Short-Run Subsidies and Long-Run Adoption of New Health Products: Evidence from a Field Experiment*

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Abstract

Short-run subsidies for health products are common in poor countries. How do they affect long-run adoption? A common fear among development practitioners is that one-off subsidies may negatively affect long-run adoption through reference-dependence: People might anchor around the subsidized price and be unwilling to pay more for the product later. But for experience goods, one-off subsidies could also boost long-run adoption through learning. This paper uses data from a two-stage randomized pricing experiment in Kenya to estimate the relative importance of these effects for a new, improved antimalarial bed net. Reduced form estimates show that a one-time subsidy has a positive impact on willingness to pay a year later. To separately identify the learning and anchoring effects, we estimate a parsimonious experience-good model. Estimation results show a large, positive learning effect but no anchoring. We then discuss the types of products and the contexts for which these results may apply.

JEL codes: C93, D12, H42, O33. Keywords: technology adoption; experimentation; social learning; anchoring; malaria; prevention.

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1 Introduction

In 2010, an estimated 7.6 million children died before the age of five (Liu et al., 2012). It is estimated that nearly two thirds of these deaths could be averted using existing preventative technologies, such as vaccines, insecticide-treated materials, vitamin supplementation, or point-of-use chlorination of drinking water (Liu et al., 2012; Jones et al., 2003). An important question yet to be answered is how to increase adoption of these technologies.

A commonly proposed way to increase adoption in the short run is to distribute those essential health products for free or at highly subsidized prices (WHO, 2007; Sachs, 2005). There are two main economic rationales to do so. First, given the infectious nature of the diseases they prevent, most of these products generate positive health externalities, and without a subsidy private investment in them is socially suboptimal. Second, when the majority of the population is poor and credit-constrained, subsidies may be needed to ensure widespread access (Cohen and Dupas, 2010; Tarozzi et al., 2013).

For some products, such as vaccines, one-time adoption is sufficient to generate important health impacts. One-time subsidies are well-suited for these technologies. But for other products, such as anti-malarial bed nets, water treatment kits, or condoms, repeat purchases and consistent use are required to generate the hoped-for health impacts. A key question and ongoing debate is whether one-time subsidies for such technologies increase or dampen private investments in them in the long run.

A short-run subsidy may increase demand in the long run if the product is an experience good. Beneficiaries of a free or highly subsidized sample will be more willing to pay for a replacement after experiencing the benefits and learning the true value of the product if they previously had underestimated these benefits. This learning might spread to others in the community (those ineligible for the subsidy) and increase the overall willingness to pay in the population.

These positive effects hinge upon people using a product or technology that they receive for free or at a highly subsidized price. This might not be the case, however. Households that are not willing to pay a high monetary price for a product might also be unwilling to pay the non-monetary costs associated with using the product on a daily basis. In other words, subsidies may undermine the "screening effect" of prices (Ashraf, Berry and Shapiro, 2010; Chassang, Padro i Miquel and Snowberg, 2012). Subsidies could also reduce the potential for psychological effects associated with paying for a product, such as the "sunk cost" effect, whereby people who have paid more for a product feel more compelled to put it to good use.[1]

Even if people use products they receive as free trials, they might be unwilling to pay a higher price for the product once the subsidy ends or is reduced. This could happen if people take previously encountered prices as reference points, or anchors, that affect their subsequent reservation price (Köszegi and Rabin, 2006). Such effects, known in psychology as "background contrast effects" and first identified experimentally by Simonson and Tversky (1992), have recently been observed

[1] Recent experiments conducted in urban Zambia and rural Kenya find no evidence for the the psychological sunk cost effect, however (Ashraf et al., 2010; Cohen and Dupas, 2010).

outside the lab by Simonsohn and Loewenstein (2006). Under such reference-dependent preferences, one-time subsidies for health products could generate a sort of entitlement effect that would dampen long-run adoption.

The view that these negative effects might dominate the standard positive learning and health effects is quite prevalent among development practitioners. There is, however, no rigorous evidence to date as to what short-run subsidies do to long-run adoption of new technologies.

To inform this debate and gage the relative importance of these effects, we conducted a field experiment in Kenya with a new health product, the Olyset long-lasting insecticide-treated bed net (LLIN), a recent innovation in malaria control. The Olyset LLIN is significantly more comfortable to sleep under than traditional bed nets, it is sturdier and more durable, and it stays effective for much longer. Given these characteristics, its long-run adoption should be boosted by the learning effects of a one-time subsidy, unless anchoring around the subsidized price is important. The experiment included two phases. In Phase 1, subsidy levels for Olyset LLINs were randomly assigned across households within six villages. Households had three months to acquire the product at the subsidized price they had been assigned to. Prices varied from \$0 to \$3.80, which is about twice the average daily wage for casual agricultural work in the study area. In Phase 2, a year later, all households in four villages were given a second opportunity to acquire an Olyset LLIN, but this time everyone faced the same price (\$2.30). The Olyset was not available outside of the experiment, but traditional nets were available on the market for \$1.50.[2]

This experimental design allows us to estimate the effects of one-off subsidies on demand, both over time and across individuals. We first test whether subsidies increase the short-run level of adoption. We find very large effects: adoption in Phase 1 increases from 7% to over 60% when the price decreases from \$3.80 to \$.75, and reaches 98% when the price drops to zero. What's more, information about the product appears to diffuse through spatial networks – households are more likely to purchase the Olyset in Phase 1 when the density of households around them who receive the high subsidy was (randomly) higher. The timing of voucher redemptions as well as survey evidence further suggest the presence of informational spillovers on the product characteristics within the three months during which vouchers could be redeemed.

We then estimate how the Phase 1 subsidy level affects willingness to pay for an Olyset net in Phase 2. We find that gaining access to a highly subsidized Olyset net in the first year increases households' observed willingness to pay for an Olyset net a year later: households who had to pay \$.75 or less in Phase 1 were 7.2 percentage points more likely to invest in a \$2.30-Olyset in Phase 2 than those who faced a higher Phase 1 price (corresponding to a 49 percent increase). Ultimately those who benefited from a high subsidy in Phase 1 were three times more likely to own two Olysets by the end of the study period than those who did not. This suggests the presence of a positive learning effect which dominates any potential anchoring or entitlement effect. Suggestive follow-up

[2]When 23 local retail shops around the study areas were offered the opportunity to stock Olyset nets, they were unwilling to purchase Olyset nets at wholesale prices above \$1.50, fearing lack of demand. This is not specific to the study context. Products considered for public subsidies are typically not available at local markets precisely because of low demand at unsubsidized prices.

survey evidence is consistent with the presence of a learning effect. On the other hand, higher exposure through spatial networks in Phase 1 appears to dampen adoption in Phase 2, suggesting a positive health spillover effect that reduces the need for private investment in prevention.

While these reduced form results suggest that the total effect of short-run subsidies on long-run adoption of a new, improved antimalarial bed net is positive, they do not allow us to separate out and quantify the learning effect from the anchoring and health spillover effects. For this, we estimate an experience-good model that allows for reference-dependent preferences, learning from experimentation, informational spillovers and health spillovers, but assumes agents are myopic (they do not engage in strategic experimentation nor anticipate health spillovers). We estimate the model using both Phase 1 and Phase 2 adoption decisions and find evidence of an economically large and statistically significant learning effect, but no evidence that Phase 1 prices are taken as reference points in Phase 2.

Overall, our results suggest that short-run subsidies for new health products impact long-run adoption through their effect on knowledge about the products, not through anchoring effects. The sign of the learning effect, while positive in our context, will obviously depend on the product and circumstances, however – in particular, on people's priors on the product as well as on how easily observable the health effectiveness of the product is. In the penultimate section of the paper, we discuss the contexts and products for which learning may go the other way, and make a few conjectures regarding four commonly subsidized products: water filters, chlorine, cookstoves and deworming. We then relate our results to those of related field studies. The most closely related study is Kremer and Miguel (2007), also in Kenya, which find that introducing a small fee to keep a school-based deworming treatment program going reduced coverage from 75% to 19%. Their experimental set-up does not provide the counterfactual however – what share of households would have paid the fee had they not been exposed to a free trial for a few years? We argue it may have been more than 19%, *not* because the free trial created a sense of entitlement – rather, because the free trial enabled households to learn the private costs of deworming outweigh the private gains.

Besides contributing to the literature on pricing and user fees for health products, and to the lively policy debate on free distribution versus cost-sharing, our paper contributes to a growing literature on the role of learning-by-doing and social learning in technology adoption in poor countries (see Foster and Rosenzweig 2011 for a review; and Munshi and Myaux 2006, Adhvaryu 2013, and Oster and Thornton 2012 for learning about health technologies in particular). Our paper also contributes to the empirical psychology and economics literature, testing behavioral economics in the field (see DellaVigna 2009 for a review), and complements earlier papers that have estimated, in rich countries, how the willingness to pay for a product can be affected by anchors (Ariely, Loewenstein and Prelec, 2003), previously encountered prices (Simonsohn and Loewenstein 2006; Mazar et al. 2009), or the range of options available (McFadden 1999; Heffetz and Shayo 2009). Finally, our paper makes a contribution to the literature on experience goods pricing (Bergemann and Valimaki 2000, 2006).

2 Background and Experimental Design

2.1 Background on Insecticide-Treated Bed Nets

Over the past two decades, the use of insecticide-treated bed nets (ITNs) has been established through multiple randomized trials as an effective and cost-effective malaria control strategy for sub-Saharan Africa (Lengeler, 2004). But coverage rates with ITNs remain low. Until recently, one of the key challenges to widespread coverage with ITN was the need for regular re-treatment with insecticide every 6 months, a requirement few households complied with (D'Alessandro, 2001). This problem was solved recently through a scientific breakthrough: long-lasting insecticidal nets (LLINs), whose insecticidal properties last at least as long as the average life of a net (4-5 years), even when the net is used and washed regularly. The first prototype LLIN, the Olyset Net, was approved by WHO in 2001, but did not get mass produced until 2006. At the time this study started in Kenya in 2007, the Olyset net was not available for sale, and its quality–relative to that of regular ITNs available for sale–was unknown.

More specifically, at the time of the experiment, the "status quo" technology that households in Kenya had access to was a regular ITN, subsidized by Population Services International (PSI). Pregnant women and parents of children under-five could purchase an ITN for the subsidized price of Kenyan shillings (Ksh) 50 ($0.75) at health facilities, and the general population could purchase ITNs for the subsidized price of Ksh 100 ($1.50) at local stores.

In our study sample, 80% of households owned at least one bed net (of any kind) at baseline, but given the large average household size, the coverage rate at the individual level was still low, with only 41% of household members regularly sleeping under a net. About 33% of households had an LLIN of the brand PermaNet® at baseline. The PermaNet® LLINs were received free from the government during a mass distribution scheme targeting parents of children under 5 and conducted in conjunction with the measles vaccination campaign of July 2006, ten months before the onset of this study. These PermaNets differ substantially from the Olyset LLIN used in our experiment: they are circular and not rectangular, made of polyester and not polyethylene, and have a smaller mesh. They cannot be distinguished from traditional re-treatable ITNs with the naked eye, while Olyset nets can. Finally, Olyset nets have been judged to be more comfortable to sleep under than either traditional ITNs or the PermaNet®, thanks to the wider mesh that enables more air to go through (making the area under the net less hot).

2.2 Experimental Design: Phase 1

The experiment was conducted in Busia District, Western Kenya, where malaria transmission occurs throughout the year. In Phase 1, the study involved 1,120 households from six rural enumeration areas. Participating households were sampled as follows. In each area, the school register was used to create a list of households with children.[3] Listed households were then randomly assigned to

[3] Around 90% of households in the study areas have children. Since Kenya introduced Free Primary Education in 2003, school participation is high. In 2007, the year this study started, the net primary enrollment rate was estimated

a subsidy level for an Olyset net. The subsidy level varied from 100% to 40%; the corresponding final prices faced by households ranged from 0 to 250 Ksh, or at the prevailing exchange rate of 65 Ksh to US$1 at the time, from 0 to US$3.8.[4] Seventeen different prices were offered in total, but each area, depending on its size, was assigned only four or five of these 17 prices. Thus, if an area was assigned the price set {Ksh 50, 100, 150, 200, 250}, all the study households in the area were randomly assigned to one of these five prices according to a computer-generated random number. All price sets included high, intermediate, and low subsidy levels. However, the lowest price offered in a given area was randomly varied across areas, and drawn from the following set: {0, 40, 50, 70}. Only two areas had a price set that included free distribution for some households.

After the random assignment to subsidy levels had been performed in office, trained enumerators visited each sampled household. A baseline survey was administered to the female and/or male head of each consenting household.[5] At the end of the interview, the respondent was given a discount voucher for an Olyset net corresponding to the randomly assigned subsidy level. The voucher indicated (1) its expiration date, (2) where it could be redeemed, (3) the final (post-discount) price to be paid to the retailer for the net, and (4) the recommended retail price and the amount discounted from the recommended retail price. Vouchers could be redeemed at participating local retailers (one per area). The six participating retailers were provided with a stock of blue, extra-large, rectangular Olyset nets. At the time of the study, such nets were not available to households through any other distribution channel, which facilitated tracking of the study-supplied nets.

The participating retailers received as many Olyset nets as vouchers issued in their community, and no more. They were not authorized to sell the study nets to households outside the study sample. For each redeemed voucher, the retailers were instructed to note the voucher identification number and the date of redemption in a standardized receipt book designed for the experiment. The list of redeemed vouchers and the vouchers stubs themselves were collected from retailers every 2 weeks.[6]

The subset of households who had redeemed their Olyset voucher were sampled for a short-run follow-up administered during an unannounced home visit 2 months on average after the voucher had been redeemed. During the follow-up visit, enumerators asked to see the net that was purchased with the voucher, so as to ascertain that it was a study-supplied Olyset net. The follow-up survey also checked whether households had been charged the assigned price for the net. Usage was assessed as follows: (1) whether the respondent declared having started using the net, and (2)

at 86% and the gross primary enrollment rate was 113%. We estimate that our sample represents around 80% of all households in the study areas.

[4] A few years prior to this study, the Kenya Central Bureau of Statistics and the World Bank estimated that 68% of individuals in Busia district (the area of study) live below the poverty line, estimated at $0.63 per person per day in rural areas (the level of expenditures required to purchase a food basket that allows minimum nutritional requirements to be met) (Central Bureau of Statistics, 2003).

[5] Whether the female head, male head or both were interviewed and given the voucher was randomized across households. It had little effect on take-up (see Dupas, 2009). All regressions below include controls for the randomized gender assignment.

[6] Participating retailers were not allowed to keep the proceeds of the study Olyset sales. However, as an incentive to follow the protocol, participating retailers were promised a fixed sum of $75 to be paid upon completion of the study, irrespective of the number of nets sold but conditional on the study rules being strictly respected.

whether the net was observed hanging above the bedding at the time of the visit.

2.3 Experimental Design: Phase 2

In a subset of areas (4 out of 6), a long-run follow-up was conducted 12 months after the distribution of the first Olyset voucher.[7] All households in those areas were sampled for the long-run follow-up (both those who had redeemed their first voucher, and those who had not). Data on the (presumed) incidence of malaria in the previous month was collected. Households were also asked if they knew people who had redeemed their vouchers and what they had heard about the net acquired with the voucher. In addition, for those who had redeemed the voucher, usage of the Olyset net was recorded as in the first follow-up.

At the end of the visit, households received a second Olyset voucher, redeemable at the same retailer as the voucher received a year earlier. All households faced the same price (Ksh 150 or $2.30) for this second voucher. The set-up used with retailers was identical to Phase 1.

By comparing the take-up rate of the second, uniformly-priced voucher across Phase 1 price groups, we can test whether being exposed to a high subsidy dampens or enhances willingness to pay for the product a year later. Note, however, that since LLINs have a lifespan of 4 to 5 years, at the time they received the second Olyset voucher, households who had purchased an Olyset with the first voucher in Phase 1 did not yet need to replace their first one. The redemption rate for the second voucher thus measures, for those households, the willingness to pay for an additional Olyset, or the discounted present value of a replacement Olyset (if households wanted to hoard the second Olyset until a replacement was needed).

2.4 Baseline Characteristics and Balance Check

The baseline survey was administered at households' homes between April and October 2007. It assessed household demographics, socioeconomic status, and bed net ownership and coverage. Table 1 presents summary statistics on 15 household characteristics, and their correlation with the randomized Phase 1 price assignment. Specifically, we regress each baseline characteristic on a quadratic in the price faced in Phase 1 and a set of area fixed effects. We report the coefficient estimates and standard errors in columns 3 and 4, as well as the p-value for a test that the two coefficients on the price polynomial are jointly significant (column 5). All of the coefficient estimates are small and none can be statistically distinguished from zero, suggesting that the randomization was successful at making the price assignment orthogonal to observable baseline characteristics. Column 6 shows that randomized assignment to a "high subsidy" level (price $\leq$Ksh 50) is also, as expected, completely orthogonal to household characteristics.

[7] Unfortunately two areas (randomly selected among the four areas without free distribution) had to be left out at the time of the long-run follow-up for budgetary reasons.

2.5 Verifying Compliance with Study Protocol

All households that redeemed their vouchers declared, when interviewed at follow-up, that they had been charged the assigned price when they redeemed their voucher at the shop. This suggests that participating retailers respected the study protocol. What's more, the sales logs kept by participating retailers show that, in total over Phase 1 and Phase 2, 95% of the redeemed vouchers were redeemed by a member of the household that had received the voucher. Only two of the individuals that redeemed a voucher declared having paid to acquire the voucher. This suggests that there was almost no arbitrage between households prior to voucher redemption.

To check whether households sold the Olyset to their neighbor after redeeming the voucher, we conducted unannounced home visits and asked to see the Olyset that had been purchased with the voucher (as mentioned above, the study-supplied nets were easily recognizable). These home visits were conducted after both Phase 1 and Phase 2. Overall, more than 90% of households that had redeemed a voucher could show the corresponding Olyset during the spot check.

3 Experimental Results

The experimental results are shown graphically in Figures 1 and 2, and in Table 2. We find three main results: (1) adoption in phase 1 is very sensitive to own-price; (2) adoption in phase 1 is *positively* affected by exposure to neighbors who received a large subsidy; (3) adoption in phase 2 is not lower among high-subsidy recipients in phase 1; in fact it is somewhat higher.

3.1 Short-run: Phase 1 Adoption

Direct effects Figure 1 presents experimental evidence on the impact of price on Phase 1 adoption. Panel A shows that the demand function is quite steep: take-up is quasi-universal for free vouchers (at 97.5%), but drops to 70% and then 55% when the price goes to Ksh 40 ($0.6) and and Ksh 90 ($1.4), and further drops to around 30% when the price crosses the Ksh 100 threshold ($1.5).

In contrast, Panel B of Figure 1, which shows usage rates (among those who redeemed their voucher) at both the 2-month and 1-year follow-up, suggests that the likelihood that people used the Olyset net does not increase with the price paid. As a result, the adoption rate (purchase $\times$ usage) drops substantially as the price increases (as the subsidy level decreases): after 12 months, adoption is at 90% under the full subsidy regime, just below 60% at the Ksh 50 price point, and lower than 10% when the price is Ksh 250 (see Figure 1, Panel C).[8]

The result that initial adoption is very sensitive to price is consistent with the result obtained among pregnant women by Cohen and Dupas (2010), in a separate study also in Western Kenya. It is also consistent with the results in Tarozzi et al. (2013), who find that regular bed net coverage in Orissa (India) decreases from 51% to 10% when the price increases from free to full.

[8] Attrition at follow-up was not correlated with price, and therefore the estimates of the effect of price on adoption are unbiased.

Compared to other health products, the adoption function we observe is not as steep as that observed by Kremer and Miguel (2007), who find that increasing the price of deworming from 0 to Ksh 20 decreases adoption from 75% to 19%. Adoption of Olyset nets is also much higher overall than that observed by Ashraf et al. (2010) concerning water disinfectant: they find that increasing the price from 300 to 800 Zambian Kwacha (which is equivalent to going from just Ksh 6 to 17) decreases the purchase rate from 80 to 50 percent, but leaves the adoption rate (purchase × usage) unaffected, at roughly 30%.

Spillover effects Given the large differences in take-up across price groups, the random assignment of households to price groups in Phase 1 generates an exogenous source of geographic variation in exposure to Olyset nets. Appendix Figure A1 shows that households that received a voucher for a highly subsidized Olyset typically redeemed it within a few weeks (a few days if they got a free one). In contrast, those who were assigned a high price not only were very unlikely to redeem their voucher, as we have seen above, but also if they did, they took two months to redeem. Thus, across neighborhoods within a given village, the "exposure" to Olyset nets within the first three months the voucher could be redeemed varied with the share of households that received a high subsidy level. Since this share was exogenously determined by the random assignment, we can exploit this variation to estimate social effects without running into the reflection problem (Manski, 1993).

Using GIS coordinates, we compute, for each household in the sample, the number of sampled households that live within a given radius, and the number and share of them who received a voucher for a high subsidy.[9] On average, households have 1.28 neighbors within a 250m radius (4.01 neighbors within 500m, 7.77 within 750m) who received a high subsidy. This represents, at the mean, 22-25% of the study households living within these radii.[10]

Table 2 presents estimates of the spillover effects estimated parametrically through OLS, controlling for own subsidy status. We run the following regression:

$$Y_{hj1} = \beta High_{hj1} + \gamma ShareHigh_{hj1} + \upsilon_j + \varepsilon_{hj1}$$

where $Y_{hj1}$is whether household h from area j bought the Phase 1 Olyset; $High_{hj1}$ is a dummy equal to 1 if household received a high subsidy (price of Ksh 50 or lower) in Phase 1, and υ_j is an area fixed effect. The regressor of interest is $ShareHigh_{hj1}$, the share of neighbors (within a 500m radius – the results are unchanged when we use alternative radii) who received a high subsidy in Phase 1. (We impute this share to be zero if there are no other study households in this radius). In the specifications shown we do not control for the total number of study households within 500 meters but results are unchanged if we do. Finally, since the density measures may be spatially

[9] We use neighbors as proxies for social contacts as we did not map out social networks in the areas of studies. To the extent that our measure of social networks is noisy, this will bias our results downwards. Note however that neighbors are a very important part of social networks in rural Western Kenya. Data collected by Dupas et al. (2013) in the same area of study shows that 68% of women in rural households speak to at least four neighbors daily, and 91% speak to at least four neighbors a few times a week.

[10] Regression estimates confirm that, within village/area, these exposure measures are not significantly correlated with the voucher price (results not shown).

correlated, we present standard errors corrected for spatial dependence in brackets, in addition to presenting the White standard errors in parentheses. We use the spacial dependence correction proposed by Conley (1999).[11]

The results in Table 2, row 2, suggest positive spillovers – if all of a household's neighbors sampled for the study received a high subsidy, the probability of redeeming one's own voucher increases by 22 percentage points. This implies that households are almost 50% more likely to invest in the Olyset net if all of their sampled neighbors received the high subsidy. This is a non-trivial effect since the average price households had to pay for the Olyset in Phase 1 was 109 Ksh ($1.65), close to the average daily wage and a relatively large sum for rural households.[12]

3.2 Long-run: Phase 2 Adoption

Direct Effects The effect of high subsidies on Phase 1 adoption suggests a large potential for it to affect Phase 2 adoption through learning effects. We now test whether households who benefited from a high subsidy in Phase 1 were more or less willing to pay for an Olyset in Phase 2.

Recall that the price of the second Olyset offer was uniform across all households (at 150 Ksh). Panel B of Figure 2 presents the average purchase rate for the second Olyset, for each Phase 1 price. Average take-up appears higher among the higher subsidy groups (Phase 1 price of 0 or 50 Ksh).[13]

Based on this, in column 3 of Table 2, we present results of an OLS regression in which the dependent variable is a dummy for having purchased the Phase 2 Olyset, and the main regressor is a dummy for having received a "high subsidy" in Phase 1 (price $\leq$ 50 Ksh). As was clear from the figures, the effect of having received a high subsidy in Phase 1 has a very large effect on purchase and adoption in Phase 1 (columns 1 and 2), but the effect on Phase 2 purchase is much more modest (column 3). The Phase 2 effect is only significant at the 10 percent level, but is robust to controlling for household-level controls (not shown).[14]

Overall, the evidence points to a positive effect of a high Phase 1 subsidy on Phase 2 adoption, but the effect is not strong, and only at the margin of significance. Note, however, that the take-up of the second voucher among high-subsidy recipients reflects mostly the demand for a second Olyset

[11] Spatial correlation is a concern because two households who live near each other will have overlapping radii. The greater the distance between two households, the smaller the overlap will be. In fact, once the distance between two households reaches $2r$ meters, their r-meters radii will not overlap at all. The Conley covariance matrix allows general correlation pattern for distances shorter than 2r. Specifically, it uses weights that are the products of two kernels, one for each geographic coordinate (longitude and latitude). The kernels go from 1 to zero, decreasing linearly with the distance between the two observations and reaching zero when the distance is $2r$.

[12] We tested for heterogeneity in the strength of the spillover by own-subsidy status by running a specification interacting $High_{hj1}$ and $ShareHigh_{hj1}$. The coefficient on the interaction term was positive but small and insignificant.

[13] Recall that Phase 2 was conducted in only 4 of the 6 areas. Panel A of Figure 2 reproduces Panel A of Figure 1, showing the purchase rate in Phase 1, for the subset of households included in Phase 2.

[14] In a specification estimating separately the effects of getting the full subsidy and the high-but-not-full subsidy on Phase 2 purchases, the point estimates for both subsidy groups appear virtually identical to each other, but given the relative small sample size the standard errors increase and we cannot reject the null for either of them (though the 95% confidence intervals are [−.026; +.162] and [−.044; +.180] and we can reject any negative impact of more than a few percentage points).

net, whereas for most households that received a high price for the first voucher, the take-up of the second voucher reflects the demand for a first Olyset net (since take-up of the first voucher was low at high prices). Under the reasonable assumption that the marginal utility of Olysets is decreasing in the number owned, holding everything constant, the demand for a second Olyset would be lower than the demand for a first Olyset. In other words, the fact that the take-up for the second voucher is not significantly *lower* in the high-subsidy group than in the low-subsidy group by itself suggests that the willingness to pay in the high-subsidy group may have increased.[15]

Survey evidence suggests households who acquired an Olyset had overall a very positive experience with it, suggesting positive learning. Households who had purchased the first Olyset were asked: "In your opinion, how does this Olyset net compare to other nets you may have had in the past?" The great majority (90%) said that the Olyset was better.[16] At the 2-month follow-up, the main (non-exclusive) reasons given for why the Olyset was better concerned the heightened comfort level (37%), the sturdiness (40%) and the health effectiveness (26%). At the 1-year follow-up, the same share of respondents mentioned comfort and sturdiness, but the share mentioning health effectiveness had risen to 40%. Finally, among those who purchased the Phase 1 Olyset, the self-reported willingness to pay for a replacement Olyset was $2.7, much higher than their self-reported willingness to pay $1.6 for an ITN at baseline.

Spillover Effects Does this positive experience trickle to others? The coefficient on social exposure in column 3 of Table 2 suggests that redemption in Phase 2 was *negatively* affected by exposure via neighbors. This is somewhat surprising given the static spillover effects – if exposure via neighbors increased experimentation in Phase 1 and there are learning effects, then we should expect a reduced form effect from exposure to subsidized neighbors on Phase 2 adoption. The fact that we don't could be driven by people reacting to the health spillovers over time: people with more neighbors using an Olyset get convinced to invest in one themselves, but as the malaria transmission rate decreases over the course of the year in areas with higher Olyset coverage, the private returns to investing in prevention decrease in those areas. Consistent with this interpretation, we find that greater exposure to highly subsidized neighbors lowered the probability that a household invested in *two* Olyset nets (column 4 of Table 2). Estimates of the health effects in Table A1 indeed suggest positive health spillovers, though statistical power is very limited.[17]

[15] Follow-up data on the usage of the Olyset net obtained with the second voucher suggests that the second Olyset had indeed lower immediate returns for households: the Olyset acquired with the second voucher was 23% more likely to still be in its package at the time of the follow-up visit two to four months later (potentially suggesting that part of the demand for the second Olyset was driven by hoarding, since that type of net was not available on the market at the time of the study). Respondents who had their Olyset in its package reported storing it for the future. As long as the discount factor is less than 1, this implies lower returns (everything else constant) to the second Olyset.

[16] The rate was 96% among those who had started using the Olyset, and 70% among those who had purchased it but not yet started using it.

[17] Table A1 presents regression results where the unit of observation is an adult, the dependent variable is whether the person is reported as having had a malaria episode in the month preceding the 1-year follow-up survey, and the regressors of interest are own and peers' Phase 1 subsidy status. There are up to two observations per household (the husband and the wife). Standard errors are clustered at the household level.

4 Mechanisms: Structural Model and Estimation

The results so far strongly suggest that a one-time subsidy for the Olyset net *did not reduce* future willingness to pay. This means that potential negative anchoring effects of subsidies were, if present, overwhelmed by a positive learning effect, but the reduced form results do not enable us to separately identify the magnitudes of each these effects separately. What's more, we find some evidence that exposure to highly subsidized neighbors reduces long-term adoption, possibly by reducing the need for prevention, creating a second channel through which the reduced form effect of a short-term subsidy on long-run adoption is providing a lower bound on learning. In this section, we tease out these various countervailing forces and separate out the learning by doing from other effects by imposing some structure and jointly estimating Phase 1 and Phase 2 decisions.

4.1 Model

We consider a very stylized model. There are two periods and one preventative health product. In each period, households invest in the product if the expected utility gains outweighs the utility costs.

Households' utility is composed of two additive terms: intrinsic utility and gain-loss utility. Intrinsic utility is a function of absolute outcomes, expected private benefits and private costs. Gain-loss utility captures reference-dependence: households can experience utility losses when they pay more for the product than expected, and utility gains when they pay less than expected.

The expected private benefit from adopting the product depends on the disease burden, own vulnerability, and beliefs about the quality of the product. The quality itself depends on a number of factors, such as sturdiness, comfort, and effectiveness at preventing infection, but for simplicity we assume that individuals aggregate their beliefs about these different factors into this one attribute we call "quality".

We allow for two forms of spillovers: health spillovers (e.g., if bed net coverage reaches a certain threshold in the community, the disease risk is reduced); and information spillovers (people can get a signal about the product's quality from their neighbors). We assume that households are myopic – they do not engage in strategic experimentation nor anticipate health spillovers. They do however observe accurate information on the disease burden in real time.

When the product is first introduced at the beginning of period 1, the disease burden is high and vulnerability to risk is maximal for everyone. Households do not know the quality of the product, and they do not have a reference point for the price of the new product.

Households who acquire the product in period 1 get information about some of its characteristics (e.g., sturdiness, comfort) and immediately update their beliefs about its quality and thus the private returns to using it. This information about the product's basic characteristics can diffuse spatially and affect other households' decision to adopt the product in period 1.

In period 2 (a year later), households face a new price for the product. They now take the price they faced in period 1 as reference price. The disease risk has changed based on the local level of

adoption in period 1. What's more, households who adopted in period 1 are less vulnerable to risk since they own one unit of the product. They also updated their beliefs about the product's quality based on their health outcome in the previous year. The health outcome is privately observed, however, thus learning from experimentation does not diffuse spatially.[18]

4.1.1 Formal Set-up and Notations

Household h invests in the product in period t if the expected utility gain outweighs the utility costs:

$$E(R_t) > \varepsilon_{ht} + ap_t + f(p_{rt} - p_t)$$

where $E(R_t)$ denotes the expected health gain (in utility terms) of using the health product in period t. ε_{ht} is a household- and time-specific preference shock, p_t is the price at which the product is offered in period t, a is the marginal utility from income, and $f(p_{rt} - p_t)$ is the gain-loss utility.

We consider the following linear form for the gain-loss utility (dropping the time subscripts): $f(p_r - p) = r \times (p - p_r)$ if $p_r \geq p$ (gains) and $f(p_r - p) = \lambda r \times (p - p_r)$ if $p_r < p$ (losses), where p_r is the reference price, r is that we call the reference-dependence parameter (which can be interpreted as the weight attached to gain-loss utility), and λ is the loss aversion parameter.[19]

Finally we consider $E(R_t) = E(m)\gamma_t v_{ht}$, where m is the quality of the product, γ_t is the disease risk in the area and v_{ht} is the vulnerability of household h to this disease risk at time t. The disease risk γ_t depends on the overall rate of adoption of the health technology among neighbors in the previous period, but households are myopic and do not anticipate these health spillover effects. A household's vulnerability to the disease risk, v_{ht}, depends on the household's ownership of the product: if the household does not own a long-lasting bed net, adopting one has a higher return ($v_{ht} = 1$) than if it already has one ($v_{ht} < 1$).

4.1.2 Period 1 Adoption

When the product is introduced at the beginning of period 1, households have a prior about its quality m, a distribution $\mathcal{N}(\mu, s_0^2)$. They take the price they observe in period 1 as the reference price therefore the gain-loss utility term is zero. Given the disease burden γ_1 and vulnerability $v_{ht} = 1$, household h buys in period 1 if:

$$\mu\gamma_1 > \varepsilon_{h1} + ap_1$$

Note that households in this model are myopic: they do not consider the motive of adopting in

[18] This is a strong assumption. We make it because it would not be possible to estimate separately the health effectiveness information spillover from the health spillover since the same variation in neighborhood coverage drives both. Since empirically it seems the effect of the health spillover dominates, we ignore spillovers in learning about health effectiveness, but we acknowledge this as a limitation.

[19] This linear form for gain-loss utility means no diminishing sensitivity. We have estimated the model using a power gain-loss function (Tversky and Kahneman 1992) and various levels for the degree of diminishing sensitivity, and the results are unchanged. We show the results with the linear assumption for simplicity.

period 1 in order to learn more about the quality of the technology. This assumption is reasonable in our context since the product we consider in the experiment was not available on the market at the time, and households did not anticipate that we would come back after one year to offer the product again.[20]

Households who acquire the product in period 1 get information about its characteristics and immediately (and homogeneously) update their beliefs about its quality: their prior on m shifts from the distribution $\mathcal{N}(\mu, s_0^2)$ to the distribution $\mathcal{N}(\mu + l_1, s_1^2)$.

During the course of period 1 (which in the experiment corresponds to the 3-month window during which the voucher for the first Olyset could be redeemed), households that did not immediately get the product themselves can learn about it from others. We consider the following social diffusion process: a household with n owner households within a 250-meter radius, N households total in that radius, and c social contacts among them, has a chance $1 - (1 - \frac{n}{N})^c$ to update its beliefs to $\mathcal{N}(\mu + l_1, s_1^2)$.[21] If they learn, they update their purchase decision and buy in period 1 if:

$$(\mu + l_1)\gamma_1 > \varepsilon_{h1} + ap_1$$

4.1.3 Period 2 Adoption

In period 2 (a year later), the disease risk has changed based on adoption in period 1. Specifically, we consider that $\gamma_2 = \alpha\gamma_1$, with $\alpha < 1$ if local take-up of the product in period 1 is above a certain threshold t, and $\gamma_2 = \gamma_1$ otherwise. This threshold effect reflects the shape of health spillovers identified in the medical literature (e.g. Hawley et al., 2003; Killen et al. 2007).

What's more, households have updated their beliefs about the product's quality. Those who adopted in period 1 received a private signal $r_{1h} = m + \nu$ with $\nu \sim \mathcal{N}(0, k^2)$ and update their belief using Bayes' law; they adopt in period 2 if:

$$(\mu + l_1 + l_2)\gamma_2 v_2 > \varepsilon_{h2} + ap_2 + f(p_1 - p_2)$$

where $l_2 = \frac{s_1^2}{\sqrt{k^2+s_1^2}} \frac{r_{1h}-\mu-l_1}{\sqrt{k^2+s_1^2}}$ is how much they updated their belief on the mean quality, and $v_2 < 1$ indicates that the returns to the second Olyset are lower than those of the first Olyset net.[22]

[20] For products that are available on the market, forward-looking households could invest in a product even if the myopic gains are outweighed by the costs, for the option value of learning. The magnitude of this option value will depend on beliefs with respect to the long-run price, but non-linearly: If people expect the price in the long-run to be outside their budget set, then the option value of learning is zero. If they expect the long-run price to be free, the option value is also zero. But at intermediate price beliefs, the option value of learning will be positive. To the extent households are forward-looking this way, the scope for subsidies to affect short- and long-run adoption will be reduced.

[21] The probability to learn l_1 from a social contact who owns the product is 1. The probability that a given social contact owns the product is $\frac{n}{N}$. The chance that the household does *not* learn l_1 from a given social contact is thus $(1 - \frac{n}{N})$, and the chance of learning it from at least one contact is $1 - (1 - \frac{n}{N})^c$.

[22] The model assumes that all those who acquire the product in the first period experiment with it and get both signals (l_1 and l_2) on quality. We make the simplification of equating acquisition to usage since it is what we observe empirically (remember Figure 2, panel B). For products that are more costly to use than anticipated (e.g. people only discover how badly chlorinated water tastes once they have tried it once), it is possible that the initial learning l_1 is negative – that is, the mean prior decreases right after acquisition, and some households may after that not find

Finally, households that did not adopt the product in period 1 adopt in period 2 if:

$$\gamma_2 \times (\mu + l_3) > \varepsilon_{h2} + ap_2 + f(p_1 - p_2)$$

where $l_3 = 0$ for those who did not learn about the product's characteristics from a neighbor over the course of the year, and $l_3 = l_1$ for those who did (since we assume that adopters discuss the observable characteristics of the product with their neighbors, but not their private health signals).

4.2 Estimation

We estimate the model by Maximum Likelihood. To compute the likelihood function we assume that the distribution of the *iid* preference shocks ε_{ht} is logistic, such that at any period t the probability that a household purchases the product is:

$$\begin{aligned} \Pr(T = 1) &= Pr(\varepsilon_{ht} < E(R_t) - ap_t - f(p_{rt} - p_t)) \\ &= \frac{1}{1 + exp[-E(R_t) + ap_t + f(p_{rt} - p_t)]} \end{aligned}$$

We allow for area fixed effects and estimate the following five parameters of interest: (1) μ : the prior on quality; (2) l_1 : how much the mean prior on quality changes upon acquisition – this is the learning from product characteristics, which can be transmitted through social learning; (3) l_2 : how much the mean prior on quality changes upon experimentation over a year; (4) r: the reference-dependence parameter; and finally, (5) a: the marginal utility of income (the higher $\hat{a}$, the more sensitive to price the demand is). Note that failing to reject the null for r does not necessarily imply individuals do not exhibit reference-dependence, only that there is no loss aversion over the period 1 price (i.e. there is no anchoring around the period 1 price).

Assumptions We impose the following values in our benchmark estimation (and test for sensitivity of the results to these assumptions in Figure A2):

- Based on the medical literature cited above, the threshold for health spillovers is $t = 0.6$ over a 500-m radius and the strength of the spillover effects is $\alpha = 0.8$. In other words, if Olyset net coverage reaches at least 60% among study households living within 500m of household h, the disease risk faced by household h after a year is reduced by 20%.[23]

- The return to the second Olyset net is 85% that of the first one: $v_2 = 0.85$. This could be because the second Olyset is not put to use immediately and the discount factor is 0.85, or because there are diminishing returns to individual Olyset coverage within a given household

it worthwhile to use the product (as found in Ashraf et al., 2010). In that case, they would not receive the private signal r_{1h} and their period 2 decision would depend on their downward-revised prior on quality, the distribution $\mathcal{N}(\mu + l_1, s_1^2)$. This would reduce the scope for subsidies to boost adoption in the long-run.

[23] The thresholds identified in the literature are 50% (Hawley et al., 2003) and 35-65% (Killen et al., 2007). Here we take a slightly higher threshold of 60% since our sample includes only around 80% of the total population in the areas of study.

(since the insecticide halo generated by one Olyset can be sufficient to keep mosquitoes at bay from the entire sleeping area).[24]

- The loss aversion parameter is set at $\lambda = 1.6$. (A recent review by Booij, van Praag, and Kuilen (2010) shows the range of estimates forλ in the literature is [1.07; 2.61] (Table 1)).
- We set the number of social contacts at $c = 4$. This is based on the evidence from Dupas et al. (2013) discussed in footnote 9.

Identification Separate identification of the learning from experimentation (l_2) and reference dependence parameter (r) is made possible by the random variation in period 1 price (which creates random variation in both experimentation and reference points) along with the random variation in the share of neighbors within a 500-m radius receiving the high subsidy. This latter variation generates random variation in coverage density and thus in the disease environment (and thereby in the returns to investing in an Olyset net) in Phase 2, holding own Phase 1 price constant. Thus different rates of take-up among households facing the same price sequence but a different disease environment helps pin down the learning effect.

The random variation in the share of neighbors within a 250m radius receiving a high subsidy (and therefore the share of neighbors who acquire the product immediately upon receiving the voucher) enables estimation of l_1 from the Phase 1 adoption decision. The random variation in subsidy levels across neighbors within a 250m radius also affects the ownership rate at the onset of Phase 2, but this alone cannot be used to estimate l_1 (or to estimate information spillovers about health effectiveness if we allowed them) from the Phase 2 adoption decision of those who did not purchase in Phase 1, since by then it is counfounded by the health spillover (the change in the disease burden).[25]

Results The results under the benchmark specification are presented in Table 3. The estimate of the reference-dependence parameter (r) is small economically – at only 6.6% of the estimated value of the marginal utility of income (a).

In contrast, the total learning effect after a year worth of experimentation ($l = l_1 + l_2$) is very large: comparing the value of the estimate to that of μ, it corresponds to an increase in perceived quality of 41%. In monetary terms, this total learning effect increases demand in period 2 as much as decreasing the price from Ksh 150 to Ksh 105 (so it corresponds to the effect of a 30% price drop). Interestingly, the short-term learning from product characteristics (l_1), which can be transferred to neighbors, makes up only about 31% of the total learning effect; it is significant at conventional

[24] We picked 0.85 because it is an upper bound for discount factors elicited experimentally (see for example Schaner (2013) for a mean estimated *weekly* discount factor as low as 0.70 in rural Kenya, and Harrison et al. (2002) for an average discount rate of 28% (equivalent to a discount factor of 0.78) in Denmark). Sensitivity of the results to the value of this parameter is presented in figure A2 and discussed below.

[25] We use the 250m radius for the information spillover vs. the 500m radius for the health spillover to help with the identification. The neighborhood variables are obviously highly correlated between the 250 and 500m radii, but not identical, and the variation between the two (which is random) is useful for identification.

levels (the p-value in the benchmark case is 0.171). The magnitude of l_1 means that learning about the product from neighbors increases demand by about as much as a 9% price drop.[26]

Sensitivity We test the sensitivity of these results in Figure A2. We plot the coefficient estimate and the 90% confidence interval over a large range of possible values for the five parameters that are imposed rather than estimated. Overall, the results appear very robust, although looking at the sensitivity analysis helps visualize which assumption matters for the identification of the coefficients of interest. Clearly, the estimate of learning through year-long experimentation (l_2) directly depends on the assumption on diminishing returns – if there are no diminishing returns ($v_2 = 1$), the learning effect is much smaller. The estimate of the reference-dependence parameter is sensitive to assumptions on the shape and magnitude of the health spillovers – as spillovers reduce in strength ($\alpha \to 1$), the reference-dependence parameter increases, but it remains small in economic terms, and if $\alpha = 1$ we lose separate identification of the reference-dependence parameter and the learning from experimentation. If we knock-off the informational spillovers by setting the number of social contacts (c) at zero or very close to zero, the two learning parameters (learning from characteristics and learning from experimentation) cannot be separately identified, though their sum remains unchanged.

5 Discussion: External Validity

Our results suggest that a one-time, introductory subsidy for long-lasting insecticide-treated nets enabled learning, and this learning *boosted* willingness to pay for them a year later, holding the disease risk constant. In this section we discuss how generalizable this finding is. We ask three questions: (1) Did the experimental design limit the scope for anchoring around subsidized prices? (2) Are there cross-product entitlement effects of subsidies? and (3) For what types of health products and contexts would we expect the same results to obtain?

Did the experimental design limit the scope for anchoring?

We find no meaningful evidence that people anchor around subsidized prices. Is that a true result or is it an artifact of the experimental design? Two experimental features could have limited the scope for anchoring effects in our setting. First, subsidies were randomized across households within village. Since households may have noticed their neighbors received a different subsidy level than theirs, this could have limited the salience of one given price around which to anchor. It is worth noting that most subsidy programs have some form of targeting rules, however, also yielding

[26] The model does not allow for an income (via health) effect. The one-year follow-up survey data however suggests that the incidence of malaria among household heads (either the male or the female) may have been lower among households who received a high subsidy in Phase 1, though we do not have enough statistical power to reject the null of no health effect (see Appendix Table A1). A health effect among household heads could potentially have generated an income effect. We do not have data on income to directly test for an income effect, which means that in the estimation any potential income effect is picked up by the learning from experimentation parameter (l_2). Our estimate of learning may therefore be an overestimate.

heterogeneous pricing across households within village. For example, subsidies are often targeted based on demographics (presence of a pregnant woman in the household or number of children) or means-tested, and how much people anchor under such programs might be similar to what we observe in our experimental setting.

Second, recall from section 2.3 that the recommended retail price and the amount discounted from the recommended retail price were indicated on the Phase 1 vouchers. This may have reduced the potential for anchoring. From a policy standpoint, indicating the non-subsidized price on a voucher or product is relatively costless and quite common (this was the case in the Ashraf et al. (2010) experiment with chlorine), therefore estimating the overall effect of subsidies in the presence of full information about the non-subsidized price is of direct policy interest. That said, it would be useful for future research to test the extent to which anchoring effects are at play in the absence of such information. For products such as bed nets, whose retail value tends to be known, it may not have mattered, but for less known products anchoring effects might be larger.

Cross-Product Entitlement Effects?

Another potential worry is that subsidies for one product may lead to entitlement effects vis-a-vis other products. In particular, households might expect that the government or NGO that subsidized product A will also soon start to subsidize product B (if product B belongs to the same class of product, say health products), and thus adopt a "wait and see" stance. To test whether this is the case in the Kenyan context, in the two areas where the high subsidy was a full subsidy, we distributed vouchers for partially subsidized WaterGuard, around 5 months after the first Olyset voucher was distributed. The results are shown in Column 5 of Table 2. Take-up of WaterGuard was not lower among recipients of free Olysets (in fact it was slightly higher though insignificantly so), suggesting no cross-product entitlement effects. In other words, households who get a chance to receive a free Olyset did not expect that other health technologies should be given to them for free in order for them to experiment with them.

Which health products do these results apply to?

Would the results obtained for bed nets apply for other preventative health products? Besides vaccines, which are already universally subsidized, the other key products for which subsidies are commonly discussed are water purification products (chlorine solutions and filters), cookstoves, and deworming medicines.

The first potential difference between these products and bed nets concerns the level and accuracy of priors on the returns to using the product. As mentioned earlier, the Olyset, the bed net considered in the study, is much more comfortable than earlier generations of bed nets. To the extent that people assume all nets are equally comfortable, people's priors on the private returns to using Olysets were therefore likely to be underestimates. In contrast, both water disinfectants and deworming pills have important negative side effects (water disinfectant makes the water tastes like chlorine, while deworming treatment makes children nauseous for a few days). It is unlikely

that households without prior exposure to these products would anticipate such side effects, and therefore their priors on the returns are likely overestimates. With regards to cookstoves, evidence suggests that households tend to overestimate how difficult it is to adapt one's cooking to the new stove, thus they may underestimate the returns to switching.

The second difference is in the durability of the product, and hence the potential for a one-off subsidy to enable learning about the health effectiveness of a product. A bottle of water disinfectant lasts only about 1 month for a standard household, whereas deworming treatment needs to be repeated only every 6 months, and bed nets, cookstove and water filters have a lifespan of multiple years. A mother who got a free sample of water disinfectant is unlikely to have learned much about the effectiveness of the product when she needs to make a repurchase decision a month later. In contrast, by the time a bed net, filter or cookstove needs to be replaced a few years later, households will have had ample time to observe their impact on health.

A third dimension concerns the magnitude of the health externality. The health externality is low for water disinfectants, water filters and cookstoves, high for deworming, and high for bed nets but only above a certain threshold (Hawley et al., 2003; Killen et al. 2007).

Putting all this together, we make the following conjectures:

1. One-time subsidies for cookstoves and water filters have the potential to boost subsequent adoption through learning effects. This boosting effect will be higher than for the Olyset net, given that health externalities are lower for these products, thus private returns remain large even when coverage rates are high.

2. A one-time subsidy for water disinfectant is unlikely to have a meaningful impact on subsequent adoption: it will have a possibly negative learning effect (people learn chlorine tastes bad, but within one month they don't learn it reduces diarrhea). This is in line with the empirical evidence to date: Ashraf et al. (2010) find that Zambian households who are enticed to buy one bottle of disinfectant when it is subsidized end up not using it to purify their water, suggesting a potentially negative learning effect (they were put off by the chlorinated taste) or no learning at all (they did not even try it). Dupas et al. (2013) look at the long-run impact of giving just one free bottle of water disinfectant to mothers of young children in Kenya, and, while they see an increase in short-run adoption among subsidy recipients, they find no effect whatsoever on the probability that households use water disinfectant two years later. A longer subsidy (repeated free trials), on the other hand, appears to boost long-run adoption.

3. A one-time subsidy for deworming is likely to *reduce* subsequent adoption: it will have a negative learning effect. This is also in line with the evidence to date: Kremer and Miguel (2007) observe lower adoption rates of deworming treatment among households who have more social contacts who received a deworming subsidy, and argue this is driven mostly by households learning that the private returns to deworming are outweighed by private costs.

6 Conclusion

It is often argued that subsidies for high-return technologies or products in the short-run might be detrimental for their adoption in the long run. There are two main arguments: (1) subsidies may not foster learning about the technology nor improve health if subsidy recipients do not use it; and (2) previously encountered prices may act as "anchors" that affect people's valuation of a product independently of its intrinsic qualities.

This paper used a randomized field experiment to estimate the effect of a one-time, targeted subsidy on the long-run adoption of a new health product (the long-lasting antimalarial bed net Olyset) which is both more comfortable and more effective than its predecessor. We find that temporary subsidies for a subset of households increase short-run adoption rates among both subsidy recipients and their neighbors, and subsequently increase willingness to pay for bed nets through learning effects that appear to trump any potential anchoring effect. Structural estimation of an experience good model that allows for both information and health spillovers generate results consistent with an important learning from own experimentation effect, no anchoring around subsidized prices, and positive social diffusion effects of product characteristics.

The extent to which the adoption of new products is affected through "free trial" periods and how it diffuses through neighbors or friends is a central question, especially for less developed economies where modern diffusion channels, such as TV commercials, do not reach the great majority of the population. The empirical evidence provided in this paper suggests that, at least for some class of preventative health products, learning by doing and social learning are important channels through which short-term, targeted subsidies can affect long-run adoption. The extent to which these results would apply to curative health products may vary depending on the availability of information on the true underlying cause of illness (Adhvaryu, 2012; Cohen et al., 2013) and the counterfeit prevalence (Björkman et al., 2012).

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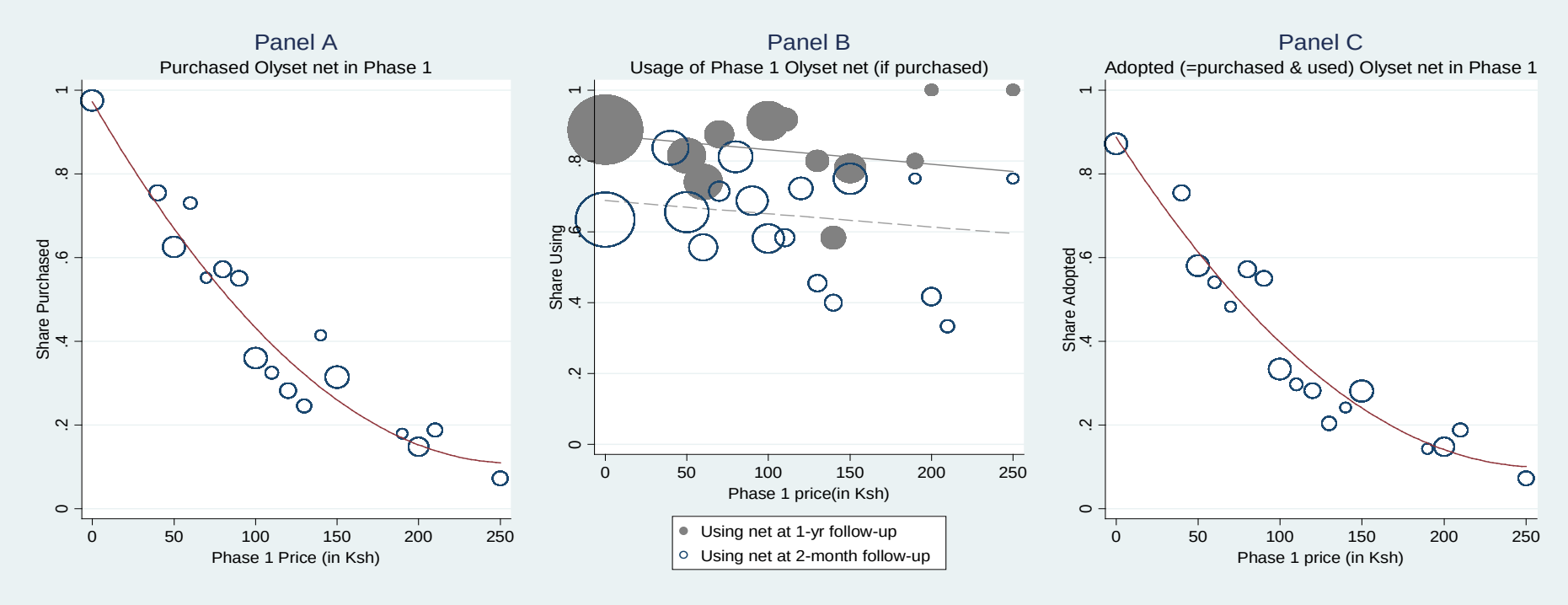
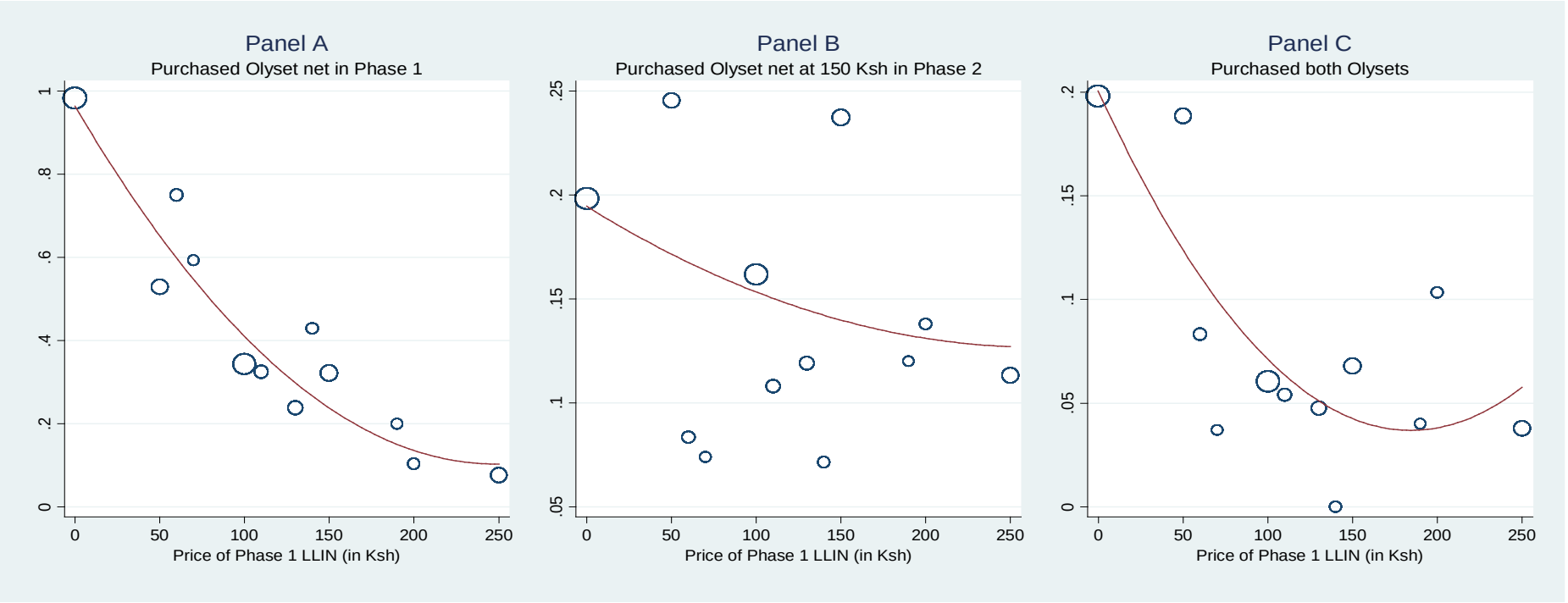
Figure 1: Effects of Phase 1 Price Subsidy on Phase 1 Adoption



Notes: Data from 1,120 households (Panels A and C), 479 households (Panel B, hollow circles), 273 households (Panel B, solid circles). The size of the circles reflects the relative size of the sample at each price point. The lines are quadratic fits (Panels A and C) or linear fits (panel B). The 1-yr follow-up was conducted in only 4 of the 6 study areas. Usage is self-reported (see Table 2 for results on observed usage.) The exchange rate at the time of the study was around 65 Ksh to US$ 1 .

Figure 2: Effects of Phase 1 Price Subsidy on Phase 2 Adoption



Notes: Data from 599 households in the four areas sampled for Phase 2. Panel A reproduces Panel A of Figure 1 for the subsample included in Phase 2. The size of the circles reflects the relative size of the sample at each price point. The lines are quadratic fits.

Table 1. Baseline Characteristics of Participating Households

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Household (HH) demographics	Sample Mean	Sample Std. Dev.	*OLS Coeff on* Phase 1 Price (in US$)	*OLS Coeff on* (Phase 1 price in US$) squared	*P-value Joint Test* (Price and Price Squared)	*OLS Coeff on* High Subsidy in Phase 1	N
Household size	7.1	2.7	-1.207	-0.919	*0.145*	-0.586	1112
			(.536)	(.443)		(.393)	
Age of Household Head	45.7	13.4	-1.608	-2.311	*0.065*	-1.064	1079
			(2.608)	(2.165)		(1.912)	
Number of children (under 18) currently living in household	5.4	2.9	-0.747	-0.606	*0.490*	-0.299	1120
			(.552)	(.456)		(.405)	
Socio-Economic Status							
Female head has completed primary school	0.25	0.43	-0.068	-0.013	*0.765*	-0.020	1116
			(.084)	(.07)		(.062)	
Number of household members with an income-generating activity	1.8	1.0	-0.247	-0.094	*0.071*	-0.2	1112
			(.203)	(.168)		(.149)	
Household assets index value (in US $)	338	325	-30.866	6.329	*0.800*	-30.097	1120
			(62.897)	(51.957)		(46.132)	
Electricity at home	0.02	0.14	0.013	0.019	*0.791*	0.004	1108
			(.027)	(.022)		(.02)	
At least one member of HH has a bank account	0.12	0.33	-0.071	-0.045	*0.520*	-0.016	1116
			(.064)	(.053)		(.047)	
Bednet Ownership at Baseline							
Number of bednets owned	1.7	1.5	-0.226	-0.274	*0.636*	-0.051	1112
			(.292)	(.241)		(.214)	
Share of HH members that slept under a net the previous night	0.41	0.37	-0.038	-0.047	*0.615*	0.002	1112
			(.072)	(.059)		(.052)	
HH owns a circular PermaNet LLIN[a]	0.33	0.47	-0.169	-0.195	*0.520*	-0.068	538
			(.132)	(.141)		(.076)	
HH ever received a free bednet	0.32	0.47	-0.108	-0.095	*0.533*	-0.027	1112
			(.091)	(.075)		(.067)	
Has ever shopped at shop where voucher has to be redeemed	0.62	0.49	-0.036	0.029	*0.609*	-0.035	1110
			(.085)	(.071)		(.062)	
Declared willingness to pay for a bed net (in US$)	1.56	1.55	-0.285	-0.003	*0.353*	-0.202	1100
			(.306)	(.252)		(.225)	
Distance from shop where voucher has to be redeemed (in km)	1.86	1.58	-0.677	-0.534	*0.108*	-0.334	1094
			(.302)	(.249)		(.222)	

Notes: Columns 3 and 4 show coefficient estimates and their standard errors for two independent variables (the Phase 1 price, column 3, and its square, column 4) estimated through a common linear regression (one for each row) with area fixed effects. Column 6 presents coefficient estimates from a separate OLS regression with area fixed effects for each row. Standard errors are presented in parentheses.

[a] The LLINs subsidized during the experiment were family-size rectangular Olysets.

Table 2. Experimental results

	(1)	(2)	(3)	(4)	(5)
	Purchased Olyset net in Phase 1	Purchased and Used Olyset net in Phase 1[a]	Purchased Olyset at 150 Ksh in Phase 2	Purchased both Olysets	Purchased Waterguard[b]
Phase 1 Price ≤ 50 Ksh (High Subsidy)	0.387	0.281	0.068	0.115	0.046
	(0.034)***	(0.033)***	(0.040)*	(0.031)***	(0.065)
	[0.034]***	[0.033]***	[0.04]*	[0.033]***	[0.052]
Density of Phase 1 High Subsidy recipients within 500 meter radius	0.223	0.168	-0.183	-0.102	-0.156
	(0.092)**	(0.090)*	(0.099)*	(0.078)	(0.144)
	[0.083]***	[0.088]*	[0.119]	[0.104]	[0.145]
Area Fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1094	1094	584	584	256
Mean of Dependent Variable	0.458	0.321	0.158	0.094	0.434
Mean of Dependent Variable in non-"High Subsidy" group	0.341	0.233	0.137	0.057	0.411

Notes: Linear probability model estimates. All regressions include enumeration area fixed effects and control for two cross-cutting randomized treatments (gender of voucher recipient and framing) discussed in Dupas (2009). 26 (15) observations are dropped for phase 1 (phase 2) because they do not have valid GIS data to compute the social exposure variables. White standard errors presented in parentheses. Standard errors corrected for spatial dependence are presented in brackets. * indicates significance at 10%; ** at 5%; *** at 1%. Results are unaffected if household-level characteristics shown in Table 1 are controlled for. Results are also unaffected if a control for population density (total number of study households within 500 meter radius) is controlled for.

[a] "Purchased and Used Olyset in Phase 1" is a dummy equal to 1 if the household redeemed the 1st Olyset voucher and the net was seen hanging during at least one of the two surprise follow-up visits.

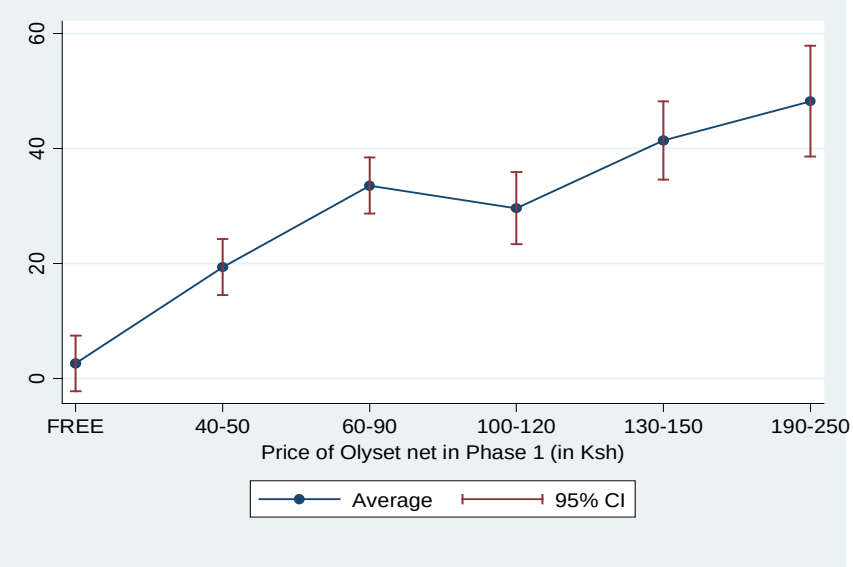
[b] Waterguard is a water purification product. Vouchers for Waterguard subsidized at 50% were offered 5 months after the first Olyset voucher was distributed, to test for the presence of cross-product entitlement effects. This Waterguard voucher sub-experiment was conducted in only the two enumeration areas where the High Subsidy was a full subsidy. See text section 5 for details.

Table 3. Maximum Likelihood Estimates

		Estimate	Std. Err.	
Prior on effectiveness	mu	2.423	0.295	***
Learning from characteristics	l_1	0.309	0.226	
Learning from experimentation	l_2	0.696	0.313	*
Marginal utility from income	a	0.0219	0.0018	***
Reference-dependence parameter	r	0.0015	0.0012	
Total learning effect	$l = l_1 + l_2$	1.005	0.323	***

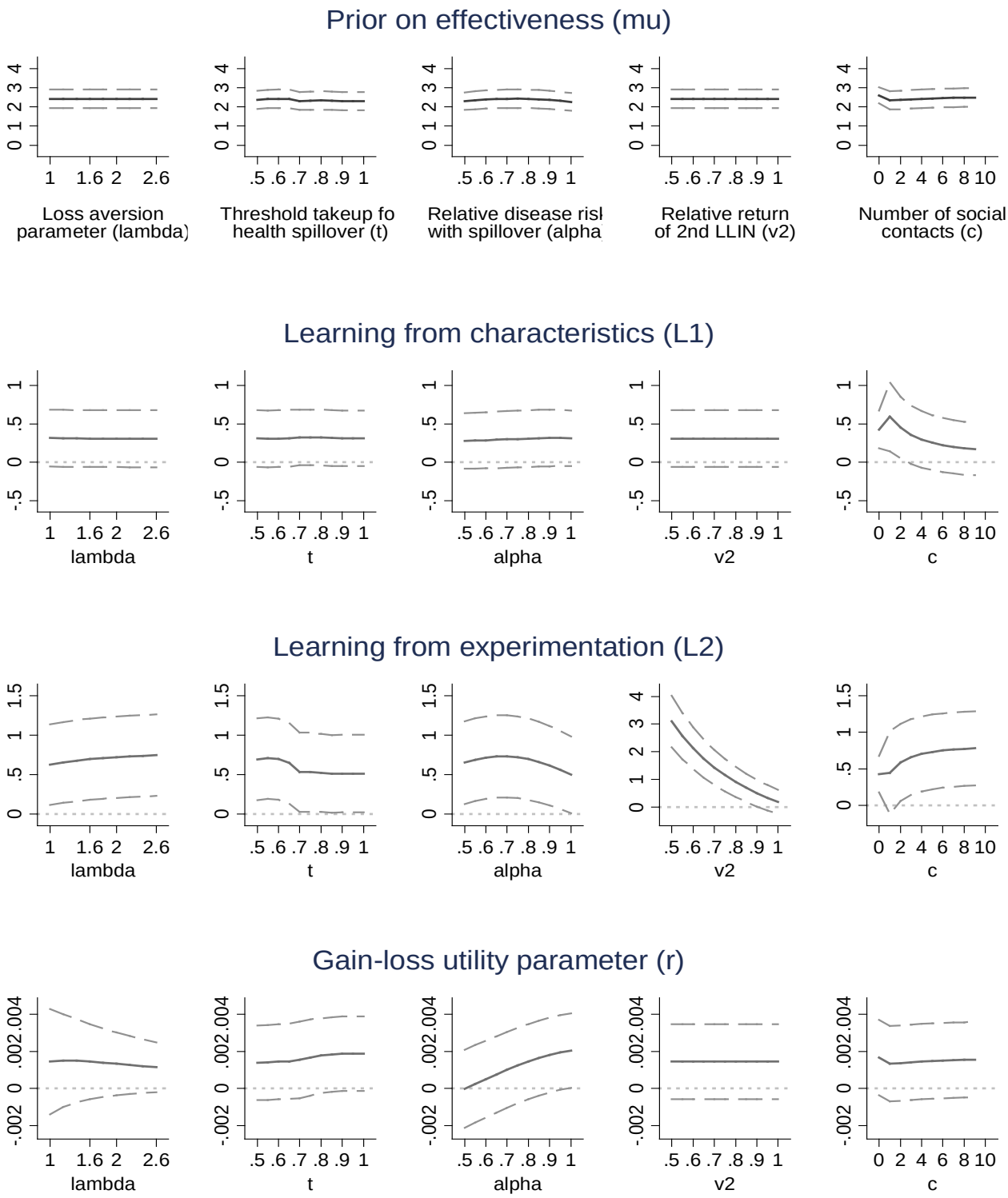
Notes: Sample includes 584 households with valid GIS data in the 4 areas sampled for Phase 2. Estimates from the benchmark model under the values 1.6, 0.6, 0.8, 0.85, and 4, for, respectively, the following parameters: loss aversion λ, health spillovers threshold t, spillover effect a, relative return of second net v_2 and number of social contacts c. Sensitivity of the estimates to these values is presented in Figure A2.

Figure A1
Number of Days needed to Redeem Phase 1 Olyset Voucher, by Phase 1 price group



Notes: Data from 479 households that redeemed their Phase 1 voucher.

Figure A2
Sensitivity of Maximum Likelihood estimates



Notes: This graph shows how the ML estimates vary as the imputed values for the scalars λ, t, a, v_2 and c change. For each of these, we re-estimated the ML estimates for μ, l_1, l_2, r and a under 11 possible values over the range shown on the x-axis, holding the other 4 scalars at their benchmark values. Benchmark values are: λ=1.6, t=0.6, a=0.8, v_2=0.85 and c=4.

Table A1. Health Effects

	(1)	(2)
	Dep. Var: Had malaria in the month preceding the 1-yr Follow-up Survey	
Phase 1 Price ≤ 50 Ksh (High Subsidy)	-0.027	-0.025
	(0.023)	(0.023)
Share of study households with High Subsidy within 500m radius	-0.079	-0.083
	(0.057)	(0.058)
Total # of study households within 500m radius	-0.002	-0.002
	(0.001)*	(0.001)*
Observations	937	906
Household-level controls	No	Yes
Mean of Dep. Variable in non-"High Subsidy" group	0.098	0.098

Notes: Sample restricted to the four areas where the first year follow-up was conducted for both redeemers and non-redeemers of the Phase 1 voucher. Coefficient estimates obtained using linear regression with area fixed effects and gender fixed effects. Sample includes up to two observations per household (male and female head). Standard errors are clustered at the household level. Price varies from 0 to US$3.8. Household level controls in column 2 include all variables presented in Table 1.
*Significant at 10%; ** significant at 5%; *** significant at 1%.