23,000 Ksh (see Table 1). Thus, the payout had a negligible effect on household income and should only have affected cash-on-hand.

effects. Price was also by far the dominant factor in the smaller studies in Guatemala, India, and Uganda.

Our results have several important policy implications. First, as in Devoto et al. (2012), Tarozzi et al. (2013) and others, our results suggest that providing credit to cash-constrained households could increase investment. Another alternative might be to provide people with savings products geared towards health. For example, Dupas and Robinson (2013b) experiment with several simple savings mechanisms and find that providing even the simplest savings technology (a box with a lock) increased investment in preventative health by 68%. Second, in line with many other papers, our results suggest that targeting interventions at women are more likely to positively affect investment in child health. Third, our results also suggest that simple information campaigns are unlikely to manufacture additional demand for health products. While intensive and potentially very costly educational interventions in the spirit of, for instance, Cairncross et al. (2005) or Luby et al. (2004, 2005) might be effective in stimulating demand, less involved interventions are unlikely to have much impact. Finally, price is by far the biggest determinant of investment: a major reason for low investment in preventative health products is simply that low-income households are unwilling to pay high prices for them. Thus the potential subsidization of preventative health products, on externality grounds, is clearly an implication of our research.⁸

2. Experimental design, Kenya

2.1. Background

Worldwide, over 2 billion people are infected with soil-transmitted helminths (STHs), the most common of which include hookworm, roundworm, and whipworm (Hotez et al., 2007). Such infections are very prevalent in Western Kenya, where this study takes place.⁹ School-aged children and pregnant women are especially vulnerable to STH infection. While mild infection typically has limited health consequences, more severe infections can have effects on morbidity, and are also suspected to increase vulnerability to other illnesses, such as malaria, HIV, tuberculosis, and anemia.¹⁰ A number of studies have demonstrated important health and education effects of reducing worm infections (Baird et al., 2011; Bleakley, 2007; Miguel and Kremer, 2004; Ozier, 2011).

Once infected with hookworm, children can be dewormed through the use of the relatively inexpensive drug albendazole. Another pathway to reduce worm infection is to prevent children from getting infected in the first place. When an infected person defecates in the soil, hookworm eggs hatch and develop into larvae, which are able to live in the contaminated ground for up to a month before requiring a human host to survive (Brooker et al., 2006). Hookworm helminths are most commonly contracted through the skin (typically through bare feet), after which they migrate into the circulatory system, passing through the trachea and on to the esophagus where they are swallowed and passed into the intestines (Bethony et al., 2006). Thus, in areas where people do not have access to pit latrines or flush toilets, hookworm is likely to be a problem.

An important way to prevent hookworm infection is to limit skin contact with infected soil. Since infection is often through the feet, the simplest technology to prevent infection is to wear shoes. Several studies

have documented a strong correlation between regular shoe-wearing and a decreased incidence of worms. For instance, Erosie et al. (2002) and Phiri et al. (2000) estimate worm infection odds ratios of 7.1 and 1.8, respectively, to regular shoe wearing among school children in Malawi and Ethiopia (while controlling for other risk factors). While these studies cannot document a causal relationship between the lack of shoes and incidence of STH infection, the findings suggest that wearing shoes should reduce infection given the transmission pathway.

While there are obvious non-health benefits to wearing shoes, the public health evidence suggests that, overall, shoes are likely to have large direct effects on health (not only by preventing hookworm infections, but also by keeping children clean and by preventing potentially very painful foot injuries). In addition, the social return to shoe-wearing will be even higher because of spillovers to other individuals living nearby. In our sample, health is a major reason that people report for why they wear shoes; in our data, 74% reported health as the most important reason to buy shoes while 26% reported that it is to have children properly dressed. Similarly, 76% and 54% list worm and injury prevention, respectively, as reasons to purchase shoes. Another 43% report keeping children clean as a reason (which can be related to health as well).¹¹

2.2. Sampling

Our main experiment was conducted in the Busia and Samia Districts of Western Kenya from February to May, 2010. Busia is a rural area near the Ugandan border with an estimated population of about 44,000 (Central Bureau of Statistics, 2001).

In our sample, parents reported that 23% of their children had a worm infection in the previous year (Table 1). This is due in part to the fact that shoe ownership is so low: parents report that only 17% of children own shoes, and an even smaller proportion of children were actually observed to be wearing shoes by an enumerator during home visits (13%).

There are several types of shoes available in Western Kenya. The most expensive are dress shoes, which cost about 750 Kenyan shillings (us\$10) per pair.¹² These types of shoes are typically worn by adults or by children on more formal occasions, such as going to church. They are far less likely to be worn around the home by children, where worm infection is probably most likely to take place. A more common type of shoe is open-toed rubber shoes (flip-flops or rubber sandals), which are less expensive, costing about 85 Kenyan shillings (us\$1.13) per pair at retail prices, and which are more likely to be worn around the home. For this reason, we focused on the latter product for this study.¹³ While open-toed shoes as these might be less effective than dress shoes, if worn regularly they should presumably limit infection through skin contact with the soil or when using the latrine.

To obtain as representative a sample of households as possible, a door-to-door census was conducted with 1547 households in two villages located roughly 11 km apart (Ikonzon and Bhukulungu). The census collected basic information, including whether the household had a male or female head, the number of children in the household, and the GPS location of the household. With this data, we created 51 geographic clusters based on the GPS coordinates, and randomly selected 1069 households for project participation, stratified by geographic cluster. We were able to interview 999 of these (93.4%).¹⁴

⁸ It is also possible that demand for any particular health product is muted because households face competing risks. If so, demand might be increased by complementary health investments.

⁹ In a study conducted in the Rongo district of Western Kenya, Riesel et al. (2010) found that 30% of children between the ages of 2 and 18 were infected with hookworm alone, and 68% were infected with at least one hookworm, roundworm, or whipworm parasite. Similarly, Pullan et al. (2011) estimate that 54% of the population of Busia (the district of study) resides in an STH hyperendemic area, while the combined STH (i.e., hookworm, roundworm, and whipworm) prevalence rate across Western province is 80.7%.

¹⁰ See Hotez et al. (2007) for a comprehensive discussion of the current medical literature.

¹¹ Conceivably, households could instead avoid worms by treating their children with albendazole, which costs about US \$0.70 at local pharmacies and which needs to be taken every 6 months. Since shoes cost about \$1.13, and assuming shoes last 1 year, then the shoes would have to prevent 80% as many worm infections as albendazole to be cost-effective solely on health costs. Note, however, that this is a conservative estimate since shoes have other benefits.

¹² The exchange rate was roughly 75 Ksh to \$1 during the sample period.

¹³ The shoes were too small to be worn by adults and therefore could only be used by children.

¹⁴ Appendix Table A1 shows attrition by treatment status. As can be seen, attrition appears to be orthogonal to treatment.

Table 1
Randomization check.

	(1)	(2) (3) (4)			(5)
		Coefficients of OLS regression of given treatment on dependent variable			p-Value for test of joint significance of
	Overall mean	Script	Male sampled for interview	Experimental payout	All price dummies
<i>Panel A. Household level variables</i>					
Dual-headed household	0.81 (0.39)	0.00 (0.02)	−0.03 (0.02)	−0.09 (0.06)	0.16
Number of children	3.52 (1.74)	−0.03 (0.11)	−0.03 (0.02)	0.20 (0.26)	0.87
Average child health (1–5 scale) ^a	2.53 (0.60)	0.03 (0.04)	−0.05 (0.04)	0.07 (0.09)	0.97
Percentage of children with worms in past year	0.23 (0.19)	0.00 (0.01)	−0.01 (0.01)	0.00 (0.03)	0.63
Proportion of children owning shoes	0.17 (0.19)	−0.01 (0.01)	0.02 (0.01)	−0.06 (0.03)**	0.25
Percentage of children at interview wearing shoes	0.13 (0.44)	0.04 (0.03)	0.02 (0.03)	−0.12 (0.07)	0.44
Proportion of children who do not always use latrine/bathroom	0.92 (0.25)	0.02 (0.02)	0.00 (0.02)	0.00 (0.04)*	0.97
Value of animals and durable goods owned (in 1000 Ksh)	22.88 (17.95)	0.49 (1.22)	0.53 (1.22)	0.18 (2.85)***	0.59
Total household income in past week	893.54 (1281.58)	−39.51 (98.22)	−85.85 (97.92)	20.57 (245.21)	0.87
<i>Panel B. Individual level variables</i>					
Gender (1 = male)	0.28 (0.45)	−0.04 (0.02)	–	0.07 (0.06)	0.42
Years education	5.60 (3.80)	−0.07 (0.24)	–	−0.31 (0.57)	0.74
Literate (Swahili)	0.67 (0.47)	0.00 (0.03)	–	−0.03 (0.07)	0.77
Age	39.34 (14.57)	−0.61 (0.93)	–	3.45 (2.18)	0.43
Occupation = farmer	0.54 (0.50)	−0.01 (0.03)	–	−0.07 (0.08)	0.53
Self-reported health status (1–5 scale)	2.43 (0.70)	0.01 (0.04)	–	0.22 (0.11)**	1.00
Percentage of adults at interview wearing shoes ^b	0.34 (0.46)	−0.02 (0.03)	–	0.03 (0.07)	0.47
Had worms in past year	0.26 (0.44)	−0.02 (0.03)	–	0.00 (0.08)	0.40
Amount invested (out of 100 Ksh) in risky asset	52.47 (20.71)	1.77 (1.34)	–	–	0.41
Somewhat patient	0.08 (0.27)	0.00 (0.02)	–	–	0.62
Number of observations	999				

Notes: In Column 1, the overall sample mean is reported (with the standard deviation in parentheses). Columns 2–5 report results from a regression of the given dependent variable on price dummies, an indicator for whether the household was sampled for the script, an indicator for whether the male was sampled to participate, and the experimental cash payout. Columns 2–4 report coefficients (standard errors in parentheses), while Column 5 reports the p-values for the test of joint insignificance of all the price dummies. The table is broken into panels for household and individual level variables because individual means would be expected to differ between men and women. The coefficient on the experimental payout is not included in the regressions for time/risk preferences as the payment is not orthogonal to those (and they are included in controls in all regressions – see text). See text for definitions of risk/time variables. Exchange rate was about 75 Ksh to \$1 US during this time period.

^a One is “very good” and 5 is “very poor.”

^b This variable is listed as an individual level variable since men are more likely to wear shoes than women.

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

2.3. Experimental treatments

We implemented four main experimental treatments, all cross-cut against each other (see Appendix Fig. A1 for the experimental design and Appendix Table A2 for the sample sizes in each treatment cell). All treatments were conducted after administering a baseline survey (discussed below), and obtaining informed consent. First, we estimate an experimental demand curve by implementing a methodology based on Ashraf et al. (2010), Cohen and Dupas (2010), Dupas (2009), and Kremer and Miguel (2007). In particular, we visited households and provided them with a coupon offering a random discount on the shoes. The market price at the time was about 85 Ksh (\$1.13), and we provided households with coupons for 5, 15, 25, 35, 55, or 65 Ksh. Coupons were valid for a period of about 2 months.

Second, to measure the impact of information on health investment, we randomly selected half of the households to receive an information script on the symptoms of worms, transmission pathways, and on several strategies to prevent infection, including wearing shoes, using pit latrines, and hygiene.¹⁵ In addition to its emphasis on prevention, the script also stressed the dangers of untreated hookworm infection in children (anemia in particular) and the accompanying issues of growth retardation and delayed cognitive development. We used a script, rather than a more involved educational seminar, because results from our earlier studies in Guatemala, India, and Uganda suggested that a script

¹⁵ The script can be found on the authors' websites.

had similar impacts to a seminar (as we will discuss later). In addition, using the script made it easier to randomize at the individual level.

Third, to measure the role of liquidity, we provided households with randomly varying cash payments. As part of our baseline survey, we elicited risk and time preferences for all households using standard laboratory techniques. For the risk preference questions, households were given a series of choices in which they could decide how much to invest (out of 40 Ksh or 100 Ksh) in an asset which paid out three times the amount invested with probability 0.5 and nothing with probability 0.5. For the time preference questions, households were given the option of accepting 40 Ksh immediately or a larger amount in the future.

To incentivize truth-telling, we implemented a Becker–Degroot–Marschak elicitation mechanism in which one of the questions was randomly selected for payout. Though all questions had a positive probability of being picked, the odds of it being a lower-stake question were higher. This was implemented by picking a numbered piece of paper out of a bag, which indicated the question that would be paid out. If a risk question was picked, a colored ball was drawn to determine whether the amount invested would be tripled or lost and the respondent was given his money. If a time preference question was picked and the respondent chose to take the smaller amount in the present, he was paid immediately. If he chose to take the larger amount in the future, he was revisited later to receive the payout. To minimize the need to revisit households and to maximize the chance that households got cash immediately, the odds of picking a risk preference question were higher.¹⁶ Our empirical strategy is based on the fact that, conditional on risk and time preferences, the cash payout is random. We can therefore use variation in the amount paid out to estimate the effect of liquidity on purchase (while controlling for risk and time preferences).

Fourth, to measure whether there are differences within the household in the willingness to invest in health technology in households with both a female and a male head, we randomly selected either the husband or the wife for the intervention.¹⁷

Finally, we can causally estimate externalities using the experimental variation in the treatments. For any given pre-existing social group, it is random how many people in that group received the script, lower-priced coupons, or any of the other experimental treatments. Thus, by random chance, the intensity of treatment varies within any social network. We have two measures of social networks. The first is geography (as measured by GPS location), as in Dupas (2013). Furthermore, to create additional variation, we stratified intensity by geographic cluster (so that the variance across clusters exceeds that obtainable by random chance). Second, at baseline, we asked people to identify the three people whom they spoke with most often, and matched these names to their assigned treatment group (if they were in the study area). Of the 966 people willing to give the name of at least one contact, we matched 68.9% of the named contacts to our census list. We therefore estimate network effects by comparing the probability of purchase across people with randomly varying treatment intensities among their friends or neighbors.

3. Data

There are three main pieces of data that we use to evaluate the program. First, at baseline, we administered a background survey to all sampled households. In addition to standard demographic questions, we collected information on child health, worm exposure, and shoe ownership. We also collected information on household knowledge of

¹⁶ Ultimately, only 1% of households were selected for a time preference question for which they elected to wait – the remaining 99% received cash immediately.

¹⁷ If both spouses were present, enumerators would ask to interview the sampled respondent privately. However, if the other spouse insisted on listening to the survey, he was not prevented from doing so. This occurred 14.5% of the time. As might be expected, we find some evidence that such households were less responsive to the gender treatment, though the difference is statistically insignificant. It is likely, however, that the spouse being present attenuates the gender differences.

worms, transmission pathways, and prevention strategies at the end of the survey.¹⁸ As this was collected *after* reading the script for those sampled for it, this allows us to test treatment–control differences in knowledge at the time of administration. We use the follow-up survey discussed below to measure retention over the project period.

After the survey, we paid households their random cash payout and gave them a coupon which could be redeemed at a local shop for the price indicated. The shops were located in market centers that households would typically visit regularly for shopping (approximately 1.5 km away from the average household). The coupon was pre-printed with the household's ID number on it, so that any redeemed coupon could be matched to our household data. We hired an enumerator to supervise the redemption and maintain a log containing the name of the person redeeming the coupon, the number of coupons redeemed at one time, and the sizes of the shoes purchased. We rely on these administrative records from the shop to examine purchase decisions.

Lastly, we conducted a follow-up survey with 379 randomly selected households once the redemption period had ended (about 3 months after the first coupons had been given out).¹⁹ The follow-up survey included questions on shoe usage, as well as the same module used to measure worm knowledge in the baseline. In addition, after some qualitative piloting, we added in a number of questions about why people redeemed their coupons, which we use to support our main empirical findings.

4. Empirical methodology

Since all treatments were randomized, we can obtain an unbiased effect of each of the main experimental effects on the purchase of shoes with the following specification:

$$Y_i = \sum_{j=5}^{65} \beta_j P_{ij} + \gamma S_i + \delta C_i + \mu G_i + X_i' \phi + \varepsilon_i \quad (1)$$

where $P_{ij} = 1$ if household i received a coupon at price j , $S_i = 1$ if the household received the script, C_i is the cash payout the household received, and $G_i = 1$ if the husband was sampled to receive the coupon. We estimate the coefficients on the price, script, and cash payout for the entire sample. However, to estimate differences in redemption between husbands and wives (μ) we run separate regression for dual-headed households (81% of households are dual-headed). Finally, X_i is a minimal vector of controls consisting only of controls for risk and time preferences (since the experimental payout is only random conditional on risk and time preferences), and dummies for the stratification clusters (which we include to improve precision, as discussed in Bruhn and McKenzie (2009)).

In dual-headed households, we were not always able to enroll the sampled respondent in the project and so instead enrolled the spouse (mostly because the male was the selected spouse but he worked away from home during the day). We therefore also present an Instrumental Variable specification in which we instrument the male participating in the project with being sampled to participate.

¹⁸ Note that the worm knowledge questions were asked in an open-ended way (that is, the codes were not read out).

¹⁹ Participation in the follow-up appears to be orthogonal to treatment in the full sample (see Appendix Table A1, Column 3). Within the subsample of married households (Column 4), however, households receiving a price of 25 or 35 Ksh are somewhat overrepresented, while households where the husband was sampled for treatment are somewhat less represented. The differences are small, however, and are unlikely to affect much our follow-up results since we use that survey primarily to calculate averages rather than to compute treatment effects. The only treatment effect we use the follow-up for is the effect of the script on knowledge, and the script is not correlated with appearing in the follow-up.

Finally, as discussed in the previous section we can estimate geographical spillovers with the following regression:

$$Y_i = \sum_{j=1}^J \beta_j T_{ijr} + \gamma N_{ir} + X'_i \phi + \varepsilon_i \quad (2)$$

where T_{ijr} is the proportion of people within a radius r from person i who received the given treatment T . There are J such treatments (the prices, script, cash payout, and gender treatments). Here X_i includes the same controls as the previous regression, but also includes all the individual level treatments. We also include the total number of people in the cluster (N_{ir}) in order to account for possible scale effects.

We can perform a similar regression for self-identified baseline contacts. While the total number of contacts named is not random, nor is the number of contacts who could be matched (as those who could not be matched are likely to be outside the study area), conditional on the number of contacts that were matched, the number receiving any treatment is random. Thus the following regression can be implemented:

$$Y_i = \sum_{j=1}^J \beta_j T_{ij} + \gamma C_i + X'_i \phi + \varepsilon_i \quad (3)$$

where T_{ij} is the proportion of contacts who are treated, and C_i is the total number of contacts.

5. Results

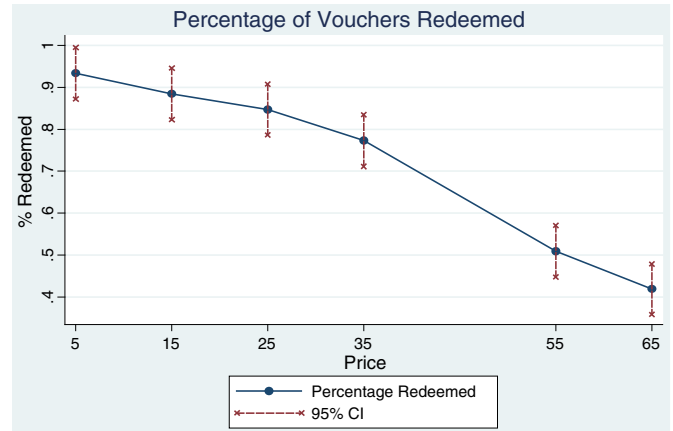
5.1. Background statistics and randomization check

Background statistics for the sample are presented in Table 1. For each variable, Column 1 presents the sample mean, while the remaining columns test whether the treatments are orthogonal to that variable. To do this, we regress each variable on indicators for all the experimental prices, the script and gender treatments, and the amount won in payments from the experimental games. As the experimental payments are only random conditional on risk/time preferences, we include those in the regressions as controls (for all variables but the risk/time preferences themselves). We split the table into household (Panel A) and individual (Panel B) variables, as we expect the individual variables to differ between men and women (one of the experimental treatments).

From Panel A, Column 1, 81% of households are dual-headed, and the average household has 3.5 children. As mentioned previously, health problems are common – respondents reported that 23% of their children had worms in the past year, and the average child's health is rated 2.53 on a scale of 1–5 (where 1 is “very good” and 5 is “very poor”). Shoe ownership is low (parents report that 17% of their children have shoes, though this may be an overestimate as only 13% of those present were wearing shoes at the interview), and it is common for children to use the “bush” instead of a latrine – 92% of children do this at least occasionally. This is consistent with low average income – total household income is just 900 Ksh (US \$12) per week, putting these households around the \$1 per day level.

Turning to Panel B, only 28% of respondents are men. This is because we sampled the male for the interview in only half of the households, and we were only able to successfully interview 61% of those men (the remainder lived away from the home most of the time or were away from home during our interviews). In addition, there are very few unmarried men – the vast majority of single-headed households are widowed females. The average respondent is 39 years old and has 5.6 years of education, and 67% of the sample is fluent in Kiswahili. Shoe ownership is low and worms are prevalent among adults as well: 34% own shoes and 26% report having worms in the past year.

Turning to the randomization check in Columns 2–5, we find very few differences between treatment groups. The experimental payout is negatively correlated with child shoe ownership and positively correlated with



Notes: 95% Confidence Intervals in parentheses.

Fig. 1. Demand for shoes. Notes: 95% confidence intervals in parentheses.

adult health status, but coefficients on script, gender, and price treatments are all insignificant for all variables. We conclude from Table 1 that treatment is orthogonal to baseline characteristics.

5.2. Experimental treatments

5.2.1. Estimating demand for children's shoes

Our first main result is to estimate the demand curve for children's shoes. We present this graphically in Fig. 1 (without any controls) and in the first column of Table 2. As can be seen, demand falls off relatively quickly with the price. While 93% of households buy when the price is 5 Ksh, only 77% buy at 35 Ksh, 51% at 55 Ksh, and 42% at 65 Ksh. As the market price is around 85 Ksh, these results suggest that the majority of people value shoes at a price lower than the market price. Note that although we never provided coupons to receive the shoes for free, the fact that 93% redeem at 5 Ksh suggests that there is not a discontinuity in demand at 0 Ksh.

It is important to note that these redemption figures are upper bounds on true demand at these prices. This is because the other experimental treatments, in particular the cash payout, tended to increase demand (as we will show later). It is also possible that the program tended to create a general excitement for the product, which increased demand at all prices. In addition, since households talked with others about the prices they received for their coupons (as we will show in the spillover effects section), it is possible that people were particularly price sensitive because they were aware of whether they got a favorable price or not. Finally, since the vouchers had an expiration date, it is possible that they encouraged some people to redeem who would otherwise have procrastinated (note, however, that to the extent a deadline is important, policymakers could certainly implement time-limited coupons).

Even without any other controls, the R-squared in Column 1 shows that 78% of the variation in the purchase decision can be explained by the price alone. This finding is in line with other recent studies which show that demand is very price sensitive in developing countries.²⁰

²⁰ It is possible that households sold the shoes (or the coupon) after purchasing them. However, the data suggests that this is unlikely: at follow-up, the enumerator asked to see the shoes and record their condition. Ninety percent of households that purchased shoes could produce them, and most of the remainder reported that the child was away from the home and is wearing them. Thus, an extreme upper bound on resale would be 10%. Of the households who could produce the shoes, 94% of the shoes appeared used, which suggests that most coupons were redeemed and used by the sampled household (especially given that it would be hard to produce another pair of shoes not acquired through the program, since baseline ownership is so low).

Table 2
Experimental treatments.

	(1)	(2)	(3)	(4)	(5)
Dependent variable = 1 if purchased shoes					
Price = 5 Ksh	0.93 (0.03)***	0.98 (0.09)***	1.05 (0.42)**	1.08 (0.27)***	1.11 (0.27)***
Price = 15 Ksh	0.88 (0.03)***	0.94 (0.09)***	1.01 (0.41)**	1.01 (0.27)***	1.05 (0.27)***
Price = 25 Ksh	0.85 (0.03)***	0.91 (0.09)***	0.96 (0.41)**	0.96 (0.26)***	1.00 (0.27)***
Price = 35 Ksh	0.77 (0.03)***	0.84 (0.09)***	0.89 (0.41)**	0.91 (0.26)***	0.94 (0.27)***
Price = 55 Ksh	0.51 (0.03)***	0.59 (0.09)***	0.64 (0.41)	0.67 (0.26)**	0.70 (0.27)***
Price = 65 Ksh	0.42 (0.03)***	0.49 (0.09)***	0.54 (0.41)	0.57 (0.27)**	0.60 (0.27)***
Received script			-0.02 (0.03)	-0.02 (0.03)	-0.02 (0.03)
Experimental payout (in 100 Ksh) ^a			0.22 (0.06)***	0.23 (0.07)***	0.23 (0.07)***
Male sampled to receive coupon				-0.05 (0.03)*	
Male actually received coupon ^b					-0.09 (0.05)*
Sample	Full sample		Married couples only		
Estimation	OLS	OLS	OLS	OLS	IV
Cluster dummies	N	Y	Y	Y	Y
Observations	999	999	999	812	812
R-squared	0.78	0.79	0.80	0.79	-

Notes: Experimental payout is in 100 Ksh. Exchange rate roughly 75 Ksh to US \$1 during this time period. 3 of 999 households are missing information on the experimental payout. To avoid dropping these, we code them as 0 and include dummies for having a missing value (so that the coefficients are relevant only for those with non-missing values). Clusters were calculated from GPS coordinates and sampling was stratified at that level. Standard errors are in parentheses.

^a Values of cash payout: mean – 35 Ksh, median – 30 Ksh, minimum – 0 Ksh, maximum – 200 Ksh.

^b The regression in Column 5 is an IV treatment-on-the-treated (TOT) regression, where an indicator for the male being interviewed is instrumented with whether the male was sampled for the interview. See Appendix Table A2 for the first stage regression.

* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

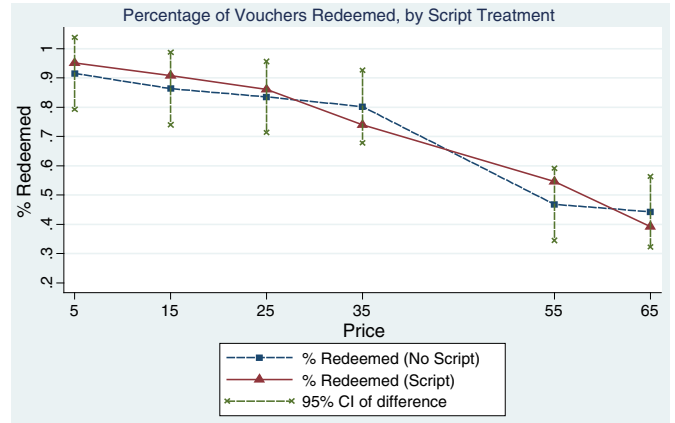
The primacy of price in this setting is particularly striking in that households had to go to a nearby shop to redeem the coupon, and most did not redeem until a few days later – thus, households had to hold the money for a few days, and would have had many chances to spend the money on something else.

5.2.2. Effects of interventions on demand

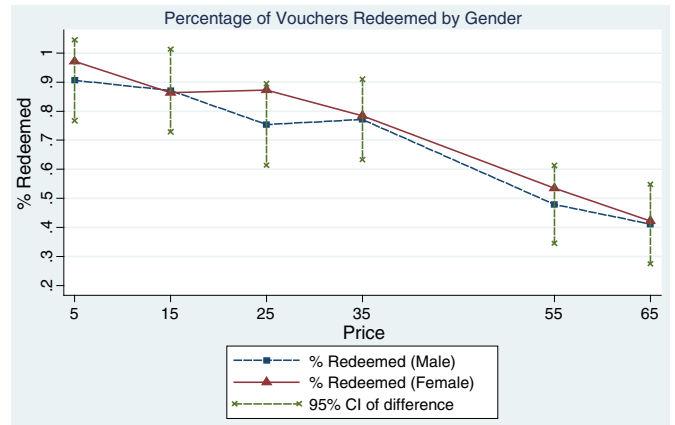
In Fig. 2, we plot demand curves for various treatment groups (note that results in this figure are not regression adjusted with controls). Panel A shows how demand varies with the script payment. We find that there is no discernible effect from the script. The two demand curves lie virtually on top of one another, crossing each other three times. Table 2, Column 3 confirms these basic results in a regression framework with controls. Note that the standard errors are relatively small, so that we can confidently rule out large positive effects of the script.

Panel B shows how demand differs by the gender of the respondent. Since gender is only randomly determined for those households which have two heads, this regression is restricted to the 81% of households which are dual headed. Though the standard errors are relatively large (especially when comparing demand at each particular price), there is evidence that women are more likely to redeem coupons than men. Note that these are Intent-to-Treat comparisons – the gender differences are bigger for those households in which we were able to successfully interview the sampled spouse.

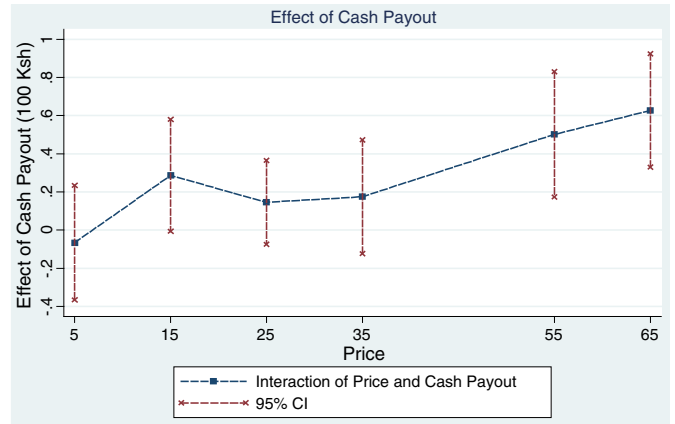
A. Information



B. Parental Gender



C. Liquidity



Notes: The confidence intervals reported are of the difference between the given experimental groups. All figures are Intent-to-Treat estimates.

Fig. 2. Experimental treatments. Notes: The confidence intervals reported are of the difference between the given experimental groups. All figures are Intent-to-Treat estimates.

To measure gender difference quantitatively, we include a treatment indicator for the male being sampled in Table 2, Column 4. This regression is restricted to dual-headed households. We find that when the male is offered the coupon, the household is 5 percentage points less likely to buy shoes (significant at 10%). Since we were not able to treat all the men sampled for the interview, we run an IV specification in Column 5 and find a 9 percentage

point effect, again significant at 10% (see Appendix Table A3 for the first stage).²¹

Fig. 2, Panel C shows the effect of the cash payout.²² For simplicity, this graph does not control for risk/time preferences (the results look very similar with those controls). As the payout is continuous, we graph the γ_j coefficients from the following regression

$$Y_i = \sum_{P_j=5}^{65} (\beta_j P_{ij} + \gamma_j P_{ij} * R_i) + \varepsilon_i \quad (4)$$

where R_i is the cash payout (in 100 s of Kenyan shillings).

The mean and median cash payout were 35 Ksh and 30 Ksh, respectively. They thus represent a very small fraction of weekly income (which from Table 1 is around 900 Ksh per week), and an even smaller fraction of total households wealth (again from Table 1, the average household owns 23,000 Ksh worth of animals and durable goods). Thus, if these payouts affect demand, they work through a cash-on-hand effect, rather than because they have any effect on total income. We find a strong evidence of a liquidity effect, particularly at intermediate or high prices (the smaller effect at lower prices is because demand is already so high to begin with). Table 2, Column 3 shows the regression results with controls for risk/time preferences. On average, every additional 100 Ksh in randomized cash payout increases the probability of purchase by 22 percentage points. Although we do not present regressions with interactions with price, the basic results are robust to the inclusion of controls (i.e., the cash payout only matters at moderate or high prices).

One possible concern with the cash payout would be if people felt that they were expected to spend the money on shoes. We view this as unlikely for several reasons. First, they had to redeem the coupon at the shop on their own, after the field officer had left. Second, during the survey, respondents were explicitly informed that the cash payouts were as compensation for their time, not to help them buy the product. Third, consistent with other studies, redemption fell off rapidly as the price increased, suggesting that households did not feel impelled to purchase the product at even moderate prices.

5.3. The script and worm knowledge

One important finding thus far is that the worm education script has no effect on the purchase decision. Is this because the script was ineffective in conveying knowledge? To address this question, we gave a nine-question worm quiz (at both baseline and follow-up) to test the effect of the script on knowledge about worms and worm prevention. In Table 3, we regress the percentage of questions answered correctly on the script treatment. We find large effects: in the baseline, respondents who were given the script scored 34 percentage points higher on the quiz (on a low base of just 29% in the control group). In the follow-up survey, the difference was somewhat smaller but still highly significant: a 24 percentage point difference, against a base of 37%. Thus, health knowledge did indeed increase, but the increase in knowledge did not increase the demand for the shoes.²³

²¹ Though not shown in the table, we do not observe any interaction between gender and receiving the information script (results on request).

²² We do not have accurate records of experimental payouts for 3 of the 999 households. To avoid losing these households in the regressions (such that direct comparison across columns is possible), we create a dummy for having missing information for this variable and then code their cash payout as zero. Thus the experimental payout coefficient is identified off of only those for whom we have non-missing data. We omit the dummy for “missing experimental payout information” for space.

²³ A possible confounding factor for why information does not lead to purchase is that shoes have other non-health benefits, and people buy shoes only for these other reasons. However, we have two pieces of evidence to suggest this is not the case. First, when asked, a significant portion of people report health as an advantage of wearing shoes. Second and more robustly, we also find no evidence of information effects in the smaller studies we conducted in Guatemala, India, and Uganda which focused on products with minimal non-health benefits (soap and multivitamins). These results are discussed in more detail in Section 7.

Table 3
Worm knowledge.

	(1)	(2)
<i>Panel A. Immediately after getting script</i>		
Read script	0.34 (0.01)***	0.34 (0.01)***
Extended controls	N	Y
Observations	989	989*
R-squared	0.54	0.91**
Mean in control group	0.29	0.29
<i>Panel B. Three to four months later</i>		
Read script	0.24 (0.02)***	0.24 (0.02)***
Extended controls	N	Y
Observations	377	377
R-squared	0.40	0.92
Mean in control group	0.37	0.37

Regressions in Column 2 control for all experimental treatments and the risk/time preferences. Some values of the experimental payout and gender of the respondent were missing. To avoid dropping these, we code them as 0 and include dummies for having a missing value (so that the coefficients are relevant only for those with non-missing values).

Standard errors in parentheses.

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

5.4. Peer effects

There were strong reasons to expect that there might be spillover effects in our experiment. Several studies in agriculture find large spillover effects (e.g., Conley and Udry, 2010; Foster and Rosenzweig, 1995). Health-specific studies also tend to find spillover effects, though these effects can either serve to increase adoption (e.g., bed nets in Kenya in Dupas, 2013, menstrual cups in Nepal in Oster and Thornton, 2012) or decrease it (e.g., deworming drugs in Kenya in Kremer and Miguel, 2007), depending on relative costs and benefits, as well as health externalities.

There are three possible channels through which spillovers may occur in the context of this experiment. First, people may talk to each other about the health benefits of the shoes from information they got from the script. Second, if shoes are an experience good, people who were exogenously induced to buy shoes because they received a low-priced coupon may learn from using and may then discuss benefits with contacts. Third, exogenously increasing usage among friends and neighbors might stimulate demand through an imitative channel, since shoes are readily observable and carry a certain prestige value.

Despite expectations to the contrary, we find very little evidence of imitative peer effects in the purchase decision (either positive or negative) for either geographical neighbors or health contacts (Table 4). First, Panel A presents geographical spillovers across different radii (300, 500, and 1000 m). For each specification, we report the percentage of contacts receiving the script, the percentage receiving a low-priced coupon (which we define as less than 35 Ksh here),²⁴ and the total number of people living within the radii.²⁵ Although many of the point estimates are positive, none are significant and many are quite small (given that the independent variable is measured as a percentage). Note, however, that the confidence intervals on these estimates are wide, and include fairly large values. Panel B does the same for the matched informational contacts and, again, there is no discernible positive effect, even though the confidence intervals are much tighter than

²⁴ The results are not sensitive to defining this variable differently.

²⁵ Due to the geographic stratification, there is more variation in the price than in the script. The percentage of neighbors getting a price lower than 35 Ksh has a mean of 0.67 with a standard deviation of 0.17, while the percentage of neighbors receiving the script has a mean of 0.51 and a standard deviation of 0.11.

Table 4
Testing for spillover effects in redemption.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: redeemed coupon						
<i>Panel A. Geographical neighbors</i>						
<i>Within 300 m of household</i>						
# of neighbors	0.001	0.001				
	(0.001)	(0.001)				
% of Neighbors getting price less than 35 Ksh	0.073					
	(0.080)					
% of neighbors getting script		0.057				
		(0.117)				
<i>Within 500 m of household</i>						
# of neighbors			0.000	0.000		
			(0.001)	(0.001)		
% of neighbors getting price less than 35 Ksh			0.020			
			(0.109)			
% of neighbors getting script				0.295		
				(0.209)		
<i>Within 1000 m of household</i>						
# of neighbors					0.000	0.000
					(0.001)	(0.001)
% of neighbors getting price less than 35 Ksh					0.037	
					(0.150)***	
% of neighbors getting script						0.058
						(0.657)**
Observations	997	997	998	998	999	999
<i>Panel B. Health contacts</i>						
# of contacts in experiment	0.022	0.022				
	(0.017)	(0.017)				
% of contacts getting price less than 35 Ksh	—					
	0.015					
	(0.039)					
% of contacts getting script		−0.07				
		(0.038)*				
Observations	868	868				

Notes: regression control for all experimental treatments and include geographical (cluster) controls. Standard errors are clustered at that level. Standard errors are in parentheses.

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

those for geographical neighbors (if anything, the effect for the script is negative, though small and significant at only 10%).

Why don't we observe any spillovers (either positive or negative)? One important reason is that the scope for social learning (either through the script or through experience with the product) might be limited in this case. In particular, since information did not have a first-order effect on the individual who received it, it is unlikely that we would find second-order informational conformity across households.²⁶ Furthermore, while it might be possible to learn about the protective benefits of shoes from wearing them, this process might be very noisy, making it difficult to extract a signal. Shoes have existed for a long time and the benefit of wearing the shoes is not immediately obvious (as is more apparent with brand new technologies with which people are unfamiliar). It also might be possible that it takes time for information to diffuse generally, and there was not enough time for information to travel in the relatively short period between the baseline and follow-up surveys. A study conducted over a longer time horizon may be able to identify some of these delayed peer effects.

Thus, unlike most studies of spillover effects in development (which focus on social learning), the main role for externalities in this experiment is likely through imitative peer effects. We might expect these in our study because shoes are often desirable for children, and seeing

other children wearing shoes may increase the demand for children to have and wear them. There is considerable evidence that such effects can be relevant in human capital (e.g., Borjas, 1995; Sacerdote, 2001). Munshi and Myaux (2006) provide one of the few pieces of evidence for pure imitative effects for a health product (contraceptives) in a developing country.

To shed more light on why we find no imitation, we present responses to the follow-up debriefing survey we conducted at the end of the project in Table 5. Interestingly, the Table shows that the lack of spillover effects is not because people do not place value on imitation or relative wealth. From Panel A, the majority of people report that seeing their peers' children with the shoes made them (and their children) want them more. Most people also report that they would feel poor if the neighbors bought the shoes and they did not, and they also report that they would value certain prestige items more if their neighbors had them (such as a TV). Furthermore, Panel B shows that people do report talking about the program with others: 72.5% of people who received the script reported talking to somebody else about the health effects of worms and 51.7% of people who didn't receive the script reported hearing from others about worms.

Why then didn't people who saw many of their neighbors purchase shoes buy them themselves? We asked those parents who purchased shoes what factor was most important in redeeming the coupon. Forty-two percent reported that the price was the biggest factor and 33% reported health (including worms and other injuries). However, only 10.8% reported responding to pressure from children and 7.5% reported some influence of neighbors. Thus while "keeping up with the Joneses" may well be an important consideration for people, the primary barrier is clearly price.

Moreover, social interactions were actually so strong that they might have mitigated the desire to imitate. From Panel B, 78.9% of people report that their neighbors knew what priced coupon they got. In Panel C, we asked people if they would be seen as a "bad parent" if they did not buy the shoes at different prices. While 77.9% of parents report that they would be seen as a bad parent if they didn't buy the shoes at a low price, only 50.3% reported that they would be if they didn't buy

Table 5
Mechanisms.

	(1)
<i>Panel A. Imitative peer effects</i>	
Were slippers popular among children?	0.926
Did your child ask you to buy slippers?	0.689
Did your child want them more because other children had them?	0.753
Did you want them more because other children had them?	0.705
Would you feel poorer if the neighbor's children had slippers but yours didn't?	0.699
Would your neighbor buying a TV make you want to buy one more?	0.532
Would you feel poorer if the neighbors had TV and you didn't?	0.629
<i>Panel B. Social interactions</i>	
Did you talk to anybody else who had received a voucher?	0.799
Did neighbors know what price coupon you got?	0.789
If received script: did you talk to others about worms?	0.725
If didn't receive script: did anybody talk to you about worms?	0.517
<i>Panel C. Reasons for purchasing</i>	
Would other parents think you were a bad parent if you didn't redeem at low price?	0.779
Would other parents think you were a bad parent if you didn't redeem at high price?	0.503
<i>For those who redeemed, what is the main reason you redeemed?</i>	
Price was low	0.416
Pressure from children	0.108
Neighbors	0.075
Health	0.330
Other	0.072

Notes: Means presented from follow-up interview at conclusion of project.

379 households participated in the follow-up, though the number of observations is smaller for some variables.

²⁶ We also do not find evidence of knowledge spillovers on the worm quiz (results upon request).

the shoes at a high price. Thus, imitative peer effects may have actually been mitigated by the strength of social connections.²⁷

6. External validity

In 2008 we conducted smaller-scale studies in three different countries with two products that could be used by both children and adults: hand soap and multivitamins. This trio of smaller studies took place prior to, and served as the pretext for, our larger experiment in Kenya. The scope of the smaller studies was confined to ascertaining information and price effects alone, but findings are strongly consistent with the Kenya study, indicating that the effects of these demand factors are unlikely to be limited to one geographical context or health product. The studies were carried out in the small village of Panyebar, Guatemala (pop. 2031), the town of Busia, Uganda (pop. 36,600),²⁸ and the large city of Chennai (Madras), India (pop. 4,700,000).²⁹ These experiments were more limited in scope than the Kenya study: all respondents were women, we did not conduct long interviews with households, we did not measure knowledge from the information treatments, and we did not measure peer effects. However, the general results nevertheless concur strongly with those in Kenya.

6.1. Experimental design

In each of the three country sites, our experimental protocol was designed to be very similar. Households were randomly selected to receive either hand soap or a multivitamin supplement, where the retail value of the products ranged from approximately \$2 in Guatemala and Uganda to \$1 in India. Both products could potentially have large health effects. The use of soap accompanied by regular hand washing is documented to decrease diarrhea in both children and adults in developing countries (Curtis and Cairncross, 2003; Luby et al., 2004, 2005; Ray et al., 2010). Similarly, women and children in resource-poor regions are known to suffer from nutritional deficiencies (Stein, 2010; Torheim et al., 2010), a possible solution to which may be the increased use of multivitamins and/or iron supplements in women of reproductive ages and/or children (Huffman et al., 1998; Ramakrishnan, 2004; Yip, 1994).³⁰

The only intervention in all three of these sites was an information treatment. In Guatemala, treated households were invited to a five-day public health seminar lasting an hour and a half each morning. In Uganda, households attended a one day health seminar that emphasized basic hygiene, nutritional information, and preventative healthcare through the encouragement of hand washing and proper vitamin intake. Finally, in India, treatment households were read a health script. For those offered soap, the script reminded people that hand-washing with soap and clean water at the appropriate times could prevent germs from spreading. For those offered vitamins, the script focused on the importance of broad vitamin intake for general health.

To measure the impact of information, all households were visited for an endline survey. After completing the survey, households were given cash as a gift for participating, and offered the opportunity to purchase the health product directly from the enumerator at a randomly selected discount (25%, 50%, or 75% of the retail price). This design

²⁷ Another possibility which we are not able to rule out is that spillovers extended beyond a given social network. For instance, the program might have created excitement in the entire study area.

²⁸ Busia, Uganda is located on the Ugandan side of the border with Busia, Kenya (Busia is a border district).

²⁹ Population sources: Panyebar, Guatemala: Ludwinski et al. (2011); Busia, Uganda: Uganda Bureau of Statistics (2002); Chennai, India: Census of India (2011).

³⁰ The specific products differed slightly across countries. In Guatemala, the hand soap was a three-pack of standard anti-bacterial soap bars, a product that was used among Guatemalan households, but not widespread. Soap products were similar in India, but differed slightly in Uganda, where the hand soap was a more common multi-purpose soap. In all three sites, the vitamin product was an 8-oz. bottle of multi-vitamin syrup primarily intended for children, but often used by adults. Instructions recommended once-daily use as a supplement to existing meals.

negated any issues with cash liquidity since subjects used the money from the survey payout to pay for the health products, and was done to minimize any frictions related to product purchase. Of course this came at a trade-off of the more realistic scenario in which one typically must purchase such products at a local store.

The sample sizes for the 3 studies were 350 in Guatemala (197 treatment and 153 control), 455 in India (208 treatment and 247 control), and 516 in Uganda (266 treatment and 250 control). Please see the online Appendix for additional detail on experimental protocol in the smaller sites.

6.2. Results

6.2.1. Background statistics and randomization check

Appendix Table A4 presents background statistics as well as a check on the effectiveness of our randomization in each of the three countries. While we have few covariates to compare across samples, there are some stark differences across countries. For example, average years of education is just 1.94 in Guatemala, compared to 5.7 in Uganda and 5.5 in India. As expected, there are few differences between treatment groups. Taking the prices first (Column 7), the only differences with a p-value less than 0.10 are education in Uganda and India. There are a few differences in regard to the seminar or script treatments. In Guatemala, women in the treatment group are younger; in India, women in the treatment group are less educated. Finally, women in Uganda who received coupons for soap were less educated and more likely to have access to piped water than women offered vitamins. We control for these covariates in our regressions (although omitting them makes little difference to our estimates).

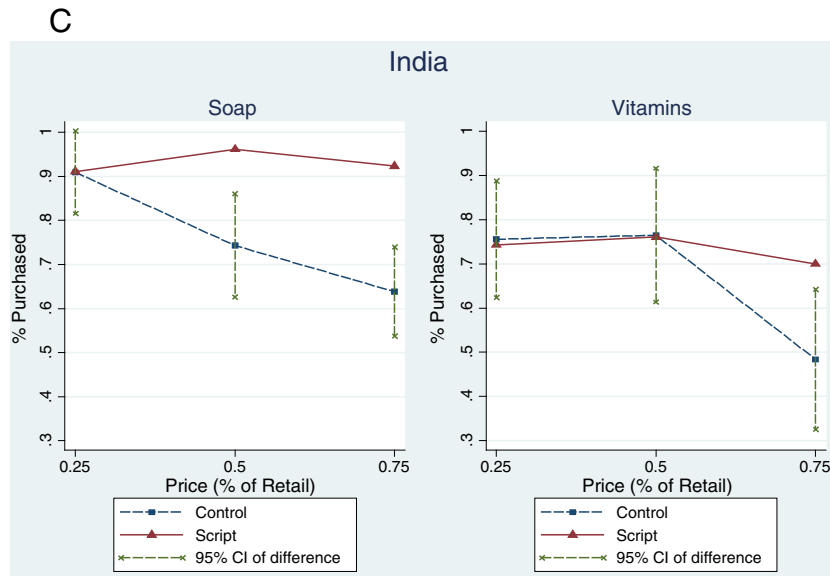
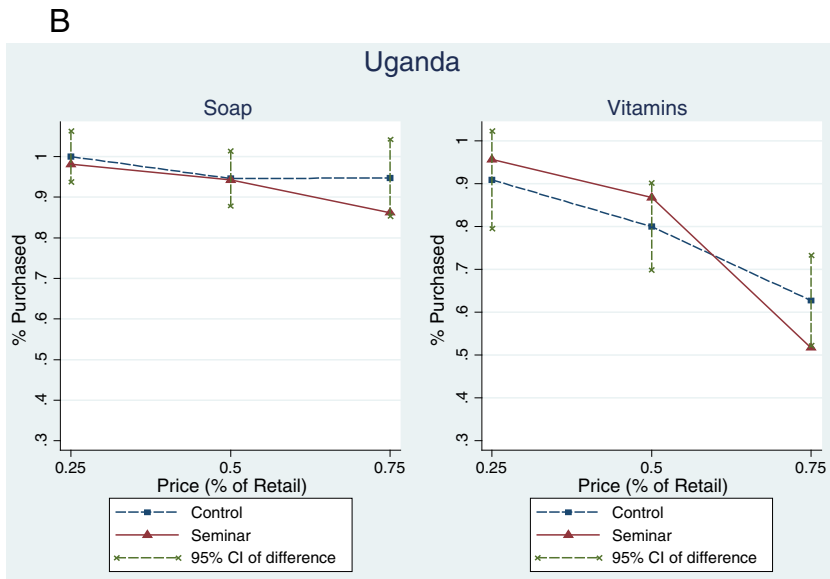
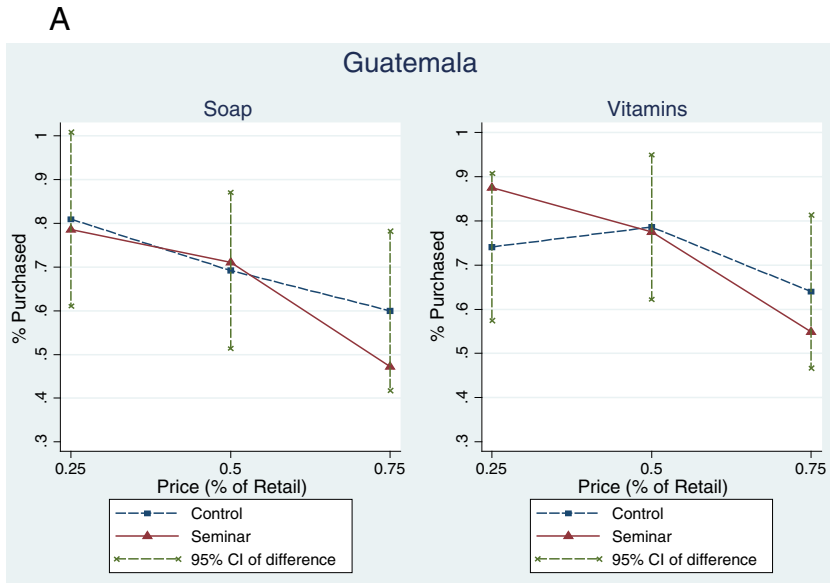
6.2.2. Experimental results

The basic experimental results are summarized in Fig. 3 and in Table 6. Each panel presents results from a different country, and each panel has two graphs which plot the treatment and control means (along with the standard error of the difference). The left graph shows soap, and the right shows vitamins.

Panels A (Guatemala) and B (Uganda) show the same pattern as Kenya: there is no discernible effect of the seminar on purchase, at any price, and demand tends to fall off rapidly for most products.³¹ This is confirmed in the regressions in Table 6: the point estimate for the health seminar is small and insignificant. Although regressions without controls are not shown, the R-squared of regressions which include only prices also vary between 0.69 and 0.95, suggesting again that price is the most important factor. Panel C (India) shows a somewhat stronger effect of the script, especially for soap (though the effect is positive for both products). Though it is hard to definitively attribute a causal factor for this difference given differences in samples, one possibility is that because subjects had cash on hand and chose whether or not to redeem the coupons right after being read the script, the script served as a “nudge” toward coupon redemption. Though the result could be due to sampling variation (since only 1 of the 6 possible product–country pairs is statistically significant), we speculate that the nudge together with the absence of transportation costs and the provision of liquidity might have encouraged purchase.

Taken together, the results from these smaller studies support the key findings of our main study in Kenya: 1) investment in health products is highly sensitive to price; and 2) information about health products has a small or negligible effects on its own, unless – perhaps – other frictions are also removed simultaneously.

³¹ The one exception is soap in Uganda. Redemption rates were high across our price treatments because the particular soap we used in the experiment was a recognized item to households and was apparently viewed as a bargain even at a 25% discount.



Notes: The confidence intervals reported are of the difference between the given experimental groups.

Fig. 3. Results from smaller-scale projects in Guatemala, Uganda, and India. Notes: The confidence intervals reported are of the difference between the given experimental groups.

Table 6

External validity: pilot experiments in Guatemala, India, and Uganda.

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable = 1 if purchased product					
	Guatemala		Uganda		India	
	Soap	Vitamins	Soap	Vitamins	Soap	Vitamins
Price = 25% retail	0.82 (0.08)***	0.80 (0.07)***	1.00 (0.03)***	0.93 (0.05)***	0.85 (0.04)***	0.73 (0.06)***
Price = 50% retail	0.73 (0.07)***	0.78 (0.07)***	0.96 (0.03)***	0.83 (0.04)***	0.78 (0.05)***	0.73 (0.06)***
Price = 75% retail	0.55 (0.07)***	0.59 (0.07)***	0.91 (0.03)***	0.58 (0.05)***	0.69 (0.05)***	0.56 (0.07)***
Invited to health seminar	-0.04 (0.07)	0.01 (0.07)	-0.03 (0.03)	0.01 (0.05)		
Read script					0.14 (0.05)***	0.05 (0.06)
Observations	174	175	233	281	234	221
R-squared	0.69	0.74	0.95	0.82	0.85	0.72

Notes: All respondents were women. Regressions include all the controls listed in Table A3.

Standard errors in parentheses

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

7. Discussion: what influences investment in preventative health?

In this section, we compare our results to those of other recent experiments on health product adoption in LDCs. We do not attempt an exhaustive review and focus solely on preventative health investments.³² We summarize our results in Table 7, where we identify the previous studies by author, country, and health product, and list results for four key outcomes: the effect of price changes, information campaigns, gender, and spillovers through peer effects.

Overall, we find a number of similarities. First, as in many other recent studies, we find that demand for our preventative health products is highly price sensitive. While it is impossible to make direct comparisons since the product and the range of subsidy varies across studies, our estimated elasticities (calculated at the mean price) fall in the -0.40 to -0.90 range. This is similar to the -0.37 mean elasticity reported in Cohen and Dupas (2010) for long-lasting insecticide-treated bed nets in Kenya and the -0.60 mean elasticity reported in Ashraf et al. (2010) for chlorine in Zambia. Our results are not as stark as those observed in the Kremer and Miguel's (2007) study of deworming drugs, in which even a modest US \$0.30 cost-recovery fee decreased demand by 80%, or in the study of Miller and Mobarak (2011) that reports an elasticity of -2.3 for improved cook stoves. Of course, differences in price sensitivity across products will be heavily influenced by how much household decision-makers expect to pay for these products, which may depend on previous availability and exposure to the product. Nevertheless our basic price results are very much in line with previous research which finds that demand for health products is very price elastic in LDCs.

Although there are relatively few randomized experiments on how information affects preventative health investment (rather than behavior change more generally), most of these studies tend to find small effects, as we do. Kremer and Miguel (2007) find no effect of education targeted at children on worm-preventing behavior. Also, since they find extreme price sensitivity for deworming drugs among the parents of children in the program, the intervention did not increase subsequent demand for the drugs among parents. Similarly, Kremer et al. (2011) find that providing information has little effect on uptake of chlorine for water. In a study similar to this one, Ashraf et al. (2013), find that

information does not affect the level of investment but does affect the slope of the demand curve. Dupas (2009) also finds that scripts to make health more salient have little effect (note however that those scripts did not convey health information).³³

One exception is Luoto et al. (2012), who carry out randomized trials in Kenya and Bangladesh on point-of-use water chlorination and filters. In both countries they implement two different marketing campaign treatments, one emphasizing only the positive health benefits of the water products, and a second strongly contrasting the positive outcomes associated with usage with the negative outcomes from non-usage. They find that the contrast-marketing message households in Kenya were about 5 percentage points more likely to have uncontaminated water in their home with insignificant results in Bangladesh. However, they find significant effects in both countries from a treatment in which subjects make a public commitment to use the product.

Thus early results suggest that simply providing information is not consistently effective in these types of investment decisions. The results in these studies may differ from some of the more positive results of health information summarized in Dupas (2011) for several reasons. First, the information provided in these studies was not very specific to the household. This differs from, for instance, the studies of Jalan and Somanathan (2008) and Madajewicz et al. (2007), in which the authors found that informing households that their own water source is contaminated (with fecal bacteria or with arsenic, respectively) induces behavior change. Such targeted information is likely to be very salient. Second, the interventions in Table 7 simply provided information during a one-time-only visit and so did not intensively attempt to change behavior or to instill learning. This is very different from studies such as Cairncross et al. (2005) or Luby et al. (2004, 2005), in which the authors found that intensive educational interventions conducted over many months were effective in improving hygiene. Third, the main outcome in the studies we summarize is whether households actually purchased the health product, which means that even if information increased the perceived value of the item, the household still had to gather enough money to buy the product. This is difficult for the very poor who do not have access to credit or to good savings products. By

³² For a much more exhaustive review (including other health decisions such as choice of water source or handwashing behavior and other methodologies), see Dupas (2011).

³³ While the script treatment in our study did not increase preventative health investment, it might have affected other behavior (for example, hygiene). Unfortunately, we do not have data to test this.

Table 7
Comparative results of some recent experimental health studies.

Study	Country	Product	Price sensitivity ^a	Information effects	Peer network effects	Gender effects
Current studies	Kenya	Shoes (rubber shoes)	$e_s = -0.405$	No effect of script	No friendship or geographical effects	Positive for mothers
	Guatemala	Hand soap and vitamins	$e_s = -0.776$ $e_v = -0.547$	No effect from health seminar	NA	NA
	India	Hand soap and vitamins	$e_s = -0.345$ $e_v = -0.407$	Significant effect of script for 1 product	NA	NA
	Uganda	Hand soap and vitamins	$e = -0.156$ $e_v = -0.883$	No effect from health seminar	NA	NA
Kremer and Miguel (2007)	Kenya	Deworming treatment ^b	Very high at $p = 0$. ^c	Education campaign (among children) had no effect	Negative peer effects on treatment uptake	NA
Ashraf, Berry, and Shapiro (2010)	Zambia	Chlorine water purification	$e = -0.60$ ^d	NA	NA	Only women
Cohen and Dupas (2010), Dupas (2009, 2013)	Kenya	Treated bed nets	$e = -0.47$ (Cohen and Dupas, 2010)	No significant effect of scripts ^e	Strong peer effects ^f	No effect
Oster and Thornton (2012)	Nepal	Menstrual cups	NA	NA	Strong peer effects	Only women
Kremer et al. (2011)	Kenya	Chlorine water purification	Very high at $p = 0$. ^g	Small effects from inform. campaign	Little evidence of peer effects	Only women
Ashraf, Jack, and Kamenica (2013)	Zambia	Chlorine water purification	$e = -0.636$. ^h	No level effect, but increases price elasticity.	NA	NA
Miller and Mobarak (2011)	Bangladesh	Improved cook stoves	$e = -2.3$	Information campaign via peers	Initial effect from respected peers	Moderately positive for wives
Luoto, Levine, Albert, and Luby (2012)	Kenya and Bangladesh	Chlorine water purification and water filters	NA	Moderate response to health marketing campaign	NA	NA

^a Column reports elasticities at mean price unless otherwise noted.

^b Albendazole and praziquantel.

^c Relative to a zero price, a \$0.30 fee cost-recovery fee decreased demand 80%.

^d About 80% of respondents bought chlorine at 300 Kw with 50% buying at 800 Kw (3200 Kw = \$1US).

^e Neither health nor financial encouragements significantly affected purchase.

^f Households who were surrounded by other households receiving a low price in a first phase (who were much more likely to purchase bed nets) were more likely to purchase them in the second phase.

^g Usage falls from 58% to 3.5% when price increases from zero to 20 Kenyan shillings and take-up is low at all positive prices.

^h Taken at means of price and take-up. When provided consumer information, magnitude of elasticity increases to -0.876 .

contrast, soap was provided as part of the intervention in Luby et al. (2004, 2005), while the main outcomes in Cairncross et al.'s (2005) study were various measures of self-reported behavior. We believe that these results have important implications for health practitioners in developing countries, where information campaigns have often played a leading role in trying to change health behaviors.

As mentioned previously, our estimates of the impact of peer effects differ greatly from other studies, almost certainly due to differences in products. The other study of which we are aware which focuses on imitative peer effects is Munshi and Myaux's (2006) study of contraceptive adoption in Bangladesh, in which women may have eschewed purchasing the contraceptives until there was evidence of their acceptability within their religious group, a case of pure conformity in which imitation may occur in the process of breaking social stigma. In the other studies (Dupas, 2013; Miguel and Kremer, 2007; Oster and Thornton, 2012) peer effects are likely to be the product of a learning process, in which households begin to understand the private and social costs and benefits of a product, an effect unlikely with a well-known product such as shoes.

Another area in which our findings relate to previous studies is in the area of gender effects. As discussed in Section 2, since there were no income transfers, the result that preventative health investment is higher when the woman receives the coupon suggests not only intra-household differences in preferences, but also constraints on information sharing within the household. A close paper in this respect is that of Dupas (2009), who randomized whether the female head, male head, or both heads jointly would receive a coupon for a bed net. While she finds no difference in take-up between the treatment in which the male gets the coupon individually and the treatment in which the female does, she finds that investment in both cases is lower than when both are given it simultaneously. Her result may also suggest some communication constraints. Miller and Mobarak (2011) also find that wives show a stronger preference for purchasing

improved cook stoves in Bangladesh, but that this preference cannot be acted upon if the price is high or if their husband's decision contradicts their own.

8. Conclusion

To conclude, we attempt to synthesize and summarize the main finding from our paper and related experiments, and to offer a few suggestions for future research.

1. *Price matters.* In keeping with numerous recent studies, we find that many low-income households fail to invest in preventative health products simply because they are unwilling to pay market prices for them. We find relatively high price elasticities in each of our four field experiments and across all three of our health products. Because of the strong positive externalities associated with preventative health products, policymakers may consider subsidies as one means of boosting adoption.
2. *Information campaigns often have limited impact on their own.* Compared to the effect of lower prices, the types of information campaigns we summarize here generally have little effect on the purchase of health products in recent experimental studies, including ours. While more intensive, long-term campaigns have been effective, more modest programs have very limited effects. In our study sites, providing information had no effect, except perhaps as a "nudge" when other constraints were simultaneously removed. More research should be done to ascertain the conditions under which health information campaigns might be more effective.
3. *Peer effects will be stronger for products which have been available for a shorter time or for which social acceptability is important.* We should expect peer effects in health product adoption when we have a strong a priori theory for their existence. This ought to be when there is strong asymmetric information about product effectiveness

between users and non-users or when bandwagon effects are socially important in the use of a product (such as contraceptives in the case of Munshi and Myaux, 2006). Otherwise, peer effects are likely to play a small role in adoption.

4. *Parental gender matters for children's health products.* Though such findings are likely very context-dependent, at least in this particular experiment, marketing health products to mothers increased investment. This appears to be driven both by differences in gender preferences and a limited flow of information within the household.
5. *Liquidity is important for health investment decisions.* Cash on hand appears to influence health product purchase. Our results suggest that this is not likely due to pure credit constraints (since even for these very poor households, the experimental payouts were small). Other work in this same part of Kenya shows that merely providing people with simple places to save greatly increases investment in preventative health (Dupas and Robinson, 2013b). Designing products to help households accumulate necessary liquidity may be a fruitful avenue for future research.

Appendix A. Supplementary data

Supplementary data to this article can be found online at <http://dx.doi.org/10.1016/j.jdeveco.2013.08.003>.

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