Keeping the Doctor Away: Experimental Evidence on Investment in Preventative Health Products

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Abstract,

Household investment in preventative health products in developing countries is typically low even though the return to such products appears to be very high. We present the results of field experiments in which we experimentally estimate demand curves for health products and test whether (1) information about health risk, (2) cash liquidity, or (3) intra-household differences in preferences affect demand. In our main experiment in Kenya involving children's shoes, which are critical for preventing worm infection, providing information through a targeted health script has no effect on purchase. While providing liquidity and targeting women affect demand, price explains the vast majority of the purchase decision. We find no evidence of spillover effects even though people talked with each other extensively about the product. We find similar results in three smaller studies in Guatemala, India, and Uganda in experiments using soap and multivitamins.

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1, Introduction,

In many developing countries, investment in preventative health products is persistently low at market prices even though the return to these products appears to be very high.[1] In a set of experimental studies involving shoes, hand soap, and vitamins, we try to shed light on this issue by evaluating several commonly-cited reasons for underinvestment in preventative 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 preventing such risks. Second, households may not purchase preventative health products because they lack liquidity. In a fully frictionless model, households who choose not to invest in health (even if the return is high) do not do so because the return to some other type of investment is even higher. However, if households face credit constraints (Tarozzi et al., 2011; Devoto et al., 2011), lack secure places to save (Dupas and Robinson, 2011a), or do not save as much as they planned to for behavioral reasons (*e.g.* Ashraf et al. 2006; Duflo et al., 2010), immediate liquidity constraints may be relevant. Third, there may be intra-household conflict in spending on health (particularly for children). Numerous studies have provided evidence that women are more likely to invest in children's health than men (*e.g.* Thomas 1990, Duflo 2001), suggesting that targeting preventative health products at female household heads may be important. Both Kremer and Holla (2008) and Dupas (2011) provide excellent and more amplified reviews of these issues.

In this paper, we overlay several interventions to the methodology of Kremer and Miguel (2007), Dupas and Cohen (2009), Dupas (2010), and Ashraf, Berry, and Shapiro (2010), who estimate experimental demand curves for health products. Our experiments take place in four countries – three smaller studies in Guatemala, India, Uganda which were conducted in 2008, and a much larger study in Kenya in 2010. In the underlying methodology, we provide vouchers

[1] A partial list of technologies with high returns includes chlorine for water (Fewtrell et al. 2005; Arnold and Colford 2007), deworming drugs (Miguel and Kremer, 2004), insecticide-treated bed nets (Lengeler, 2004), and iron supplementation (Thomas et al., 2006; Bobonis et al. 2006). Many recent studies demonstrate strong price sensitivity for health products, including Cohen and Dupas (2010), Dupas (2009, 2010), Kremer and Miguel (2007) and Ashraf et al. (2010). See Kremer and Holla (2008) for a review article.

to households with randomly selected product discounts for shoes, hand soap, and vitamin supplements which could be redeemed in exchange for the given health product. The randomization of prices allows us to directly estimate a demand curve for the product. We then cross-cut this price randomization with other treatments to test the hypotheses listed above, and ascertain whether the respective treatments shift the demand curve.

In our main experiment in Western Kenya, we focus on a particular health technology which potentially could have significant health impacts: rubber shoes for children.[2] The main channel through which shoes could affect health is from preventing worm (*helminth*) infection. This is because hookworm infection is transmitted through contact with the soil. When an infected person defecates in the soil, hookworm eggs hatch and develop into larvae, which are able to live in the contaminated earth for up to one month before requiring a human host to survive (Stoltzfus et al., 1997; Brooker et al., 2006). People are then infected when their skin comes into contact with the soil (typically through the feet of people who do not wear shoes). While worms can be easily treated after infection (see Miguel and Kremer, 2004), initial infection can be avoided by wearing shoes.[3] Hookworm has been shown to have important health effects. A well-known deworming intervention in this part of Kenya showed notable short-term health impacts (Miguel and Kremer, 2004) as well as longer-term human capital improvements (Baird et al., 2011). Spillovers from this treatment also had important cognitive effects on young children (Ozier, 2011). In a different context, Bleakley (2007) finds large effects of a hookworm-specific eradication program on school enrollment and literacy in the US South in the early 20$^{\text{th}}$ century.

Our main study takes place in the Busia and Samia districts in Kenya, an area in which worm infection is prevalent. Miguel and Kremer (2004) found that in this area 92% of surveyed children had at least one type of helminth infection and 37% had at least one moderate-to-heavy helminth infection. Although we did not test children for worms in our study, respondents report that 23% of their children had worms in the past year. While much of this is due to poor sanitation,

[2] The shoes were too small for most adults to wear, and so could only be worn by children.

[3] 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 (Phiri et al. 2002, Erosie et al. 2002). This seems plausible given the transmission pathway for the disease.

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.[4]

We conducted our primary experiment in early 2010 with a representative sample of 999 households in two areas in Western Kenya. We implemented three main experimental treatments. First, to measure the impact of health product 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 worm prevention. Second, to assess the role of liquidity constraints, we gave households a randomly determined amount of cash. Third, 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.

We find that even though the information script substantially increased worm knowledge, information alone had no impact on the ultimate purchase decision. Our estimates are precise enough that we can rule out large effects, suggesting that information alone is unlikely to be a panacea. By contrast, we find strong evidence that liquidity is important: redemption was significantly affected by the amount of cash payout individuals received. Interestingly, the interaction of the cash payout and script is positive (though statistically insignificant) – if the script has an effect, it is only when households had cash on hand. This last result suggests that information might be effective in certain situations, namely when other constraints are simultaneously removed (in this case, liquidity). In such a case, providing targeted information might serve as a type of "nudge" that encourages people to invest.

We also find that women are more likely to redeem their vouchers (by about 9 percentage points). This result is closely related to earlier studies on intra-household investment such as Thomas (1990), who shows that the propensity to invest in children increases more strongly with female than male income, or Duflo (2004) 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, general bargaining power). The experiment

[4] 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.

only varied whether the husband or wife was offered the coupon.[5]

That the gender of the parent receiving the coupon in our study matters suggests that the flow of information within the household may be limited. In this study, 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. If the wife receives a voucher for a discount on the price for children's health investment, the household may invest more in children at the expense of other items. This decision increases the mother's welfare, but may increase her husband's welfare by less, or even reduce it. Thus, if the husband receives the voucher, he may not choose to redeem it and withhold knowledge of it from his wife. Such a story is consistent with our results and is similar to Ashraf (2009), who finds evidence of intra-household communication barriers in a field experiment in the Philippines.

However, while we do find important liquidity constraint and gender impacts, by far the most important predictor of shoe purchase is price: about 78% of the variation in health-product purchase is explained through variation in price alone, overwhelming liquidity and gender effects. This result is consistent with the many other recent studies on the demand for preventative health products (many of which are summarized in Dupas, 2011).

In comparison to these other studies of products which have a preventative purpose (bednets, chlorine, water filters, etc.), shoes also have other non-health benefits. They keep the feet clean, they are observable by others and therefore may be a sign of wealth, and they improve mobility. Thus, a possible criticism for why we find no information effect is that people do not buy shoes for health, but for 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, the smaller studies we conducted in 2008 in Guatemala, India, and Uganda focused on products with minimal non-health benefits and which could be used by adults as well as children (soap and multivitamins). As in the Kenya study, we provided information to a randomly selected subset of participants, though the delivery mechanism dif-

[5] Note that while we did provide cash payouts to the sampled respondents, they were insubstantial relevant to lifetime income. Furthermore, random variation in the payout amount allows us to estimate a gender effect independently of the effect of the cash.

fered across studies. In Guatemala and Uganda, we randomly invited households to a health seminar (prior to the home visit and offer) which covered a variety of product-relevant health topics. In India, we read a product-related health script to the households at the time of survey (similar to the script in Kenya, though tailored to soap or vitamins rather than to shoes and worm prevention). In these studies, we provided all respondents with cash, and offered them the chance to buy the product at the doorstep (rather than giving them a coupon redeemable at a nearby shop). Thus we relieved both liquidity constraints and travel costs for all households, and required them to make an immediate purchase decision.

While a direct comparison to the Kenya experiment is not possible due to differences in the experimental design, the general results are quite similar. In all three sites, we consistently find very little effect of information alone. In Guatemala and Uganda, we find no effect of the health seminar; in India, we find a statistically significant difference between those given the script and those not for only one product. The higher take-up in India for that product may possibly be attributable to the fact that the script was delivered immediately before the "take it or leave it" offer for the product, so it is possible that the information was more salient when presented immediately (though it might also be due to sampling variation since only one of the six product-country combinations yielded a statistically significant difference). For most products in most countries, we continue to find that price explains the vast majority of the purchase decision (despite the fact that households had cash on hand and that the decision to purchase the product had to be made immediately).

Finally, it is possible that these interventions had spillover effects to those not treated. Several studies in agriculture find large spillover effects (*e.g.* Foster and Rosenzweig, 1995; Conley and Udry, 2010). Health-specific studies also tend to find spillover effects, though these effects can either serve to increase adoption (*e.g.* bednets in Kenya in Dupas 2010, menstrual cups in Nepal in Oster and Thornton 2011) 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.

In the Kenya experiment, we were able to causally estimate such effects in a similar manner to previous studies.[6] This is because, conditional on a given social network, the intensity with

[6] We were not able to estimate spillover effects in the other countries due to budget constraints.

which that network was treated with any intervention is random. We can then estimate spillover effects across two networks: (1) self-identified health "contacts" named at baseline, and (2) geographical neighbors living near the household. To further increase the variance in intensity (among neighbors), we stratified treatment intensity by geographic region.

Unlike most of the other studies of externalities listed above, we would not expect our experiment to generate significant learning effects. The main reason for this is that those studies focused on technologies for which learning through usage would be likely to occur, whereas our main study involves shoes, a very well-known product. Moreover, learning about the effect on worm prevention through shoe-wearing is unlikely: it takes some time to develop infections and sanitation is often so poor that even those wearing shoes might well get infected (though likely at lower rates than others), making the learning process long and noisy. In principle, there could have been some social learning if people communicated with each other about the script, but this is unlikely to translate into purchase since the script did not induce purchase even from those receiving the information directly.

It might still be possible that exogenously increasing usage among friends and neighbors would stimulate demand through an imitative channel,[7] since shoes are readily observable and carry a certain prestige value. However, we find no evidence of such effects. Interestingly, this is not because people did not talk about the shoes or because they have no desire to imitate: in a follow-up survey, 80% of parents reported talking to others about the product and over 70% of people said that seeing others with the shoes made them want them more for their own children.

There seem to be two main reasons why this did not translate into purchase. First, even though parents reported a desire to imitate, they ultimately reported that the most important barrier was the price of the shoes. Second, paradoxically, the flow of information was so strong that it may have actually mitigated some imitation from taking place. In the follow-up survey, 79% of parents reported that their neighbors actually knew the price of the voucher they had received (since they had talked about the vouchers with each other). That people knew the price others received mitigated social pressure: while a large majority of parents reported that

[7] Munshi and Myaux (2006) also study imitative peer effects. They show that rural Bangladeshi delayed purchasing 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.

they would be seen as a bad parent if they did not buy the shoes if offered a low-priced coupon, far fewer thought they would be seen as a bad parent if they did not redeem at a higher price. Thus, even though people were aware of others' prices and purchase decisions, and even though people apparently felt some pressure to "keep up with the Joneses," this pressure was mitigated by the fact that prices were largely common knowledge.

The lack of spillover effects contrasts with previous health studies in which direct social learning is possible, many of which find statistically significant effects (though sometimes negative and sometimes positive).[8] Our results suggest, quite naturally, that spillovers will likely be quite limited for products which have been around for some time and for which learning through usage is likely to be limited (like the products we study here– soap, vitamins, and shoes).[9]

Overall, our findings suggest that the most fruitful policy avenue for increasing usage is to address the price and liquidity barriers. While subsidies are the most straightforward way of doing this, another alternative might be to provide people with devices which allow them to acquire the necessary liquidity to overcome the price barrier to invest in such products. For example, Dupas and Robinson (2011b) experiment with several simple savings interventions designed to allow people to save for health, and find that people take up such products readily. In that study, providing even the simplest savings technology (a box with a lock) increased investment in preventative health by 68%. Other interventions along these lines might well prove effective in improving investment.

[8] For instance, Kremer and Miguel (2007) find negative peer effects for deworming drugs in Kenya, whereas Dupas (2010) and Oster and Thornton (2011) find positive peer effects for insecticide-treated bednets in Kenya and menstrual cups in Nepal, respectively. These differences are likely related to differences across studies in private costs and benefits, as well as health externalities. For example, deworming drugs have some negative side effects and the social benefit greatly exceeds the private one. By contrast, while bednets also have large health externalities (Hawley et al., 2003), the private cost of using a net might be smaller. In the case of menstrual cups, the benefit is entirely private.

[9] Our results differ from Duflo et al. (2009), who study the diffusion of fertilizer, another technology which has been available for some time but for which usage is low. That study finds that people do not talk about fertilizer much in the first place.

2, Analytical,Framework,

We"first"present"a"very"basic"framework"for"interpreting"our"main"results"(though"most"are"quite" straightforward,"the"intra-household"findings"deserve"some"discussion)."To"keep"the"discussion" brief and focused on the experimental setup,"we assume that there is a single private consumption" good"x_i"and"an"indivisible"children's"good"c"which"can"only"take"a"value"of"0"or"1"(*i.e.*"to"buy"the" product"or"not)."We"assume"preferences"are"additively"separable"and"can"be"expressed"as:"

$$U_i(x_i, c) = v_i(x_i) + w_i(c) \quad (1)$$

where"i"indexes"the"male"m"and"female"f."Following"Browning"and"Chiappori"(1998),"we" assume"that"the"household"is"efficient"such"that"aggregate"household"utility"can"be"written"as:"

$$U(x_m, x_f, c) = \max\{U_m(x_m, c) + \lambda U_f(x_f, c)\} \quad (2)$$

where"λ"indexes"the"wife's"bargaining"power."Normalizing"the"price"of"x_i"to"one,"the"house-" hold's"pooled"budget"constraint"is"that"

$$x_m + x_f + pc \leq y_m + y_f \quad (3)$$

where"p is the price of the children's good and"y_i is income."If the household does not purchase" the"children's"good"at"the"market"price"p,"it"must"be"that"$U(x_m, x_f, 0) \geq U(x_m, x_f, 1)$,"where" x_i and"x_i are"optimal"values"when"c"is"equal"to"0"or"1,"respectively"(i.e."which"set"the"ratio"of" marginal"utilities"equal"to"λ)."

In"the"experiment,"we"lower"the"price"of"the"children's"good"to"p',"which"allows"the"household" to"purchase"x_i if"they"purchase"the"children's"good." If"even"at"this"subsidized"price"men" would"prefer"not"to"invest"in"children" (i.e." $U_m(x_m, 0) > U_m(x_m, 1)$),"but"women"do"(i.e." $U_f(x_f, 0) < U_f(x_f, 1)$),"then"there"will"be"conflict"in"whether"to"buy"the"product." If"female" bargaining"power"is"sufficiently"high,"the"household"may"buy"the"product,"making"the"husband" worse"off"and"the"wife"better"off."If"this"is"the"case,"the"husband"will"have"an"incentive"to"not" tell"his"wife"about"the"discount,"or"to"destroy"the"voucher"so"that"the"household"cannot"redeem" it."

With this framework in mind, we can summarize predictions of the experiments below:

(1) *Providing information on the health value of the product.* If utility from the health product is increasing in information about its benefits, then providing information will increase investment in children.

(2) *Providing Liquidity* If the budget constraint is binding with respect to cash on hand, providing cash will shift out the budget constraint and may increase investment.

(3) *Targeting the husband or wife.* If differences in household preferences exist such that the wife prefers investing in children, targeting the wife may increase investment.

3, Experimental, Design, Kenya,

3.1, Background,

Worldwide, over 2 billion people are infected with soil-transmitted helminths (STHs), the most common of which include hookworm, roundworm, and whipworm.[10] Such infections are prevalent in Western Kenya, where this study takes place. 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 a STH hyperendemic area, while the combined STH (*i.e.* hookworm, roundworm, and whipworm) prevalence rate across Western province is 80.7%.

STHs may affect all portions of the population, but school-aged children and pregnant women are the most vulnerable sub-groups (Harhay et al., 2010; Brooker, 2009; Bethony et al., 2006). While mild infection typically has limited health consequences, more severe infections can have effects on morbidity (Brooker et al., 2009; Hotez et al., 2009; Bethony et al., 2006; Chan et al., 1994), and are also suspected to increase vulnerability to other illnesses, such as malaria, HIV, tuberculosis, and anemia (Bethony et al., 2006; Specht et al., 2007; Stoltzfus et al., 1997). A number of studies have demonstrated important health and education effects of reducing worm

[10] See Riesel et al. (2010), Brooker et al. (2006, 2009); Hotez et al. (2006, 2007, 2009), Bethony et al. (2006), and Harhay et al. (2010).

infections (Miguel and Kremer, 2004; Baird et al., 2011; Ozier, 2011; Bleakley, 2007).

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, helminth (specifically, hookworm) eggs hatch and develop into larvae, which are able to live in the contaminated earth for up to a month before requiring a human host to survive (Stoltzfus et al., 1997 Brooker et al., 2006). Hookworm helminths are most commonly contracted through the skin (typically through the feet of people who do not wear shoes), 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; Stoltzfus et al., 1997). Thus, in areas in which 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, Phiri et al. (2002) and Erosie et al. (2002) estimate 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, it seems natural that wearing shoes will reduce infection given the transmission pathway.

Another important health benefit of shoes is preventing foot injuries, and preventing those injuries from becoming infected. While worm infections might be a more significant concern, many parents report the prevention of such injuries as a reason to buy shoes (indeed, such injuries can be quite painful).

While shoes clearly have other non-health benefits, many respondents do report health as a benefit of shoes. For example, 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).

3.2, 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). Worm infection is very common in the area.

In our study, 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 wearing shoes during home visits (13%). Thus, increasing shoe usage could have important health consequences in this population.

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.[11] 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 market prices, and which are more likely to be worn around the home. For this reason, we focused on the latter product for this study.

To obtain as representative a sample of households as possible, a door-to-door census was conducted with 1,547 households in two villages located roughly 11 kilometers apart (Ikonzo 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 1,068 households for project participation, stratified by geographic cluster. We were able to interview 999 of these (93.5%).

3.3, Experimental, Treatments,

We implemented four main experimental treatments, all stratified by the geographic cluster

[11] The exchange rate was roughly 75 Ksh to $1 during the sample period.

and cross-cut against each other. 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 Kremer and Miguel (2007), Cohen and Dupas (2010), Dupas (2009, 2010), and Ashraf, Berry, and Shapiro (2010).[12] 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 at 5, 15, 25, 35, 55, or 65 Ksh. The coupon was 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 maintaining proper hygiene. 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 similar results from a script as a seminar (as we will discuss later).

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 questions, households were given a series of choices in which they could either accept a certain amount (40 Ksh or 100 Ksh) or could elect to take a gamble in which they would receive three times the amount invested with probability 0.5 and nothing with probability 0.5. For the time questions, households were given the option of accepting 40 Ksh immediately or a larger amount in the future. To ensure truth-telling, we implemented a Becker-Degroot-Marschak elicitation mechanism in which one of the questions was randomly picked for payout. Thus, conditional on risk and time preferences, the amount of money given to households was random. We can thus use variation in the amount paid out to estimate the effect of liquidity on purchase (while controlling for risk and time preferences).[13]

[12] These methodologies are very similar to that used to estimate demand for microcredit by Karlan and Zinman (2008).

[13] If a time preference was picked and the household chose to take the larger amount in the future, it was

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 male head, we randomly selected either the husband or the wife for the intervention.

4, Data,

There are three main pieces of data that we use to evaluate the program. First, at baseline, we administered a demographic 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 worms, transmission pathways, and prevention strategies at the end of the survey. 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.

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 kilometers 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 also 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 size of the shoes purchased. Thus, for the purchase decision we rely on these administrative records from the shop.

Lastly, we conducted a follow-up survey with a randomly selected subset of the households once the redemption period had ended (about 3 months after the first vouchers had been given out).[14] The follow-up survey included questions on shoe usage, as well as the same module to measure worm knowledge as used in the baseline (to measure knowledge retention at least over the redemption period). In addition, after some qualitative piloting, we added in a number of

necessary to revisit them to make the payout. To minimize the need to do this, the odds of picking a risk question were higher.

[14] Participation in the follow-up appears to be orthogonal to all the treatments (see Appendix Table A1).

questions about why people redeemed their coupons, which we use to support our main empirical findings.

5, 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:

$$Y_{iJ} = \sum_{j=51}^{651} {}_j P_{ijJ} + \gamma S_{iJ} + \delta C_{iJ} + \mu G_i M_{iJ} + X_i'\phi + \varepsilon_{iJ} \quad (4)$$

where $P_{ijJ} = 1$(if household i received a coupon at price j, $S_{iJ} = 1$(if the household received the script, C_{iJ} is the cash payout the household received, and $G_{iJ} = 1$(if the husband was sampled to receive the coupon. G_{iJ} is interacted with M_{iJ}(a dummy for whether the household is dual headed, with both a male and female head) because the gender of the sampled respondent is only random for such households (81% of households are dual-headed). Finally, X_{iJ} is a minimal vector of controls consisting only of M_{iJ} and G_{iJ} (so that $\mu\beta$ is interpretable), controls for risk and time preferences (since the experimental payout is correlated with risk/time preferences), and dummies for the stratification clusters (which we include to improve precision, as discussed in Bruhn and McKenzie, 2009). Finally, since we were not able to follow up with all of the men sampled for the survey in dual-headed households, we also present an Instrumental Variables specification in which we instrument actually interviewing the male with being sampled for the interview.

In addition, we can causally estimate externalities in this setup. 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. This allows us to estimate network effects by comparing the probability of purchase across people with randomly varying treatment intensities among their friends or neighbors.

We have two measures of social networks. The first is geography (as measured by GPS location), as in Dupas (2010). Furthermore, to create additional variation, we stratified intensity

by geographic cluster (so that the variance across clusters exceeds that obtainable by random chance). With this design, we can estimate geographical spillovers with the following regression:

$$Y_{iJ} = \sum_{j=1} {}_j T_{ijrJ} + \gamma N_{irJ} + X_i'\phi + \varepsilon_{iJ} \quad (5)$$

where T_{ijrJ} is the proportion of people within a radius $r\beta$ from person $i\beta$ who received the given treatment j. There are J such treatments (the prices, script, cash payout, and gender treatments). Here X_{iJ} 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.

At baseline, we also collected the names of three self-identified contacts. We asked people to identify the three people whom they spoke with most often, and matched these names to their treatment group (if they were in the study area). Of the 966 people willing to give the name of at least one friend, we matched 68.9% of the named contacts to our census list. While the total number of friends named is not random, nor the number of friends who could be matched (as those that could not be matched are likely to be outside the study area), conditional on the number of friends that were matched, the number receiving any treatment is random. Thus the following regression can be used:

$$Y_{iJ} = \sum_{j=1} {}_j T_{ijJ} + \gamma F_{iJ} + X_i'\phi + \varepsilon_{iJ} \quad (6)$$

where T_{ijJ} is the proportion of treated friends, and F_{iJ} is the total number of friends.

6, Results,

6.1, 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" at least occasionally instead of a latrine – 92% of children do this at least occasionally."

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 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."

6.2, 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 Figure 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.[15] Note also 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.

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 consistent with the other recent studies which show that demand is very price sensitive in developing countries.[16]

6.3, Effects, of, Interventions, on, Demand,

In Figure 2, we plot demand curves for the group that received the script and the group that did not (note that results in this figure are not regression adjusted with any 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, crisscrossing 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

[15] 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). Also, it is possible that the program tended to create a general excitement for the product, which increased demand at all prices.

[16] It is possible that households sold the shoes (or the voucher) after purchasing them. The data suggests this is unlikely: at the 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 wearing them. Thus, though it's likely much lower, an 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).

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.

To measure the gender difference quantitatively, we include a treatment indicator for the male being sampled in Table 2, Column 3. Since this is only relevant for dual-headed households, we interact the indicator with a dummy for whether the household is dual headed. To be able to interpret this result directly, we also include (but do not show) a dummy for being in a dual-headed household, and the interaction between *not* being in a dual-headed household and the male being sampled for treatment. 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 track 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%).[17]

Figure 2, Panel C shows the effect of the cash payout.[18] 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_{jJ} coefficients from the following regression

$$Y_{iJ} = \sum_{j=51}^{651} {}_{j}P_{ijJ} + (\gamma_j P_{ij} C_i) + (\varepsilon_{iJ} \quad (7)$$

where C_{iJ} is the cash payout (in 100s of Kenyan shillings). We find strong evidence of a liquidity effect: the cash payout shifts out demand, particularly at intermediate or high prices (the smaller effect at lower prices is because demand is already so high to begin with). Table 2 shows the regression results with controls for risk/time preferences. On average, every additional

[17] The first stage is shown in Appendix Table A2.

[18] We do not have accurate records of experimental payouts for some 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.

100 Ksh in randomized cash payout increases the probability of purchase by 22 percentage points.

Column 4 includes the same variables as Column 3, but also includes an interaction between the script and the cash payout. Interestingly, while the coefficient on that interaction is insignificant, it is positive and relatively large. While we cannot make any strong conclusions regarding this effect, this may suggest that knowledge can have an effect, but only when other constraints are simultaneously loosened (in this case, cash on hand). We will revisit this issue in the Section 5.

6.4, 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 demand for the shoes.

6.5, Peer, Effects,

As discussed above, since the intensity with which an individual's pre-existing network is treated is exogenous, we can also estimate the effect of peers on coupon redemption. As noted before, since shoes have been readily available for a very long time and most people have used them in the past, they are unlikely to be an "experience good" for which usage conveys some information. Furthermore, since information did not have a first-order effect on the individual sampled to receive it, it is unlikely that we would find second-order informational conformity

across households.[19]

Thus, "unlike most studies of spillover effects in development (which focus on social learning), the only role for externalities in this experiment is 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.

We find very little in the way of imitative peer effects for either geographical neighbors or health contacts in the purchase decision (Table 4). First, Panel A present geographical spillovers across different radii (300, 500, and 1,000 meters). 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),[20] and the total number of people living within the radii. 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). Panel B does the same for the matched informational contacts and, again, there is no discernible positive effect (if anything, the effect for the script is negative, though small and significant at only 10%).

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 peer 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 did talk about the program: 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 them-

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

[20] The results are not sensitive to defining this variable differently.

selves?"There"appear"to"be"two"main"reasons,"both"summarized"in"Panel"C."First,"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."

The"second"reason"is"that"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"the"shoes"at"a"high"price."Thus,"imitative"peer" effects"may"have"actually"been"mitigated"by"the"strength"of"social"connections.[21]"

7, External,Validity,

While"the"results"up"to"this"point"have"been"for"one"product"in"Kenya,"we"provide"some"evidence" in"this"section"that"most"of"our"basic"experimental"results"are"unlikely"to"be"specific"to"that" context." Before"the"Kenya"experiment,"we"conducted"smaller-scale"studies"in"three"different" countries"with"different"products"that"could"be"used"by"both"children"and"adults"(hand"soap" and"multivitamins)." These"studies"were"carried"out"in"2008"in"the"small"village"of"Panyebar," Guatemala"(pop." 2,031),"the"town"of"Busia,"Uganda"(pop." 36,600),[22]" and"the"large"city"of" Chennai"(Madras),"India"(pop."4,700,000)[23]."These"studies"were"more"limited"in"scope"than"the" Kenya"experiment:"all"respondents"were"women,"and"we"did"not"conduct"long"interviews"with" households"nor"measure"externalities." Nevertheless,"the"general"results"concur"strongly"with" those"in"Kenya."

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

[22] Busia,"Uganda"is"located"on"the"Ugandan"side"of"the"border"with"Busia,"Kenya"(Busia"is"a"border"district)."

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

In all three of our smaller studies, we designed the experiment so that purchase occurred at the point of the household survey. This design 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.

7.1, Background,on,Soap,and,Multivitamins,

Our health products for the smaller studies were hand soap and a multivitamin supplement. 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 (Luby et al., 2004, 2005; Ray et al., 2011; Curtis and Cairncross, 2003). Similarly, women and children in resource-poor regions are known to suffer from nutritional deficiencies (Stein, 2009; 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, Yip, 1994; Ramakrishnan, 2004).

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. The vitamin product was an 8-oz. bottle of multi-vitamin syrup primarily intended for children, but often used by adults. Products were similar in India, but differed slightly in Uganda, where the hand soap was a more common multi-purpose soap.

7.2, Experimental,Design,

7.2.1, Guatemala,

The first of our smaller studies was in Panyebar, Guatemala located in the western Guatemalan highlands, approximately five miles southwest of Lake Atitlan. The village has approximately 350 households, and all villagers are of Mayan origin, speaking Quiché as a first language and Spanish as a second language. The village is quite poor, even by rural Guatemalan standards, with little economic opportunity aside from coffee growing and subsistence agriculture.

We conducted fieldwork between June and August of 2008, during which time 350 female

heads of household were sampled for project participation. The sample was randomly divided into a treatment group of 197 and a control group of 153. The treatment group was invited to one of two five-day public health seminar lasting an hour and a half each morning. To encourage participation, each invited subject was given five tickets to use for entrance into the seminar and upon arrival the tickets were placed into a hat to be drawn for door prizes (e.g., blankets, pots and pans, machetes, etc.). In total, 84% of the randomly invited women attended the seminar. Of those who agreed to participate, we have complete follow-up data on all but one subject.

After the seminars, enumerators conducted a population survey of the village (including the full treatment group, whether they attended the seminar or not, as well as the control group), which lasted approximately 45 minutes. The primary respondent was the female head of household.

At the close of the survey we presented each female subject with Q16 (Guatemalan *quetzales*), in cash, or about $2.00. The subject then drew one of six coupons from a black bag, which entitled her to a 25%, 50%, or 75% discount on one of two products: a bottle of vitamin syrup or a three-bar pack of anti-bacterial hand soap (the product was not chosen by the subject, but was randomly determined by the coupon draw.) Both products had an approximate retail value of Q16. In Guatemala (as in India and Uganda), the products were sold "Avon lady" style at the site of the household: women had to buy the product immediately from the enumerator at the conclusion of the interviewer. Subjects chose at that time whether to take the Q16, or whether to take the health product and the remaining change. The session then concluded.

7.2.2, Uganda,

We conducted a nearly identical survey and experiment simultaneously in Busia, Uganda between May and August, 2008. Busia, Uganda shares a border with Busia, Kenya, and is a relatively busy commercial trading post. The sample frame for the study was constructed using enrollment lists from five elementary schools in the area. From roughly 2,000 children, 800 (randomly divided between treatment and control) were invited to an information session. Those selected for the seminar were given invitations while others listed their tracking information. In total, 516 women (266 treatment and 250 control) participated in the project.

As an encouragement for the treatment group to attend the health seminar, women who came to the seminar were eligible for random door prizes, as in Guatemala. Of those invited to the seminar, 87% attended. The health seminar was shorter and more focused than the one in Guatemala (lasting one day), emphasizing basic hygiene, nutritional information, and preventative healthcare through the encouragement of hand washing and proper vitamin intake.

One to three weeks after the seminar, surveys similar to those used in Guatemala were issued to each of the members of the sample at the point of household. Since tracking information had already been obtained at the initial meeting, we were able to successfully follow up with 514 of the 516 women in the study. As in Guatemala, at the conclusion of the interview, respondents received a small amount of money (about 1600 Uganda shillings, or about $1 in 2008), and were given the option to purchase the health investment good at the selected discounted price, or take the full cash payout.

7.2.3, India,

Our third smaller study took place among 455 households in Chennai, India. The sample frame was an urban population of women who had been involved in, or were potential candidates for, a local microfinance program operated by Growing Opportunity Finance, Ltd., affiliated with the large microlender, Opportunity International.

Unlike the Guatemala and Uganda studies, subjects in the India sample did not attend a health seminar. Instead, during the survey (which was similar to those in the other countries, though it also contained a microfinance module), 208 of the 455 households were randomly chosen to be read a 120-word health script. For those randomly selected to be offered soap, the script reminded people that hand-washing with soap and clean running water at the appropriate times (*e.g.*, after using the toilet, touching an animal, sneezing or coughing) could prevent germs from spreading. A similar script was read for those offered vitamins which focused on the importance of broad vitamin intake for general health. At the end of the survey, the same procedure was used as in Guatemala and Uganda in which respondents were given a small amount of money and then randomly offered a discount on a health product (the stakes were 50 rupees, again around $1).

7.3, Results,

7.3.1, Background,Statistics,and,Randomization,Check,

Appendix"Table"A3"presents"background"statistics"as"well"as"a"check"on"the"effectiveness"of" our"randomization"in"each"of"the"three"countries." Column"1"presents"the"overall"mean"across" the"sample." While"we"have"few"covariates"to"compare"across"samples,"there"are"some"stark" differences"across"countries." Average"years"of"education"is"just"1.94 in"Guatemala,"compared" to"5.7"in"Uganda"and"5.5"in"India." However,"the"subjects"of"the"Guatemala"study"have"much" better"access"to"piped"water"than"do"those"in"the"other"countries."Family"size"is"much"smaller" in"India"than"in"the"other"countries,"because"the"India"study"was"conducted"in"the"major"city" of"Chennai."

Columns"2-6"check"for"randomization"validity." They"show"coefficients"of"a"regression"of" the"given"dependent"variable"on"the"treatment"indicators"(the"price,"the"seminar"treatment"in" Guatemala"and"Uganda"and"the"script"treatment"in"India,"and"whether"soap"or"multivitamins" were"offered)."As"there"are"several"price"dummies,"Column"7"presents"the"p-value"for"a"joint"test" that"all"the"price"variables"are"jointly"insignificant."

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"regards"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"vouchers"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"(though"omitting"them"makes"little"differences"to"our"estimates)."

7.3.2, Experimental,Results,

The"basic"experimental"results"are"summarized"in"Figure"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.[24] 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: investment in health products is highly sensitive to price, and information about health products have small or negligible effects on their own, unless – perhaps – other frictions are also removed simultaneously.

8, Discussion:, What, Influences, Investment, in, Preventative, Health?,

How does this paper relate to other recent studies 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.[25] 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

[24] 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.

[25] For a much more exhaustive review (including other health decisions such as choice of water source or hand-washing behavior and other methodologies), see Dupas (2011).

campaigns, gender, and spillovers through peer effects.

Overall, we find a number of similarities with other studies. First, just as in all other recent studies, we find that demand for shoes is quite price sensitive. While it is impossible to make direct comparisons in that 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 or 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 (2007) study of deworming drugs in which even a modest US$0.30 cost-recovery fee decreased demand by 80%. 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 finding that demand for health products is very price elastic in LDCs.

Though there are relatively few randomized experiments on how information affects preventative health investment (rather than behavior change more generally), those studies which do exist tend to find small effects, as we do. Kremer and Miguel (2007) find no effect of education targeted at children bn 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. (2011), 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).

Thus, although there are few studies to summarize, these early results suggest that simply providing information is not often effective in these types of investment decisions. This differs from other studies summarized in Dupas (2011), likely for three main reasons. First, the information provided was not very specific to the household. This differs from, for instance, Jalan

and Somanathan (2008) and Madajewicz et al. (2007), who find 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 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 like Cairncross et al. (2005) or Luby et al. (2004, 2005), who find 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 come up with enough money to buy the product. This is difficult for very poor households who do not have access to credit or to good savings products. By contrast, soap was provided as part of the intervention in Luby et al. (2004, 2005), while the main outcomes in Cairncross et al. (2005) were various measures of self-reported behavior.

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 (Miguel and Kremer, 2007; Dupas, 2010; Oster and Thornton 2011) 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 voucher suggests not only intra-household differences in preferences, but also constraints on information sharing within the household. Our analytical framework suggests that husbands may simply not tell the wife (or may destroy the coupon) if he knows that she will be able to force him to reduce his own consumption to take advantage of the discount for children. The closest paper in this respect is Dupas (2009),

who randomized whether the female head, male head, or both heads jointly would receive a voucher for a bednet. While she finds no difference in takeup between the treatment in which the male gets the voucher 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.

9, 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, in each of our four field experiments and with all three of our health products, we find product take-up to be more sensitive to price than any other factor, and for the vast majority of the purchase decision to be determined by the price. This general finding is robust across all the other studies mentioned above.

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, longterm 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 bther 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 bught 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 prefer- ences 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, 2011b). Designing products to help households accumulate necessary liquidity may be a fruitful avenue for future research.

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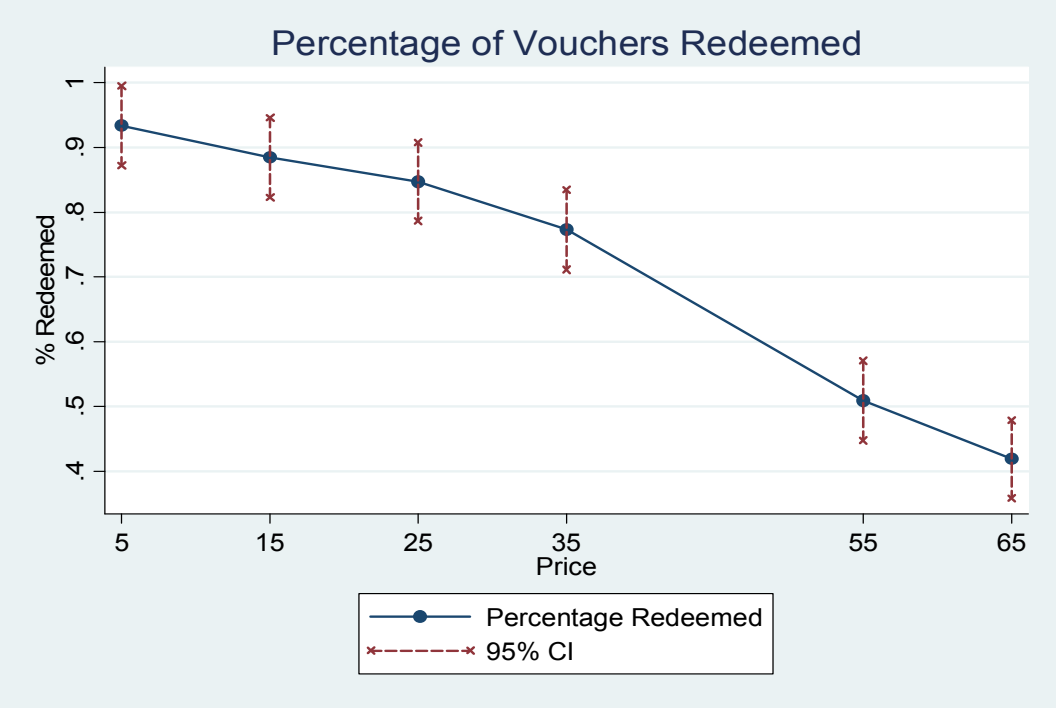
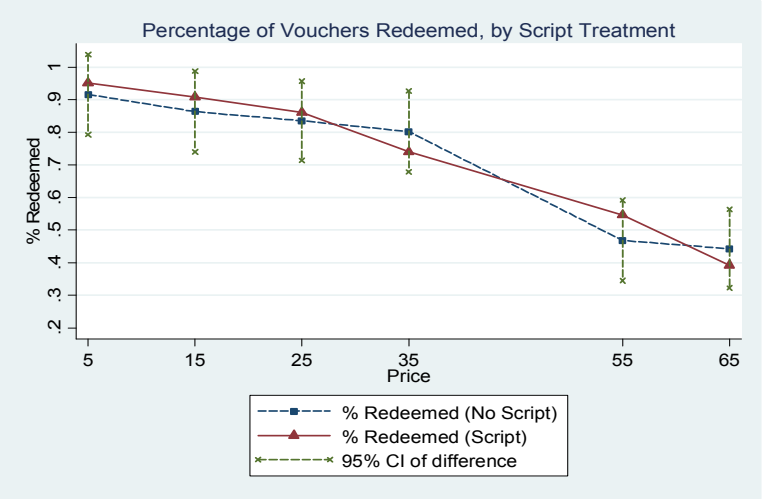
Figure 1. Demand for Shoes



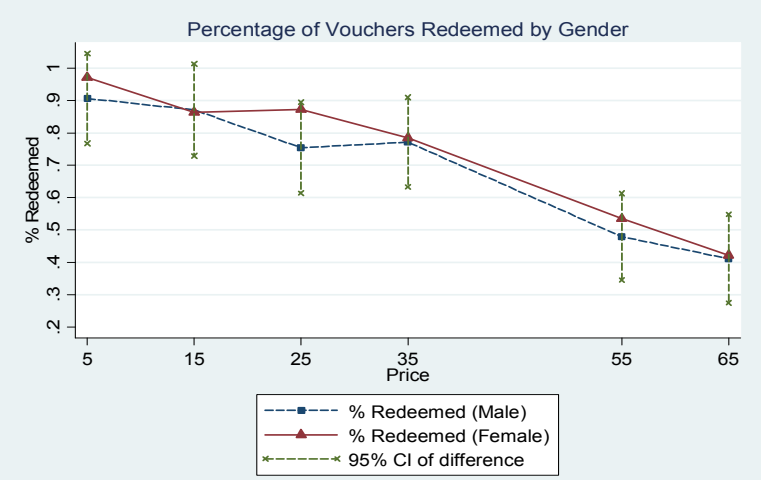
Notes: 95% Confidence Intervals in parentheses.

Figure 2: Experimental Treatments

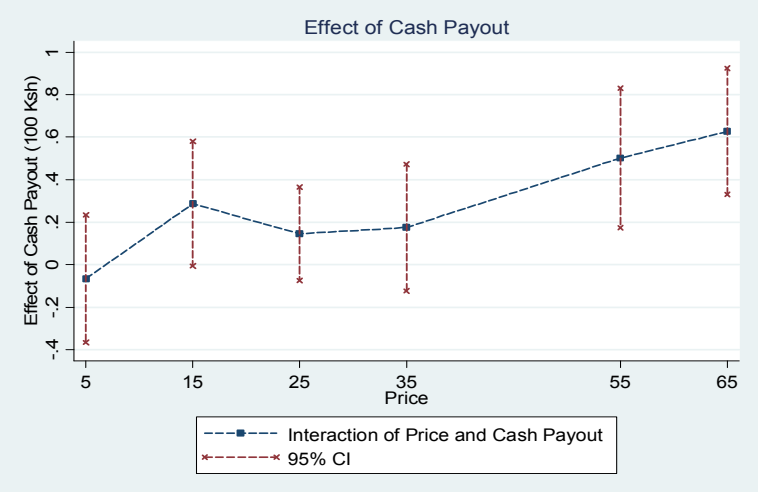
Panel A. Information



Panel B. Parental Gender



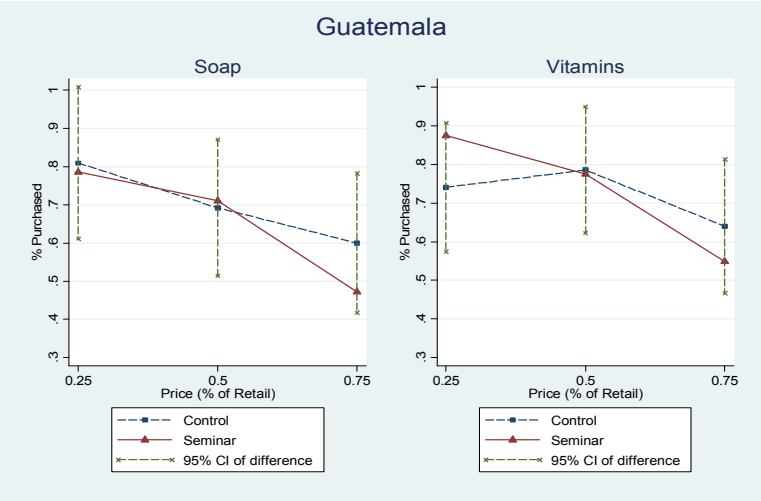
Panel C. Liquidity



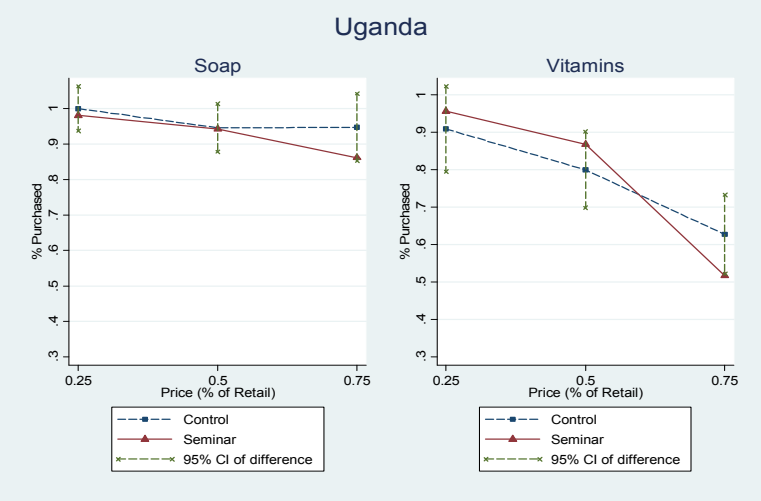
Notes: The confidence intervals reported are of the difference between the given experimental groups. All figures are Intent-to-Treat estimates.

Figure 3. Results from Smaller-Scale Projects in Guatemala, Uganda, and India

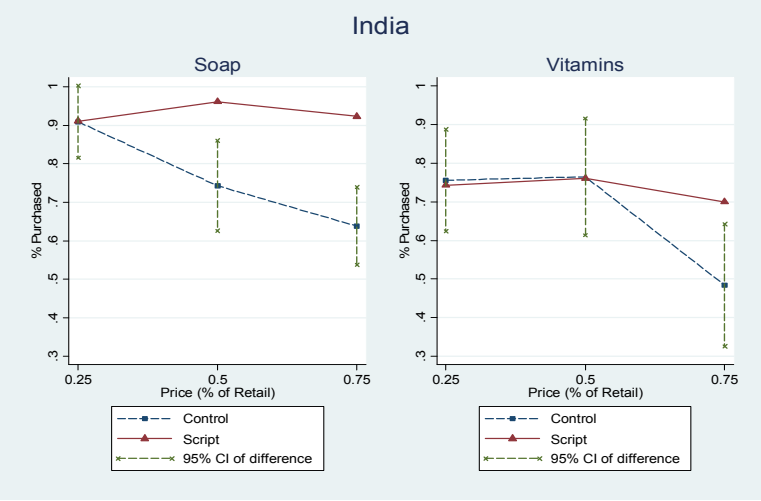
Panel A. Guatemala



Panel B. Uganda



Panel C. India



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

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
Panel A. Household Level Variables					
Dual-Headed Household	0.81	0.00	-0.03	-0.09	0.16
	(0.39)	(0.02)	(0.02)	(0.06)	
Number of Children	3.52	-0.03	-0.03	0.20	0.87
	(1.74)	(0.11)	(0.02)	(0.26)	
Average child health (1-5 scale)[1]	2.53	0.03	-0.05	0.07	0.97
	(0.60)	(0.04)	(0.04)	(0.09)	
Percentage of children with	0.23	0.00	-0.01	0.00	0.63
worms in past year	(0.19)	(0.01)	(0.01)	(0.03)	
Proportion of Children	0.17	-0.01	0.02	-0.06	0.25
owning shoes	(0.19)	(0.01)	(0.01)	(0.03)**	
Percentage of children	0.13	0.04	0.02	-0.12	0.44
at interview wearing shoes	(0.44)	(0.03)	(0.03)	(0.07)	
Proportion of Children who do	0.92	0.02	0.00	0.00	0.97
not always use latrine/bathroom	(0.25)	(0.02)	(0.02)	(0.04)	
Value of Animals Owned	10.75	0.40	0.25	1.01	0.16
(in 1,000 Ksh)	(12.26)	(0.83)	(0.83)	(1.94)	
Panel B. Individual Level Variables					
Gender (1=male)	0.28	-0.04	-	0.07	0.42
	(0.45)	(0.02)	-	(0.06)	
Years Education	5.60	-0.07	-	-0.31	0.74
	(3.80)	(0.24)	-	(0.57)	
Literate (Swahili)	0.67	0.00	-	-0.03	0.77
	(0.47)	(0.03)	-	(0.07)	
Age	39.34	-0.61	-	3.45	0.43
	(14.57)	(0.93)	-	(2.18)	
Occupation = farmer	0.54	-0.01	-	-0.07	0.53
	(0.50)	(0.03)	-	(0.08)	
Self-reported health status	2.43	0.01	-	0.22	1.00
(1-5 scale)	(0.70)	(0.04)	-	(0.11)**	
Percentage of adults	0.34	-0.02	-	0.03	0.47
at interview wearing shoes[2]	(0.46)	(0.03)	-	(0.07)	
Had worms in past year	0.26	-0.02	-	0.00	0.40
	(0.44)	(0.03)	-	(0.08)	
Amount invested (out of 100 Ksh)	52.47	1.77	-	-	0.41
in risky asset	(20.71)	(1.34)	-	-	
Somewhat Patient	0.08	0.00	-	-	0.62
	(0.27)	(0.02)	-	-	
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 report 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 individuals 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.

* significant at 10%; ** significant at 5%; *** significant at 1%

[1]One is "very good" and 5 is "very poor."

[2]This variable is listed as an individual level variable since men are more likely to wear shoes than women.

Table 2. Experimental Treatments

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable = 1 if Purchased Shoes				
Price = 5 Ksh	0.93	0.98	1.09	1.09	1.05
	(0.03)***	(0.09)***	(0.42)***	(0.42)***	(0.42)**
Price = 15 Ksh	0.88	0.94	1.05	1.05	1.01
	(0.03)***	(0.09)***	(0.42)**	(0.42)**	(0.42)**
Price = 25 Ksh	0.85	0.91	1.00	1.01	0.97
	(0.03)***	(0.09)***	(0.42)**	(0.42)**	(0.42)**
Price = 35 Ksh	0.77	0.84	0.93	0.93	0.88
	(0.03)***	(0.09)***	(0.42)**	(0.42)**	(0.42)**
Price = 55 Ksh	0.51	0.59	0.68	0.68	0.64
	(0.03)***	(0.09)***	(0.42)	(0.42)	(0.42)
Price = 65 Ksh	0.42	0.49	0.58	0.58	0.54
	(0.03)***	(0.09)***	(0.42)	(0.42)	(0.42)
Received Script			-0.02	-0.06	-0.06
			(0.03)	(0.04)	(0.04)
Experimental Payout (in 100 Ksh)			0.22	0.16	0.17
			(0.06)***	(0.08)**	(0.08)**
Male Received Treatments			-0.05	-0.05	
(Double Headed Household only)[1]			(0.03)*	(0.03)*	
Male Actually Received Script					-0.09
(Double Headed Household only)[2]					(0.05)*
Received Script * Experimental Payout				0.13	0.12
				(0.12)	(0.12)
Estimation	OLS	OLS	OLS	OLS	IV
Cluster Dummies	N	Y	Y	Y	Y
Observations	999	999	999	999	999
R-squared	0.78	0.79	0.80	0.80	-

Notes: Experimental payout is in 100 Ksh. Exchange rate roughly 75 Ksh to US $1 during this time period. 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). Clusters were calculated from GPS coordinates and sampling was stratified at that level. Standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

[1]Regressions in Columns 3 and 4 include a dummy for whether the household was double headed, and an interaction between not being double headed and being sampled for the male treatment. This is because the gender of the respondent can only be random for dual headed households, so this coefficient should be interpreted as the difference in male and female purchases for dual headed households.

[2]Variable is instrumented with whether the male was sampled for the interview. See Appendix Table A1 for the first stage regression

Table 3. Worm Knowledge

	(1)	(2)
Panel A. Immediately after getting script		
Read Script	0.34	0.34
	(0.01)***	(0.01)***
Extended Controls	N	Y
Observations	989	989
R-squared	0.54	0.91
Mean in Control Group	0.29	0.29
Panel B. Three to Four Months Later		
Read Script	0.24	0.24
	(0.02)***	(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%

Table 4. Testing for Spillover Effects in Redemption

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable: Redeemed Coupon					
Panel A. Geographical Neighbors						
Within 300m of household						
# 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)				
Within 500m of household						
# 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)		
Within 1,000m of household						
# 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
Panel B. Health Contacts						
# 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				
		(0.0375)*				
Observations	868	868				

Notes: regressions control for all experimental treatments and include geographical (cluster) controls. Standard errors are clustered at that level. Standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 5. Mechanisms

	(1)	(2)
Panel A. Imitative Peer Effects		
Were slipper popular among children?	0.926	353
Did your child ask you to buy slippers?	0.689	376
Did your child want them more because other children had them?	0.753	376
Did you want them more because other children had them?	0.705	376
Would you feel poorer if the neighbor's children had slippers but yours didn't?	0.699	376
Would your neighbor buying a TV make you want to buy one more?	0.532	376
Would you feel poorer if the neighbors had TV and you didn't?	0.629	377
Panel B. Social Interactions		
Did you talk to anybody else who had received a voucher?	0.799	378
Did neighbors know what price coupon you got?	0.789	360
If received script: did you talk to others about worms?	0.725	182
If didn't receive script: did anybody talk to you about worms?	0.517	178
Panel C. Reasons for Purchasing		
Would other parents think you were a bad parent if you didn't redeem at low price?	0.779	348
Would other parents think you were a bad parent if you didn't redeem at high price?	0.503	342
For those who redeemed, what is the main reason you redeemed?		
Price was low	0.416	279
Pressure from children	0.108	279
Neighbors	0.075	279
Health	0.330	279
Other	0.072	279

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

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	0.80	1.00	0.93	0.85	0.73
	(0.08)***	(0.07)***	(0.03)***	(0.05)***	(0.04)***	(0.06)***
Price = 50% Retail	0.73	0.78	0.96	0.83	0.78	0.73
	(0.07)***	(0.07)***	(0.03)***	(0.04)***	(0.05)***	(0.06)***
Price = 75% Retail	0.55	0.59	0.91	0.58	0.69	0.56
	(0.07)***	(0.07)***	(0.03)***	(0.05)***	(0.05)***	(0.07)***
Invited to Health Seminar	-0.04	0.01	-0.03	0.01		
	(0.07)	(0.07)	(0.03)	(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%

Table 7. Comparative Results of Some Recent Experimental Health Studies

Study	Country	Product	Price Sensitivity[1]	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[2]	Very high at p = 0.[3]	Education campaign (among children) had no effect	Negative peer effects on treatment uptake	NA
Ashraf, Berry, & Shapiro (2010)	Zambia	Chlorine water purification	e = -0.60 [4]	NA	NA	Only Women
Cohen and Dupas (2010), Dupas (2009, 2010)	Kenya	Treated bed nets	e = -0.47 (Cohen and Dupas 2010)	No significant effect of scripts[5]	Strong peer effects[6]	No effect
Oster and Thornton (2010)	Nepal	Menstrual Cups	NA	NA	Strong peer effects	Only Women
Kremer et al. (2011)	Kenya	Chlorine water purification	Very high at p=0.[7]	Small effects from inform. campaign	Little evidence of peer effects	Only Women
Ashraf, Jack, & Kamenica (2011)	Zambia	Chlorine water purification	e = -0.636.[8]	No level effect, but increases price elasticity.	NA	NA

[1]Column reports elasticities at mean price unless otherwise noted.

[2]Albendazole and praziquantel.

[3]Relative to a zero price, a $0.30 fee cost-recovery fee decreased demand 80%.

[4]About 80 percent of respondents bought Clorin at 300 Kw with 50 percent buying at 800 Kw (3200 Kw = $1US).

[5]Neither health nor financial encouragements significantly affected purchase.

[6]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.

[7]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.

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

Appendix Table A1. Background Characteristics of those Selected for Follow-up

	(1) Administered Follow-up Survey
Price = 5 Ksh	0.04
	(0.05)
Price = 15 Ksh	0.04
	(0.05)
Price = 25 Ksh	0.07
	(0.05)
Price = 35 Ksh	0.09
	(0.05)*
Price = 55 Ksh	0.00
	(0.05)
Sampled for Script	-0.03
	(0.03)
Male	-0.07
	(0.03)**
Risk Payout	-0.06
	(0.07)
p-value for joint test of all treatments	0.20
Mean of Dependent Variable	0.38
Number of Observations	999
R-squared	0.01

Notes: Omitted price category is 65 Ksh.
Standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

Appendix Table A2. First Stage for Randomization of Gender of Household Head

	(1) Male Interviewed
Male Sampled for Interview	0.57
	(0.03)***
Constant	0.04
	(0.02)**
Number of Observations	812
R-squared	0.37

Notes: No controls are included. Regressions is restricted to dual-headed households.
Standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

Appendix Table A3. Randomization Check for Pilots in Guatemala, Uganda, and India

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Coefficients of OLS Regression of Treatment Indicators on Dependent Variable					p-value for test of joint significance of all price dummies
	Overall Mean	50% Retail	75% Retail	Invited to Seminar	Read Script	Product = Soap	
Panel A. Guatemala							
Years Education	1.94	-0.55	-0.27	-0.36		-0.02	0.19
	(2.31)	(0.31)*	(0.31)	(0.25)		(0.25)	
Age	37.53	2.46	0.61	-3.39		-0.21	0.33
	(13.25)	(1.75)	(1.80)	(1.43)**		(1.41)	
Number of Children	3.48	-0.06	0.34	0.05		-0.14	0.39
	(2.38)	(0.32)	(0.32)	(0.26)		(0.26)	
Household has Access to Piped Water	0.89	0.04	0.06	-0.04		0.01	0.30
	(0.31)	(0.04)	(0.04)	(0.03)		(0.03)	
Number of Observations	349						
Panel B. Uganda							
Years Education	5.70	-0.88	-0.30	0.10		-0.75	0.09*
	(3.98)	(0.40)**	(0.46)	(0.35)		(0.36)**	
Age	32.87	1.50	-0.23	0.53		0.71	0.17
	(9.31)	(0.95)	(1.07)	(0.83)		(0.84)	
Number of Children	4.17	-0.07	-0.30	-0.07		0.22	0.32
	(1.77)	(0.18)	(0.20)	(0.16)		(0.16)	
Household has Access to Piped Water	0.05	0.01	0.02	0.01		0.08	0.75
	(0.22)	(0.02)	(0.03)	(0.02)		(0.02)***	
Number of Observations	514						
Panel C. India							
Years Education	5.50	0.74	-0.46		-1.46	-0.10	0.07*
	(4.44)	(0.50)	(0.50)		(0.41)***	(0.41)	
Age	39.38	-0.52	1.14		-1.06	0.68	0.36
	(9.82)	(1.11)	(1.12)		(0.93)	(0.93)	
Number of Children	1.25	0.03	0.01		0.03	-0.04	0.97
	(1.07)	(0.12)	(0.12)		(0.10)	(0.10)	
Household has Access to Piped Water	0.42	0.04	0.07		0.06	-0.04	0.49
	(0.49)	(0.06)	(0.06)		(0.05)	(0.05)	
Number of Observations	455						

Notes: Each row is a separate regression of the given dependent variable on all the treatments listed. The omitted price category is 25% of the retail price, and the omitted health product is adult multivitamins. All respondents were women. Standard errors in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%