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

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**Abstract**

Distribution of free or highly subsidized health products is common in poor countries, often financed by foreign aid. But large subsidies in the short run might be detrimental for product adoption in the long run. There are two main negative effects typically posited. First, previously encountered prices may act as "anchors" that affect people's willingness to pay for a product independently of its intrinsic qualities. Second, subsidizing a product makes it accessible to people who, because they do not value it enough to pay a high price for it, may not use it correctly (or use it at all) and thereby may lower beliefs about the product's effectiveness. We use a randomized field experiment to test for the presence of these two possible (negative) effects of short-run subsidies, and estimate their relative importance vis-à-vis the standard (positive) effects expected for new products: learning-by-doing and social learning. We find that, for a health product that has high private returns (the antimalarial bednet), neither of the two postulated negative effects of subsidies on take-up is large enough to overcome learning effects. Overall, temporary subsidies for a subset of households increase the average willingness to pay for bednets in the general population.

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# 1 Introduction

Over 10 million children under 5 die every year in the world (Black et al, 2003). Up to 63% 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 (Jones et al., 2003). A major question in development economics is how to increase availability and adoption of these preventative technologies.

A commonly proposed way to increase adoption in the short-run, with the hope to immediately reduce the health burden and trigger a virtuous cycle between health and growth, is to distribute those essential health products for free or at highly subsidized prices (WHO, 2007; Sachs, 2005). The rationale for some subsidization is evident for health products that generate positive health externalities (Baumol, 1972). However, there is substantial debate about how large the subsidies should be. In particular, there is considerable debate as to whether large (especially full) subsidies are good or bad for product demand in the long run.

The standard neoclassical model of consumer behavior predicts that free or highly subsidized distribution of a product in the short run will 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. This learning might trickle down to others in the community (those ineligible for the subsidy) and increase the overall willingness to pay in the population as knowledge of the true value of the product diffuses.

The relationship between the level of subsidy and the pace at which people will learn the true value of the product might not be monotonic, however. Subsidies for a new product might make it accessible to people who, because they do not value it enough to pay a high price for it in the first place, may not use it correctly (or not use it at all), thereby generating "wrong" signals about the product's effectiveness and slowing down the learning process.[1]

Furthermore, while the standard model assumes that prices affect demand only through

[1] Two recent studies test whether lower prices depress usage by failing to screen low-use households or by reducing the likelihood of sunk cost effects: Ashraf, Berry and Shapiro (2007) find evidence of screening effects but no sunk cost effects in a study of water-chlorination usage in urban Zambia; Cohen and Dupas (forthcoming) find no evidence of either screening or sunk-cost effects in a study of bednet usage among pregnant women in rural Kenya.

their effect on the budget constraint, there are a number of potential deviations from this assumption. First, in the presence of incomplete information, the price at which a product is sold might be interpreted as a signal of its effectiveness (Bagwell and Riordan 1991). Alternatively, if the non-subsidized price of the product is well-known, the size of the subsidy might be interpreted as a signal of effectiveness.

Second, consumers could take previously encountered prices as reference points or anchors that affect their subsequent maximum willingness to pay or reservation price (Koszegi 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 outside the lab by Simonsohn and Loewenstein (2006), who show that people spend less money in rent in a city they have just moved to if the city they came from has cheaper housing. Under such reference-dependent preferences, those who receive a subsidy for a health product may anchor around the subsidized price and be unwilling to pay a higher price for the product once the subsidy ends or is reduced. For example, Kremer and Miguel (2007) find that parents in Kenya who were exposed to free deworming treatment for their children for a year were extremely unwilling to pay for deworming once it stopped being free.

Finally, temporary subsidies might affect people's expectations of future prices. Those who are not eligible for the subsidy might decide to take a "wait and see" stance, hoping the subsidy reaches them one day. Those who receive the subsidy might anticipate similar subsidies in the future, not only for the subsidized product but also for other products in the same class.

This paper reports on a field experiment designed to test the relative importance of these potential and competing effects of subsidies in the adoption and diffusion of a new health technology. The technology considered is the long-lasting insecticide-treated bednet (LLIN), a recent innovation in malaria control. The experiment involved 1,122 households in Kenya and included two phases: in Phase 1, subsidy levels for LLINs were randomly assigned across households within six villages, with the maximum subsidy level randomized across villages. Households had three months to acquire the LLIN at the subsidized price they had been assigned to. Prices varied from $0 to $3.80. After a few months, a subset of households received the opportunity to acquire another health product (a water treatment product) at a uniform, positive price. In Phase 2, a year later, all households in four villages were given a second opportunity to acquire an LLIN, but this time everyone faced the same price ($2.30). Phase 2 was unannounced, therefore at the time individuals made their purchasing decision in Phase 1, they were not aware that they would receive a second chance to acquire the product a year later. The LLIN was not available outside of the experiment, but other types of nets were available on the market at the retail price of $1.50.

This experimental design allows us to test multiple hypotheses on the effects of temporary subsidies on demand, both over time and across individuals.

First, we test whether having being exposed to a full or very large subsidy for an LLIN in Phase 1 reduces willingness to pay for an LLIN in Phase 2, a year later. We find no support for this hypothesis. Instead, we find suggestive evidence that gaining access to a free or highly subsidized LLIN in the first year increases households' reported as well as observed willingness to pay for an LLIN a year later. In the context of subsidies for bednets, experience effects thus seem to dominate contrast (entitlement) effects.

Second, we can test for the presence of cross-product entitlement effects, namely, whether being exposed to a full or very large subsidy for an LLIN generates a "wait and see" stance and reduces willingness to pay for other health products. We also find no evidence for this hypothesis. Households who received a free LLIN in Phase 1 where not less, and if anything slightly more likely than other households, to invest in the water-treatment product offered at the end of Phase 1.

Third, we use the variation in the maximum subsidy level offered across villages in Phase 1 to test for relative price effects, i.e., whether a household's reservation price is affected by the lowest price offered in that household's environment. We find no evidence that households are less likely to buy an LLIN at a given positive price if others around them face a lower price for the same LLIN. This suggests that if there are relative price effects, they must be outweighed by social learning effects.

Fourth, exogenous variation in the density of households who received a free or highly subsidized LLIN in Phase 1 enables us to formally test for the presence of diffusion effects through social networks (social learning). We find that households facing a positive price in Phase 1 are more likely to purchase the LLIN when the density of households around them who received a free or highly subsidized LLIN is greater. At any given positive LLIN price, a household with 50% of highly subsidized LLIN recipients among study households living within a 500 meter radius is 11 percentage points (28%) more likely to buy the LLIN than a household with no such beneficiary within a 500 meter radius.

Overall, the findings of this paper suggest that the net effect of subsidies on long-run adoption is positive. Previously encountered prices matter, but more so through their effect on available knowledge about the product than through contrast effects.

This paper is most closely related to Kremer and Miguel (2007), who use a randomized evaluation of a school-based deworming program in Kenya to estimate, among other things, the role of peer effects in health technology adoption. They find that households were less likely to invest in deworming if they had a higher number of social contacts who benefitted from free deworming in the past. Their negative effect is consistent with the model of social learning this paper finds evidence for. The difference between the two papers in the direction of the social learning effect comes from the fact that, while bednet use has high private returns and modest non-monetary costs, deworming treatment has only low private returns and non-trivial side effects. The findings of this paper also help shed light on the Kremer and Miguel (2007) result that demand drops precipitously when the price becomes positive. Their experimental design did not allow a test of whether this drop was due to "entitlement" effects or to low perceived private returns of deworming. Our results, based on data from the same area of Kenya, suggest that entitlement might not have been the main effect at play in the drop they observe.

More broadly, this paper contributes to two literatures. First, the paper contributes to a now large literature on the role of learning-by-doing and social learning in technology adoption in poor countries. The evidence so far, mostly non-experimental and mostly focused on agricultural technologies, is rather mixed and suggests that the role of social learning is likely to vary greatly with the context and the product considered. Foster and Rosenzweig (1995) and Besley and Case (1997) find that a farmer's ability to reap profits from a new technology increases with not only her own but also her neighbors' experience with the new technology, but Munshi (2004) finds that social learning requires a certain degree of homogeneity among farmers, and Bandiera and Rasul (2006) find some evidence of strategic delay in adoption of new products. Conley and Udry (forthcoming) present evidence that social learning is important in the diffusion of knowledge regarding pineapple cultivation in Ghana, while the randomized experiment of Duflo, Kremer and Robinson (2009) finds no social learning in fertilizer use in Western Kenya. Empirical studies of social learning outside agriculture are still few in the economics literature. Behrman et al. (2001) study social networks of young women in rural Kenya and find evidence of S-shaped diffusion of attitudes and behaviors with respect to contraception and AIDS. Munshi and Myaux (2006) provide suggestive evidence from India that a woman's contraception decision responds strongly to changes in contraceptive prevalence in her own religious group within the village but not to changes outside her religious network. Oster and Thornton (2008) find evidence of peer effects in the usage of a new female hygiene product provided for free. Finally, Kremer et al. (2008) experimentally study diffusion of a water-treatment product in the same area of Kenya as that studied here, and find evidence of moderate social spillovers.

The second literature this paper contributes to is the empirical reference-dependence literature estimating 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, Koszegi and Ariely, 2009), or the range of options that are offered to them (McFadden, 1999; Heffetz and Shayo, 2008).

Our experiment brings together these two existing strands of literature and provides evidence that, for essential preventative health products in a relatively poor country, price-reference effects fall short of learning effects, and the overall impact of short-term, targeted subsidies on technology adoption in the long run appears positive and non-trivial.

The remainder of the paper is as follows. Section 2 presents a simple framework to think about technology adoption in the presence of learning by experience, social learning and reference-dependent preferences. Section 3 describes the background and the experimental design. Section 4 presents the results, and Section 5 concludes.

# 2 Conceptual Framework

This section presents a simple, general framework to guide the experimental design and empirical analysis. We consider the diffusion of a new health product among households, and how it is affected by temporary subsidies to a subset of the population. The health product is of quality $\theta$, where $\theta$ is initially unknown.

Time is discrete. In any period $t$, each household may buy either one unit of the product or none. Acquiring one unit of the product and using it correctly gives households information about the true quality level $\theta$. At the end of the period, the acquired unit becomes ineffective, whether it was used or not, and needs to be replaced. We assume that at the time the product is introduced (before anyone can experiment with the product), household $h$ has a prior $\theta_{h,0}$ about the quality of the product. At any period $t$, its subjective valuation of the product (equal to its reservation price) is $v_{h,t} = b_{h,t}\theta_{h,t}$. The factor $b_{h,t}$ is determined by household $h$'s preferences and characteristics. At period $t$, household $h$ buys the product if its reservation price $v_{h,t}$ exceeds or equals the price $p$ of the product.

To allow for reference-dependence, consider that the household factor $b_{h,t}$ is a function of previously encountered prices: $b_{h,t} = f(X_{h,t}, p^h_{\min}, p^j_{\min})$, where $X_{h,t}$ is a vector of household characteristics at time $t$; $p^h_{\min}$ is the minimum price household $h$ ever faced for the product; and $p^j_{\min}$ is the minimum price at which the product was ever offered at in village $j$ (for example, if the product was ever distributed for free to some households in the village, $p^j_{\min} = 0$).

Knowledge about the quality of the product spreads through social networks: the neighbors of a household that own the product receive a signal about the product quality by observing that household's health level. Only households that use the product experience the positive health impact. Let's call $u$ the probability that people use the product if they have acquired it. We assume the following process of learning: for household $h$ who never acquired the product, the subjective belief about the product quality is affected by the fraction of households in the neighborhood who ever acquired the product and used it correctly ($u$), and by the fraction who ever acquired the product but did not use it $(1 - u)$. Specifically, we assume the following learning process for households that have never experimented with the product themselves at time $t$:

$$\begin{aligned}\theta_{h,t} &= \theta_{h,t-1} + \alpha[\frac{N_{t-1}}{M}u(\theta - \theta_{h,t-1}) + \frac{N_{t-1}}{M}(1-u)(0 - \theta_{h,t-1})] \\ &= \theta_{h,t-1} + \alpha\frac{N_{t-1}}{M}(u\theta - \theta_{h,t-1})\end{aligned} \tag{1}$$

where $0 \leq \alpha \leq 1$ is the weight households give to others' experience; $M$ is the total number of households in the neighborhood and $N_t$ is the number of households in the neighborhood who own the product at time $t$.

This learning process implies that households exposed only to neighbors who use the product revise their estimate towards the true quality level $\theta$ (from above, if they initially overestimated $\theta$; or from below, if they underestimated $\theta$). When exposed only to neighbors who do *not* use the product (and thus do not get healthier) despite owning the product, households revise their estimate of $\theta$ towards 0.

In this very general framework, the effect of subsidies on adoption is ambiguous:

- The effect of receiving a short-term subsidy (a price $p^h_{\min}$) on own willingness to pay in subsequent periods (when the price is $p > p^h_{\min}$) is the sum of three effects:

$$\frac{\partial v_h}{\partial p^h_{\min}} = \frac{\partial \theta_h}{\partial p^h_{\min}} + \frac{\partial f}{\partial p^h_{\min}} + \frac{\partial f}{\partial X_h}\frac{\partial X_h}{\partial p^h_{\min}} \tag{2}$$

1. $\frac{\partial \theta_h}{\partial p^h_{\min}}$, the learning or experience effect, can be either positive or negative, depending on the prior belief on $\theta$: having access to the product at a cheaper price may increase the likelihood that household $h$ experiments with the product. This would in turn increase $h$'s valuation if $h$'s prior was an underestimate of $\theta$, or decrease $h$'s valuation if $h$'s prior was an underestimate of $\theta$.

2. The second term corresponds to the contrast (entitlement) effect: $\frac{\partial f}{\partial p^h_{\min}} \geq 0$.

3. The last term in equation (2) corresponds to a negative income effect: the subsidy received in period $t$ might change the household's characteristics in subsequent periods First, the less the household paid for the product in the past, the greater its disposable

income today. Second, the health benefits associated with the product may generate a positive income effect for those who use it (i.e., if healthier people earn higher wages). Both these mechanisms imply a negative income effect of higher prices.

- The spillover effect of a short-term subsidy (a price $p^j_{\min}$ for some households others than household $h$) is also ambiguous, and the sum of three terms:

$$\frac{\partial v_h}{\partial p^j_{\min}} = \frac{\partial \theta_h}{\partial p^j_{\min}} + \frac{\partial f}{\partial p^j_{\min}} + \frac{\partial f}{\partial X_h}\frac{\partial X_h}{\partial p^j_{\min}} \tag{3}$$

1. The first term represents social learning. It can itself be decomposed into two effects:

$$\frac{\partial \theta_h}{\partial p^j_{\min}} = \frac{\alpha}{M}[\frac{\partial N}{\partial p^j_{\min}}(u\theta - \theta_h) + N\frac{\partial u}{\partial p^j_{\min}}\theta]$$

   The first term is negative and reflects the fact that neighbors' demand will increase as the price falls ($\frac{\partial N}{\partial p^j_{\min}} < 0$). The second term is positive or null and reflects the fact that reducing the price might not increase overall exposure despite the increase in take-up, since those who acquire the product may not put it to proper use ($\frac{\partial u}{\partial p^c_{\min}} \geq 0$).

2. The second term in equation (3) corresponds to the contrast (relative price) effect: $\frac{\partial f}{\partial p^j_{\min}} \geq 0$.[2]

3. The last term in equation (3) refers to a potential negative income effect of higher prices via the health externality: if having more neighbors using the product generates a positive health impact, lower prices may lead to a positive income effect even for those who do not own and use the product themselves.

The experiment we describe below was designed to estimate the overall effect of temporary subsidies on both long-run adoption by subsidy recipients and adoption by their neighbors.

[2] In the presence of positive health externalities (as is the case with antimalarial bednets or deworming drugs), there is one additional channel through which the price experienced by one's neighbors might affect one's demand: people might decide to free-ride off of their neighbors and not invest in the product, if the private returns are low (this is what Kremer and Miguel (2007) observe for deworming drugs). This channel would also lead to $\frac{\partial f}{\partial p^j_{\min}} > 0$. In the experiment described below, we are not able to separately test for the presence of this "free-riding" effect and for the presence of the "relative price" or contrast effect, but we find no evidence that either effect or their sum can outweigh positive social learning effects.

It does not allow us to separately estimate the absolute magnitude of each term in equations (2) and (3), but it provides information on the relative size of these various effects, i.e., on the net signs of $\frac{\partial v_h}{\partial p^h_{\min}}$ and $\frac{\partial v_h}{\partial p^j_{\min}}$. We find that for both direct subsidy recipients and their neighbors, the net effect of price is negative (i.e., the net effect of subsidies is positive), suggesting that contrast effects of prices, whether entitlement, anchoring or relative price effects, are dominated by positive learning and income effects.

# 3 Background and Experimental Design

## 3.1 Background on Malaria and Bednet distribution in Kenya

Over the past two decades, the use of insecticide-treated 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 it 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, the LLIN used in this experiment, was not available for sale, and its effectiveness–relative to that of regular ITNs, subsidized by the social marketing organization PSI and sold through the retail sector–was not well known.[3]

In our study sample, 80% of households owned at least one bednet (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 (Table 1).

[3] In 2002, the NGO Population Services International (PSI) started implementing a Kenya-wide social marketing campaign for bednets. Until 2004, bednets were subsidized but remained expensive, at Ksh300 ($4.50). In 2004, PSI started selling ITNs to pregnant women and parents of under-fives for Ksh50 ($0.75) at health facilities, and to the general population through the retail sector at prices starting at Ksh100 ($1.50).

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 campaign of July 2006, ten months before the onset of this study. These PermaNets differ substantially from the Olyset LLIN used in our experiment: PermaNets are circular and not rectangular, made of polyester and not polyethylene, and have a smaller mesh. They cannot, *de visu*, be distinguished from traditional, not long-lasting nets, while Olyset nets can.

## 3.2 Experimental Design: Phase 1

The experiment was conducted in Busia District, Western Kenya, where malaria transmission occurs throughout the year. The study involved 1,122 households from six rural areas. Participating households were sampled as follows. In each area, the school register was used to create a list of households with children. Listed households were then randomly assigned to a subsidy level for an LLIN. The subsidy level varied from 100% to 40%; the corresponding final prices faced by households ranged from Ksh 0 to 250 (US$3.8). 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; one area had a minimum price of 40 Ksh; two areas had a minimum price of 50 Ksh; and one area had a minimum price of 70 Ksh.

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.[4] At the end of the interview, the respondent

[4] Whether the female head, male head or both were interviewed and given the voucher was randomized across households. It had no effect on take-up. In what follows, all regressions presented with household controls include controls for the randomized gender assignment.

was given a discount voucher for an LLIN 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.[5] Vouchers could be redeemed at participating local retailers (1 per area). The six participating retailers were provided with a stock of blue, extra-large, rectangular Olyset nets (one of five brands of LLINs currently recommended by the World Health Organization and the first one to be approved). At the time of the study, extra-large Olyset nets were not available to households through any other distribution channel, which facilitated tracking of the LLINs that were sold as part of the study.

The participating retailers received as many Olysets as vouchers issued in their community, and no more. They were not authorized to sell the study Olysets to households outside the study sample.[6] 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.

The subset of households who had redeemed their LLIN voucher were sampled for a short-run follow-up administered during an unannounced home visit 2 to 15 weeks following voucher redemption (or 1 to 5 months after the baseline survey). During the follow-up visit, the enumerator asked to see the net that was purchased with the voucher, so as to ascertain that it was a study-supplied LLIN. The follow-up survey also checked whether households whether households were charged the assigned price for the LLIN. Usage was assessed as follows: (1) whether the respondent declared having started using the net, and (2) whether the net was observed hanging above the bedding at the time of the visit. In addition, willingness to pay to replace the study LLIN was assessed by asking households

[5] The fact that the recommended retail price was indicated on the voucher could have dampened the possibility of anchoring effects. From a policy standpoint, indicating the non-subsidized price on a voucher or product is costless, therefore estimating the overall effect of subsidies in the presence of full information about the non-subsidized price is the relevant policy parameter.

[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.

the following question: "If you didn't have this net, up to how much would you be willing to pay to get a net like this, now that you are familiar with it?"

Note that, while the main advantage of the Olyset LLIN is its long-lasting property, it can easily be differentiated from other nets in the short run: it is sturdier than other nets because it is made of polyethylene (and not polyester) and it is also more comfortable (less hot) thanks to its wider mesh.

## 3.3 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 LLIN 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 incidence of malaria in the previous month was collected. Households were also asked if they knew people who had redeemed their vouchers and what those people had told them about the LLIN acquired with the voucher. In addition, for those who had redeemed the voucher, usage of the LLIN was recorded as in the first follow-up.

At the end of the visit, households received a second LLIN voucher, redeemable at the same retailer as the first LLIN voucher received a year earlier. All households faced the same price (Ksh150 or $2.30) for this second voucher. The set-up used with retailers was identical to that used in 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 large or full subsidy dampens or enhances willingness to pay for the same product a year later. Note, however, that since LLIN have a lifespan of 4 to 5 years, at the time they received the second LLIN voucher, households who had purchased an LLIN with the first voucher in Phase 1 did not need to replace their first LLIN. The redemption rate of the second voucher thus measures, for those households, the willingness to pay for an additional LLIN, and not a replacement LLIN. If we make the reasonable assumption of decreasing marginal returns to LLINs, the willingness to pay observed through the second voucher redemption will be a lower bound

[7] 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 budget reasons.

for the willingness to pay for a replacement LLIN.

## 3.4 Verifying Randomization

A baseline survey was administered at households' homes between April and October 2007, prior to the fist voucher distribution. The baseline survey assessed household demographics, socioeconomic status, and bednet ownership and coverage. Table 1 presents summary statistics on 15 household characteristics, as well as baseline differences by price (subsidy) groups. Overall, the various price groups appear balanced. In total, out of 75 differences computed in Table 1, only 4 (just about 5%) are significant at the 10% or 5% level, which should be expected.

## 3.5 Verifying Compliance

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, and 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, and that there was almost no arbitrage between households prior to voucher redemption.

To check for potential arbitrage after redemption (i.e., people selling the LLIN to their neighbor after having redeemed the voucher), we conducted unannounced home visits and asked to see the LLIN that had been purchased with the voucher (the study-provided 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 their voucher(s) could show the LLIN(s) corresponding to the voucher(s) during the spot checks.

# 4 Results

## 4.1 Take-up of the first LLIN: Voucher Redemption and Usage

The effects of subsidies on take-up and usage of the Phase 1 LLIN are presented in Figure 1 and in Table 2. Panel A of Figure 1 shows that the take-up of the first voucher is highly sensitive to price: take-up is quasi-universal for free LLIN vouchers (at 97.5%), but drops to 60-70% when the price is between 40 and 90 Ksh (between $0.6 and $1.4), and further drops to around 30% when the price crosses the 100 Ksh threshold ($1.5). In contrast, Panel B of Figure 1, which shows usage rates after two months (among those who redeemed their voucher), suggests that the likelihood that people put the LLIN to immediate use does not increase with price (if anything, usage seems to decrease with price, although the differences between price groups are not statistically significant). Panel C shows usage rates across price groups at the second follow-up. The usage rate reaches 90% after 1 year, and is, here again, statistically indistinguishable between price groups. These results confirm that the findings obtained among pregnant women by Cohen and Dupas (forthcoming) hold for the general population and over time.[8]

Table 2 presents regression estimates of the direct effect of prices on adoption in Phase 1. Column 3 estimates the following equation:

$$Y_{hj1} = \alpha_1 P_{hj1} + \alpha_2 (P_{hj1})^2 + \alpha_3 (P_{hj1})^3 + X_h^{'} \gamma + \upsilon_j + \varepsilon_{hj}$$

where $Y_{hj1}$ is a dummy equal to 1 if household $h$ in village $j$ bought the LLIN in Phase 1, $P_{hj1}$ is the price faced in Phase 1, $X_h$ is a vector of household characteristics, and $\upsilon_j$ is a village fixed effect. Given the random assignment of prices, $E[P_{hj1}, \varepsilon_{hj}] = 0$ and estimates of the $\alpha'$s are unbiased. Column 1 estimates a similar equation without household characteristics.

Column 4 estimates the following equation:

$$Y_{hj1} = \alpha_4 \cdot \mathbf{1}(P_{hj1} = 0) + X_h^{'} \gamma + \upsilon_j + \varepsilon_{hj}$$

[8] Appendix Table A1 shows that attrition at follow-up was comparable across price groups, and therefore the estimates of the effect of price on usage are unbiased.

where $\mathbf{1}(P_{hj1} = 0)$ is a dummy variable equal to 1 if household $h$ was assigned a price of 0 in Phase 1. Column 2 estimates a similar equation without household characteristics.

Columns 5-8 and 9-12 use similar specifications to estimate the effect of price on the time needed to redeem the voucher and on usage of the LLIN, among those who redeemed their Phase 1 voucher.

The regression analysis confirms the results observed in Figure 1. The two main findings—take-up is highly sensitive to price but usage is independent of price—are robust to adding household-level controls.[9] In addition, Columns 5-8 suggest that the time needed to redeem the voucher was, not surprisingly, highly dependent on the price people had to pay for the LLIN. Households who faced a price of $1 needed on average 13 more days to redeem their voucher than households that received a voucher for a free LLIN, and households facing a price of $2 needed 21 more days.[10]

In the terms of the framework presented in Section 2, these findings suggest that $u'(p) = 0$ (usage is independent on the price paid) but that $N'(p) << 0$ (demand drops with price).

## 4.2 Long-Run Effects of Direct Exposure

This section tests whether households who benefitted from a free or highly subsidized LLIN in Phase 1 were more or less likely to pay for a LLIN in Phase 2, when the price was high for everyone. We test this using both declared preferences and revealed preferences.

First, we look at how households' declared willingness to pay for a bed net was affected by the subsidy. This is presented in Figure 2, which is restricted to the sample of households that redeemed their first voucher. For each Phase 1 price group, two averages are presented:

[9] One could be concerned that self-reported usage suffers from social desirability bias. In particular, households that received a voucher for a free or cheap net might feel compelled to report that they are using the net, if they think that the existence of future subsidies might depend on whether or not they report using the net. A more objective measure of usage is whether the net was seen hanging above a bed during the enumerator's unannounced home visit. Appendix Table A2 reproduces the regressions presented in Table 2 columns 8-12, with "net seen hanging" as the dependent variable instead of self-reported usage. The results are essentially unchanged – if anything, recipients of free nets were more likely to have their net hanging than recipients of higher-price vouchers.

[10] 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).

the average willingness to pay for a bednet declared at baseline, before households had received the first voucher; and the average willingness to pay declared at the follow-up, when households were asked: "If you didn't have this net, up to how much would you be willing to pay to get a net like this, now that you are familiar with it?" These two averages can be considered as the "before" and "after" willingness to pay for those that redeemed their first voucher. Figure 2 shows that the willingness to pay increased substantially and significantly for all households, and especially for those households who received large subsidies. While part of this increase could be imputed to a general increase in awareness of malaria issues in Kenya over time, or to an increase in households' wealth level over time, the effect is too large to be explained by a simple time trend, suggesting that the large subsidies might have enabled households to learn the benefits associated with the net.[11]

Declared willingness to pay might suffer from social desirability bias, however. For this reason, it is important to also look at revealed preferences, namely, the take-up of the second LLIN. The price of the second voucher was uniform across all households (at 150 Ksh). Figure 3 presents the average purchase rate for the second LLIN offered, for each Phase 1 price group. The confidence intervals are large, but the average take-up was higher among the higher subsidy groups (free and 40-50 Ksh price groups).

The regression analysis presented in Table 3 confirms this result. Columns 1 through 6 estimate the following reduced form equations:

$$\begin{aligned}
Y_{hj2} &= \beta_1 P_{hj1} + \beta_2 (P_{hj1})^2 + \beta_3 (P_{hj1})^3 + X_h^{'}\gamma + \upsilon_j + \varepsilon_{hj} \\
Y_{hj2} &= \beta_4 \cdot \mathbf{1}(P_{hj1} = 0) + X_h^{'}\gamma + \upsilon_j + \varepsilon_{hj} \\
Y_{hj2} &= \beta_5 \cdot \mathbf{1}(P_{hj1} \leq 50) + X_h^{'}\gamma + \upsilon_j + \varepsilon_{hj}
\end{aligned}$$

where $Y_{hj2}$ is a dummy equal to 1 if household $h$ in village $j$ bought a LLIN in Phase 2; $\mathbf{1}(P_{hj1} \leq 50)$ is a dummy equal to 1 if the price faced by household $h$ was a high-subsidy price (below 50Ksh); and the other variables are defined as above.

The take-up in the '1st LLIN Free' group is 6.2 percentage points (41%) higher than in

[11] The average time gap between these two measures of willingness to pay was 87 days. The average gap between the time the household redeemed the voucher and the time the household was asked about willingness to pay to replace the net was 63 days.

the non-free groups, suggesting a learning-by-doing effect (Table 3, column 5). While this effect is not statistically distinguishable from zero (the 95% confidence interval is [-.03;+.15]), it is worth noting that the take-up of the second LLIN voucher in this group reflects the demand for a second LLIN, 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 LLIN (since take-up of the first voucher was low at high prices). Under the reasonable assumption that the marginal utility of LLINs is decreasing in the number of LLINs owned, holding everything constant, the demand for a second LLIN should be lower than the demand for a first LLIN. In other words, the fact that the take-up for the second voucher is not significantly *lower* in the '1st LLIN free' group than in the low-subsidy groups is enough to conclude that the willingness to pay in the '1st LLIN free' group increased.

Columns 10-12 of Table 3 estimate the following equation:

$$Y_{hj2} = \gamma U_{hj1} + X_h^{'}\gamma + v_j + \varepsilon_{hj}$$

where $U_{hj1}$ indicates whether household $h$ used an LLIN in Phase 1 (i.e., not only bought the LLIN in Phase 1 but also used it), and is instrumented with either the price faced in Phase 1, its square and its cube (column 10); or a dummy indicating whether the price faced in Phase 1 was zero (full subsidy, column 11); or a dummy indicating whether the price phased in Phase 1 was 50Ksh or lower (high subsidy, column 12). The three possible first-stage estimations are presented in columns 7-9 of Table 3.

The estimates of $\gamma$ in these instrumental variables specifications measure the effect "on the treated", that is the effect of having experimented with the first LLIN. The effect is close to a 90% increase in take-up of the second LLIN (+13 percentage points off of a 15 percent mean in the non-free group) and the significance approaches 10% (the p-value of the coefficient on "experimented" in column 10 is 0.14). Note, however, that the exclusion restriction for the instrument (the price of the first voucher affects willingness to pay for the second LLIN only through the learning effect) does not hold in the presence of contrast or entitlement effects. Thus our preferred specifications are the reduced form specifications presented in columns 1-6.

Columns 3 and 6 of Table 3 present a specification with a "high subsidy" dummy (1st LLIN price $\leq$ 50 Ksh). As was apparent in Figure 3, the high-subsidy group in Phase 1 had a higher redemption rate in Phase 2 than the other groups. The effect of having received a high subsidy in Phase 1 is significant at the 10 percent level without or with household controls. The IV estimates (column 12) is also significant at the 10 percent level, although as discussed above the IV specification is not valid in the presence of entitlement effects.

Overall, these results suggest that potential anchoring or entitlement effects are at best limited in scope, and in any case overwhelmed by a positive effect.

## 4.3 Experience or Income Effect?

The previous section suggests that households who received a free or highly subsidized LLIN are not less likely to buy a second one after a year. Rather, they appear *more* likely to buy a second net, despite the fact that most of them already own one. As discussed in Section 2, there are two possible (and compatible) explanations for this positive effect on willingness to pay for an LLIN: an experience effect (the subsidy enables households to learn about the benefits of a technology); and an income effect, which itself can come from a mechanical effect of the subsidy on the intertemporal budget constraint (those who paid less for the LLIN in year 1 have more money available to invest in an LLIN in year 2); and from a positive health effect on income (households that received the subsidy and adopt the product are less likely to suffer from malaria; this increases their productivity and decreases their malaria treatment expenditures and therefore increases their disposable income). Indeed, we find some suggestive evidence, presented in Appendix Table A3, that the incidence of malaria among household heads (either the male or the female) was lower among households who received a cheaper LLIN voucher in Phase 1. This effect is not surprising given the large medical literature showing large private returns to bednet use (Lengeler, 2004). Given the existing evidence of a link from health to productivity at the micro level (Strauss and Thomas, 1998; Thomas et al, 2003), this health effect among household heads could potentially have generated an income effect.

We do not have data on income itself (precise income data is typically difficult to measure among the self-employed). Instead, in order to test for the relative importance of the budget constraint effects, we distributed uniformly-priced vouchers for a chlorine-based water-treatment product called WaterGuard® to all study households in the two communities where the LLIN subsidy in Phase 1 was 100% for some households. The WaterGuard vouchers were distributed about 5 months after the first LLIN vouchers had been distributed. They enabled households to buy a bottle of WaterGuard at a price of Ksh 15 ($0.10), equivalent to 75% of the current retail price for WaterGuard. WaterGuard vouchers could be redeemed at the same participating local retailers as the LLIN vouchers.[12]

If the experience effect is the main channel behind the positive effect of willingness to pay for the second LLIN observed in Table 3, the take-up of the WaterGuard voucher should be completely independent of the (random) price households faced for their first LLIN voucher. Alternatively, if beneficiaries of free LLINs have higher disposable income because of the subsidy as well as due to the positive health impact of the first LLIN, the take-up of the WaterGuard product should also increase, provided clean water is a normal good.

Table 4 presents evidence on how the subsidy level for the LLIN affected take-up of the WaterGuard voucher in the two areas selected for this exercise. The results suggest that the recipients of free LLINs were 6 percentage points more likely to redeem their WaterGuard voucher than those who did not receive a full LLIN subsidy. This effect is not significant, and in relative terms, the magnitude of the effect is smaller than that observed for the second LLIN take-up in Table 3. The take-up of the WaterGuard voucher was 40% on average, and therefore a 6 percentage points increase corresponds to a 15% increase only, in contrast with the 41% increase in take-up observed for the second LLIN among recipients of a free LLIN in Phase 1. The effect on the treated (those who actively used the free LLIN) is greater in magnitude (+15 percentage points, or 37%), but still lower than that observed for the second LLIN (90%).

Overall, these results suggest that both learning and income effects are likely to have played a role in the positive impact of subsidies on willingness to pay for LLINs observed in section 4.2.

[12] Since WaterGuard was available for sale at local markets at the time of the experiment, it was necessary to offer a small discount in order to measure take-up accurately. In the absence of a discount, households would have had no incentive to bring their voucher when buying the product, and we would not have been able to trace demand.

## 4.4 Cross-Product Entitlement Effects?

Development practitioners often worry that subsidies for one product 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, we can exploit the WaterGuard voucher experiment conducted 5 months after the first LLIN vouchers were distributed.

Overall, the results presented in Table 4, showing that the take-up of WaterGuard was not lower among recipients of free LLINs, suggest that cross-product entitlement effects are likely to be limited. In other words, households who get a chance to receive a free LLIN do not seem to expect that other health technologies should be given to them for free in order for them to experiment with them.

## 4.5 Effects of Indirect Exposure

### 4.5.1 Negative spillovers? Testing for "relative price" effects

This section tests whether, holding price constant, households were less likely to redeem their voucher and less likely to pay for a net when their neighbors received a voucher for a cheaper price than theirs. Since prices were randomized across households within areas, households quickly realized that not all vouchers carried the same price/subsidy. Thus households who received a higher price might have felt they got the bad end of the bargain, or felt that since they didn't receive the higher subsidy, it might mean that the product was not really targeted at them, and thus they could do without.[13]

To test this hypothesis, which we call the "relative price effect" hypothesis, we exploit the fact that the highest subsidy level offered (i.e., the lowest price offered) randomly varied across areas. We can thus compare households who faced a given price (e.g. 50 Ksh) in two

[13] When enumerators were asked why the price varied across households, they were instructed to answer that "the prices were determined through a lottery performed on the computer". This might have reduced the likelihood that households inferred something from the size of the subsidy they received.

areas, one where 50 Ksh was the lowest price offered, and one where 50 Ksh was not the lowest price because other households received a voucher for a free LLIN. This is done in Table 5. The sample in this table is restricted to those who received a price in the following set: {40, 50, 70}. At each of these three prices, households in some areas got the best possible price in their area, while households in other areas did not (because other households got a free or cheaper bednet in their area). Table 5 estimates the following equation:

$$Y_{hj1} = \pi \cdot \mathbf{1}(P_{hj1} = P^{j}_{\min}) + \delta_1 P_{hj1} + \delta_2 {P_{hj1}}^2 + \delta_3 {P_{hj1}}^3 + X_h^{'}\gamma + \varepsilon_{hj}$$

where $P_{hj1} \in \{40, 50, 70\}$ and $P^{j}_{\min}$ is the minimum price offered in village $j$. The finding that $\widehat{\pi} > 0$ would imply that, holding price constant, there is a positive effect of being offered the minimum (cheapest) price in the village. Such a relative price effect could come from a psychological feeling-unlucky effect, or it could work through anticipation of future prices (those who do not have the cheapest price adopt a wait-and-see stance).

Since the treatment studied here (getting the minimum price in a given area) varies across areas and not across individuals within areas, the inclusion of area fixed effects is not possible for this analysis. This limits the interest of this exercise, since there are only 6 areas in the sample (so essentially 6 data points). Despite the randomization, take-up could greatly vary across areas independently of the subsidy level, for example if areas differ in their characteristics. This caveat is worth keeping in mind when looking at the results.

The coefficient estimates in Table 5 suggest that, contrary to the relative price effect hypothesis, households that could see other households in their area get a lower price were more likely, on average, to redeem their voucher. The negative effect becomes quite large and even significant when household controls are added. (The jump in the magnitude of the coefficient when household controls are added suggests that there might be differences across areas that are partly swept by the household controls.) Holding price constant, households that were the "luckiest" in their area and got the smallest price offered (the highest subsidy) are 18 percentage points (30%) less likely to have redeemed their voucher. In other words, households were more likely to redeem their voucher if they had neighbors who benefitted from a lower price than theirs. This result is consistent with a model of social learning where households learn from others about the benefits of a new technology. The next section tests for such social learning effects more thoroughly, by exploiting within-area variation in exposure.

#### 4.5.2 Positive spillovers? Testing for diffusion effects

**Non-experimental Evidence** Table 6 presents non-experimental social effect estimates. The sample is restricted to households who received a positively priced voucher in Phase 1.[14] Column 1 regresses whether households redeemed their first LLIN voucher on the number of households they know had redeemed their voucher. Knowing one more household who redeemed the voucher is correlated with a take-up higher by 12 percentage points (32%). The take-up is higher by 18 percentage points when the household declares that at least one other household recommended the LLIN to them. These results could be entirely driven by omitted variable bias, however. It is likely that households who redeemed their vouchers were more likely to ask their neighbors if they redeemed theirs too, for example. For these reasons, we next turn to experimental estimates of social learning.

**Experimental Evidence** Given the large differences in take-up across price groups, the random assignment of households to price groups generate an exogenous source of variation in the density of households that had a chance to experiment with the LLIN.

Using GPS coordinates, we computed, 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 given subsidy level. In particular, for households who faced a positive price, we computed the share of households within a given radius who received the maximum subsidy offered in the area (i.e., the share of households who received a voucher for a free LLIN in the two areas where the subsidy reached 100%; the share of households who received a voucher for an LLIN at 40 Ksh in the area where the lowest price as 40Ksh; etc.). We use three different radii to define social networks or neighborhoods: 250 meters, 500 meters, and 750 meters. Appendix Table A4 presents summary statistics on these density

[14] Since 97.5% of households who received a free voucher redeemed it, adding households who received a free voucher in this analysis doesn't add information.

measures in Panel A. On average, households who received a positive-price voucher have 1.4 neighbors within a 250m radius (4.4 neighbors within 500m, 8.53 within 750m) who received the maximum possible subsidy level offered in the area. This represents, at the mean, 22-23% of the study households living within these radii Panel B of Table A4 tests whether these density measures are correlated with the voucher price. Column 1 regresses the price households faced on the share of households with the maximum subsidy within a 250m radius, controlling for the total number of sampled households within that radius. The coefficient on the share is statistically significant at the 10% level, but small in magnitude (a household with 100% of sampled neighbors in the 'maximum subsidy' group faces a price US$ 0.23 (13 Ksh) higher than a household with 0% of sampled neighbors in the maximum subsidy group). If anything, this positive correlation between own price and exposure to neighbors with cheap prices will lead to a downwards bias in the estimates of social learning/spillovers. None of the other exposure measures have statistically significant coefficients in the price regressions (Table A4, Panel B, columns 3-6).

Figure 4 plots the coefficients of OLS regressions, where the dependent variable is whether or not a given household purchased the LLIN and the independent variable is the share (panel A) or the number (panel B) of study households within a 500m radius of the given household who received the maximum subsidy offered in the area. Both specifications show take-up increasing as exposure to the product via neighbors increases.

To confirm these results and test how sensitive these results are to the choice of the radius, Table 7 reports results from estimating regressions similar to those presented in Table 2 (columns 1 to 4), but including various measures of social exposure to LLIN, and restricting the sample to households that did not receive a free LLIN (i.e, households that received a positive-price voucher). The regressor of interest is the share of neighbors (within a given radius) who received the minimum price (maximum subsidy) offered in the area. The total number of study households within this radius (the denominator in the share variable), is included as a control variable to account for the fact that people living in more densely populated areas may be more likely to adopt new products. In Appendix Table A5, we report results from an alternate specification that uses the number of study households who received the maximum subsidy, rather than the share.

The results in Table 7 are quantitatively unchanged across all three radius choices. The results suggest that the higher the proportion of neighbors who received the high subsidy, the more likely the household is to have redeemed the voucher and purchased the LLIN. When looking at the results using the 'within 500m radius' definition of social networks, we find that, if all of a household's neighbors sampled for the study received the maximum subsidy, the probability of redeeming the voucher increases by 22 percentage points. This implies that households who did not themselves receive a maximum subsidy are over 50% more likely to invest in the LLIN if all of their sampled neighbors received the maximum subsidy. This is a non-trivial effect since the average price households had to pay for the LLIN was 120 Ksh ($1.85), a relatively large sum for rural households in the areas of study.

In Columns 3-4, 7-8 and 11-12 of Table 7, the independent variable is the share of sampled households within a given radius who are using the LLIN. To overcome the obvious endogeneity issue, we instrument the share using an LLIN with the share of sampled households within that radius who received the maximum subsidy level. The results confirm that households learn through their neighbor's experimentation with the product. As discussed earlier, the exclusion restriction does not hold in the presence of contrast effects, therefore the preferred specification remains the reduced form specification.

The specifications presented in Appendix Table A5 looks at levels, rather than densities: the regressor of interest is the total number of households within the radius who have received the high subsidy. The point estimates are positive and relatively large, but the standard errors are also large.

The positive social spillover effects detected here might not be large enough to ensure widespread adoption of LLINs through learning from early adopters in the absence of subsidies. However, what we can say with confidence from the results above is that large targeted subsidies do not seem to dampen willingness-to-pay among the general population. Rather, large subsidies and the resulting increased take-up among beneficiaries help increase the overall level of information available on the effectiveness of a new product, and, as a result, seem to increase the willingness to pay among those that do not receive the large subsidy.

# 5 Conclusion

It is often argued that large subsidies for high-return technologies (such as bednets, treadle pumps, or fertilizer) in the short-run might be detrimental for their adoption in the long run. There are two main effects typically posited. First, previously encountered prices may act as "anchors" that affect people's valuation of a product independently of its intrinsic qualities. This can lead to entitlement effects for those who receive the subsidy, and to "relative price" effects for those who are not targeted by the subsidy. Second, subsidizing a product makes it accessible to people who, because they do not value it enough to pay a high price for it, may not use it correctly and thereby may lower the beliefs about the product's returns.

This paper used a randomized field experiment to test for the presence of these two possible (negative) effects of subsidies, and estimate their relative importance vis-à-vis more standard (positive) effects, namely learning-by-doing and learning from others. We find that, for a health product with a relatively straightforward use and high private returns (the antimalarial bednet), neither of the two postulated negative effects of subsidies on take-up is large enough to overcome the traditional positive effects. Overall, temporary subsidies for a subset of households increase the average willingness to pay for bednets in the general population, through both learning by doing and social learning effects.

The extent to which the adoption of new products 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 evidence provided in this paper suggests that, at least for products with high private returns and whose use does not require specific skills, learning by doing and social learning are important channels through which short-term, targeted subsidies can translate into sustained levels of adoption. These results are consistent with the findings of prior studies on the diffusion of agricultural technologies or reproductive health technologies in both rich and poor countries.

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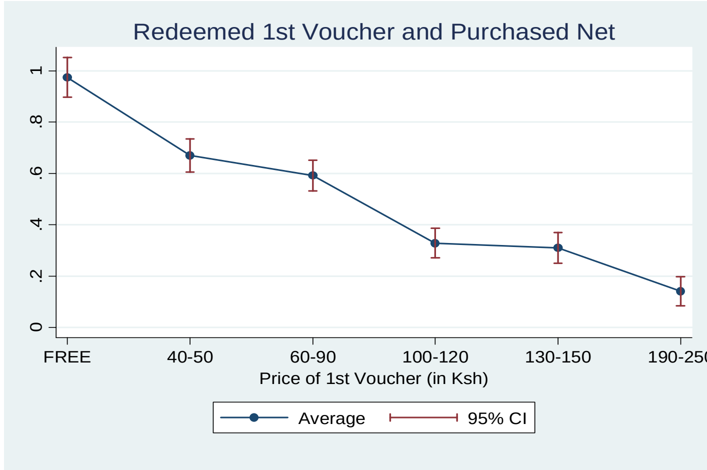
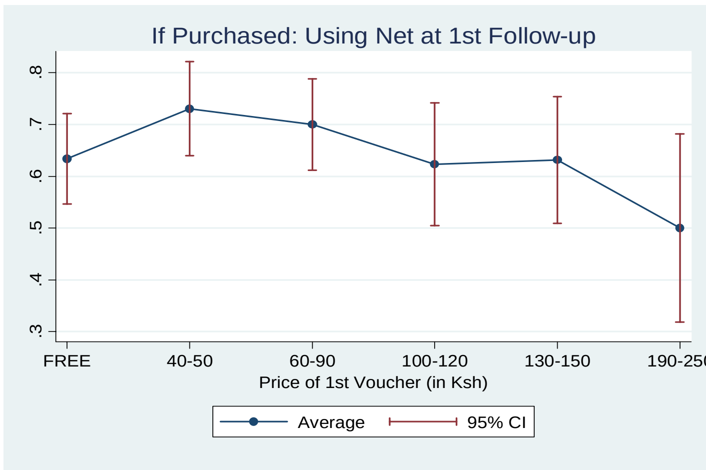
**Figure 1**

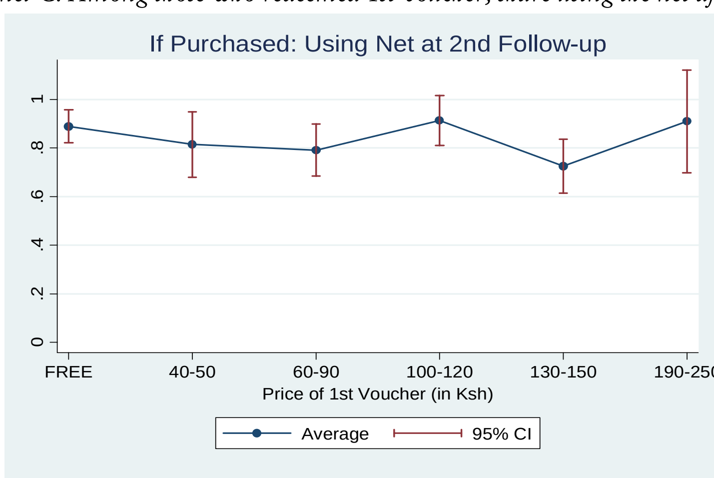
*Panel A: Share of study households who redeemed their 1st LLIN voucher and purchased the net*




*Panel B: Among those who redeemed 1st voucher, share using the net after 2 months*




*Panel C: Among those who redeemed 1st voucher, share using the net after 1 year*




*Notes: Data from 1,122 households (Panel A), 470 households (Panel B), 275 households (Panel C). The second follow-up was conducted in only 4 of the 6 study areas. The exchange rate at the time of the study was around 65 Ksh to US$ 1 .*

**Figure 2**

**Ex-Ante and Ex-Post Declared Willingness To Pay (in Ksh) for LLIN, by 1st voucher price group (subsample of households who redeemed 1st LLIN voucher)**

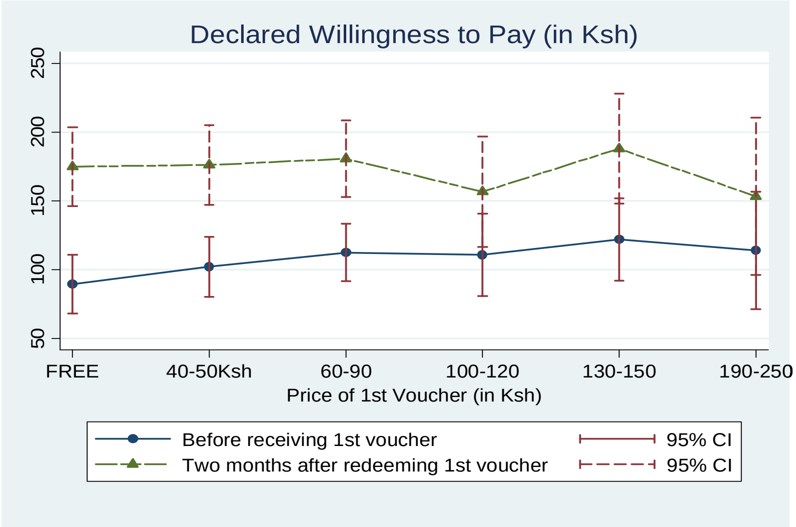
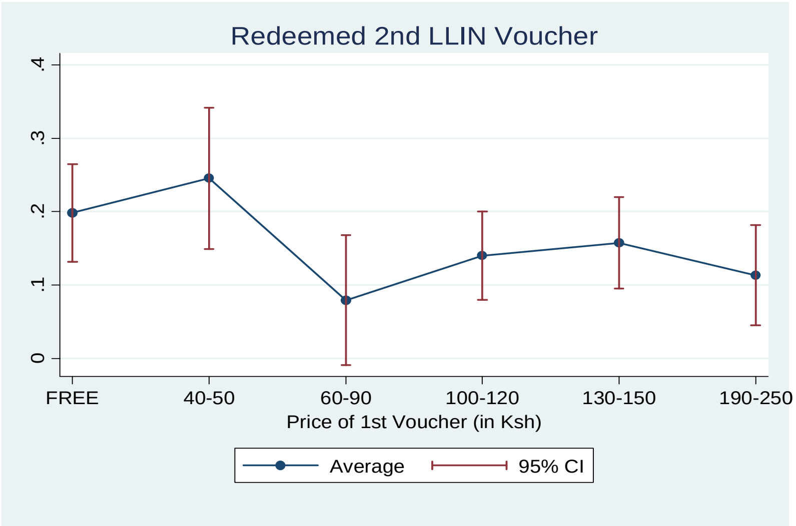



**Figure 3**

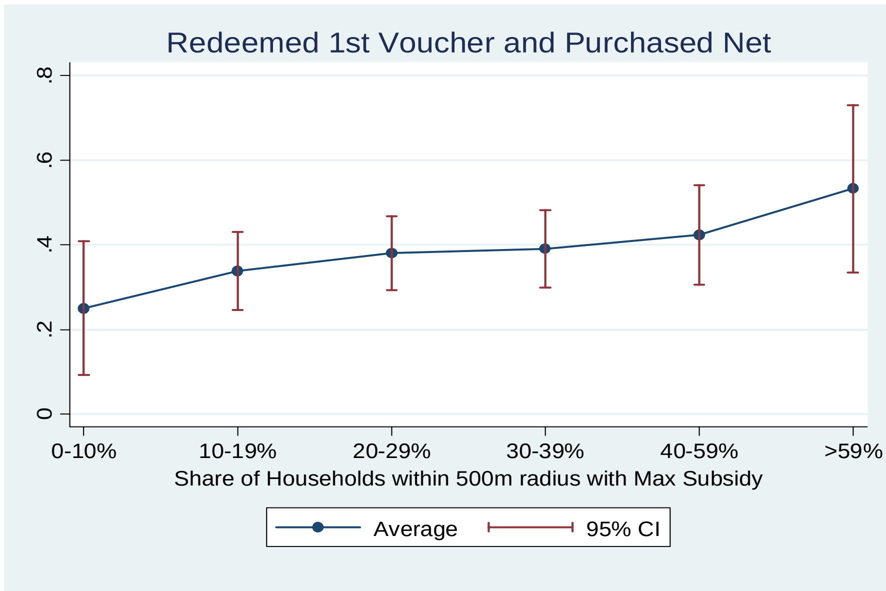
**Redemption of 2nd LLIN Voucher (uniformly priced at 150Ksh), by 1st LLIN voucher price group**



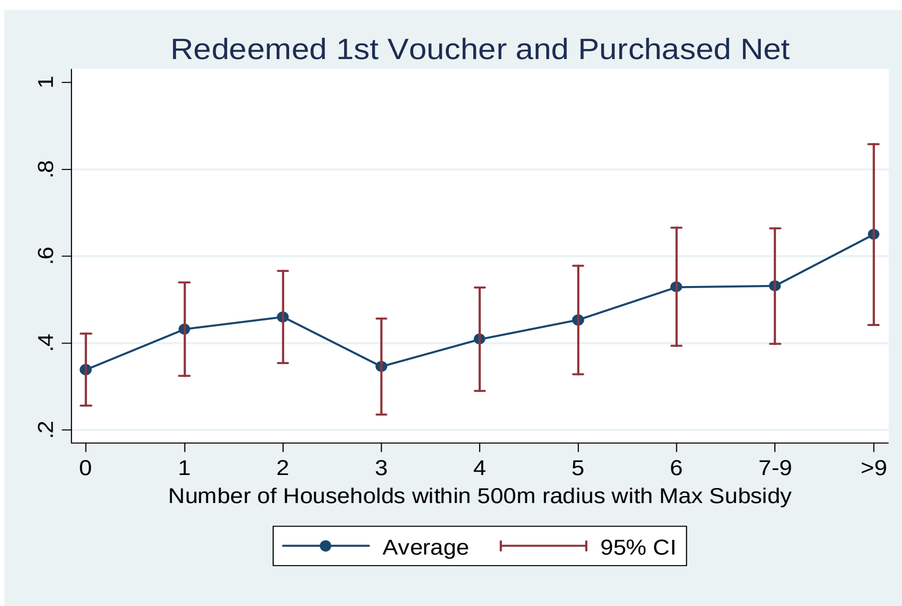

*Notes: Figure 2 includes data from 429 households. Figure 3 includes data from 599 households. Note that the two samples are not comparable across Figures: Figure 2 includes only those who redeemed their 1st voucher, across all 6 study areas. Figure 3 includes all households (whether or not they redeemed their 1st LLIN voucher) but in only 4 study areas. Ex-ante willingness to pay increases with the price group in Figure 2 since only households that did pay for the net are included.*

**Figure 4**
**Purchase of Net in Phase 1 for those facing a Positive Price, by Level of Exposure**

Panel A: By Density of Exposure




Panel B: By Absolute Exposure




*Notes: Sample restricted to households that received a positively priced 1st voucher for an LLIN. Each graph plots the coefficients and confidence intervals obtained through OLS regressions. The dependent variable is whether or not a given household purchased the LLIN and the independent variable is the share (panel A) or the number (panel B) of study households within a 500m radius of the given household who received the maximum subsidy offered in the area. In both panels, the regression controls for the total number of households that live within a 500m radius.*

**Table 1.** *Verifying Randomization: Baseline Characteristics of Participating Households, by 1st LLIN Voucher Price Groups*

| | Sample mean *Std. Dev.* | Estimated differences with Free LLIN Group | | | | | Difference between All Positive-Price Groups and Free LLIN Group |
|---|---|---|---|---|---|---|---|
| | | Group 2 Ksh40-50 | Group 3 Ksh60-90 | Group 4 Ksh100-120 | Group 5 Ksh130-150 | Group 6 Ksh190-250 | |
| ***Household (HH) demographics*** | | | | | | | |
| Household size | 7.11 | 0.635 | 0.372 | 0.584 | -0.112 | 0.435 | 0.335 |
| | *2.75* | (0.396) | (0.384) | (0.367) | (0.373) | (0.399) | (0.335) |
| Number of children (less than 18) currently living in household | 5.45 | 0.344 | 0.294 | 0.326 | -0.234 | 0.214 | 0.153 |
| | *2.85* | (0.409) | (0.395) | (0.378) | (0.384) | (0.411) | (0.345) |
| ***Socio-Economic Status*** | | | | | | | |
| Female head has completed primary school | 0.25 | 0.017 | 0.116 | 0.035 | 0.043 | 0.038 | 0.052 |
| | *0.43* | (0.062) | (0.060)* | (0.058) | (0.059) | (0.063) | (0.053) |
| Number of household members with an income-generating activity | 1.76 | 0.162 | 0.159 | 0.096 | 0.059 | -0.101 | 0.107 |
| | *1.04* | (0.150) | (0.146) | (0.139) | (0.141) | (0.151) | (0.127) |
| Household assets index value (in US $) | 342.5 | 31.9 | 43.5 | 44.0 | 28.5 | 50.2 | 37.6 |
| | *346.68* | (49.753) | (48.096) | (45.972) | (46.788) | (50.016) | (41.888) |
| Electricity at home | 0.02 | -0.007 | 0.000 | 0.011 | -0.003 | -0.004 | 0.002 |
| | *0.14* | (0.020) | (0.019) | (0.018) | (0.019) | (0.020) | (0.017) |
| At least one member of HH has a bank account | 0.12 | 0.029 | 0.100 | 0.013 | 0.013 | 0.070 | 0.034 |
| | *0.32* | (0.047) | (0.045)** | (0.043) | (0.044) | (0.047) | (0.040) |
| ***Bednet Ownership at Baseline*** | | | | | | | |
| Number of bednets owned | 1.74 | 0.092 | -0.029 | 0.023 | -0.011 | 0.114 | 0.011 |
| | *1.51* | (0.217) | (0.209) | (0.201) | (0.204) | (0.218) | (0.183) |
| Share of HH members that slept under a net the previous night | 0.41 | 0.012 | 0.028 | -0.020 | 0.032 | 0.034 | 0.010 |
| | *0.37* | (0.053) | (0.051) | (0.049) | (0.050) | (0.053) | (0.045) |
| HH owns a circular PermaNet LLIN | 0.33 | 0.096 | 0.038 | -0.040 | 0.081 | 0.154 | 0.031 |
| | 0.47 | (0.077) | (0.073) | (0.070) | (0.074) | (0.113) | (0.059) |
| HH ever received a free bednet | 0.32 | 0.032 | 0.052 | 0.024 | -0.009 | -0.004 | 0.021 |
| | *0.47* | (0.067) | (0.065) | (0.062) | (0.063) | (0.068) | (0.057) |
| Has heard of long-lasting treated nets | 0.38 | -0.010 | 0.036 | 0.053 | -0.077 | -0.020 | 0.002 |
| | *0.49* | (0.070) | (0.068) | (0.065) | (0.066) | (0.070) | (0.059) |
| Has ever shopped at shop where voucher has to be redeemed | 0.62 | 0.028 | 0.101 | 0.026 | 0.058 | 0.022 | 0.052 |
| | *0.48* | (0.063) | (0.061)* | (0.058) | (0.059) | (0.063) | (0.053) |
| Declared willingness to pay for a bed net (in US$) | 1.56 | 0.206 | 0.396 | 0.223 | 0.327 | 0.350 | 0.289 |
| | *1.53* | (0.222) | (0.215)* | (0.205) | (0.209) | (0.223) | (0.187) |
| Distance from shop where voucher has to be redeemed (in km) | 1.83 | 0.310 | 0.234 | 0.259 | 0.051 | 0.310 | 0.198 |
| | *1.66* | (0.230) | (0.223) | (0.213) | (0.217) | (0.232) | (0.194) |
| Number of Households | 1122 | 173 | 196 | 216 | 200 | 220 | |

*Columns 2-7: Differences estimated through linear regressions with area fixed-effects. Standard errors in parentheses. The number of households presented in columns 2-6 is the number of households in Groups 2-6, respectively. The size of the Free LLIN Group is 117 households. The LLINs subsidized during the experiment were family-size rectangular Olysets.*

**Table 2.** *Effect of voucher price on take-up and usage of 1st LLIN*

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | *If Redeemed 1st Voucher:* | | | | | | | |
| | Redeemed 1st LLIN voucher | | | | Number of Days Needed to Redeem Voucher | | | | Declared using study LLIN at 1st follow-up | | Declared using study LLIN at 2nd follow-up | |
| 1st LLIN Price in US$ | -0.395 | | -0.408 | | 16.796 | | 17.156 | | -0.119 | | 0.051 | |
| | (0.095)*** | | (0.097)*** | | (8.198)** | | (8.499)** | | (0.167) | | (0.174) | |
| (1st LLIN Price in US$) squared | 0.043 | | 0.053 | | -4.792 | | -5.290 | | 0.046 | | -0.130 | |
| | (0.059) | | (0.060) | | (6.116) | | (6.280) | | (0.121) | | (0.136) | |
| (1st LLIN Price in US$) cubed | 0.002 | | 0.000 | | 0.844 | | 1.003 | | -0.008 | | 0.032 | |
| | (0.010) | | (0.011) | | (1.249) | | (1.277) | | (0.024) | | (0.027) | |
| 1st LLIN Price = 0 (Free) | | 0.472 | | 0.475 | | -12.233 | | -12.129 | | 0.111 | | 0.094 |
| | | (0.056)*** | | (0.057)*** | | (3.821)*** | | (3.998)*** | | (0.077) | | (0.073) |
| Single-Headed Household | | | 0.009 | 0.013 | | | 1.867 | 1.648 | -0.012 | -0.015 | -0.088 | -0.079 |
| | | | (0.041) | (0.044) | | | (3.600) | (3.711) | (0.072) | (0.072) | (0.086) | (0.085) |
| Distance from retail shop where voucher could be redeemed | | | 0.001 | 0.002 | | | 1.077 | 0.928 | -0.018 | -0.017 | 0.028 | 0.028 |
| | | | (0.008) | (0.009) | | | (0.697) | (0.718) | (0.015) | (0.015) | (0.020) | (0.020) |
| Age of household head | | | 0.003 | 0.004 | | | -0.208 | -0.195 | 0.000 | 0.000 | 0.006 | 0.006 |
| | | | (0.001)*** | (0.001)*** | | | (0.101)** | (0.104)* | (0.002) | (0.002) | (0.002)** | (0.002)** |
| Number of children (less than 18) | | | 0.001 | 0.002 | | | -0.334 | -0.330 | 0.004 | 0.005 | 0.012 | 0.013 |
| | | | (0.005) | (0.005) | | | (0.397) | (0.409) | (0.008) | (0.008) | (0.011) | (0.011) |
| Female head has completed primary school | | | 0.051 | 0.060 | | | -0.840 | -0.246 | -0.037 | -0.039 | 0.143 | 0.153 |
| | | | (0.032) | (0.034)* | | | (2.849) | (2.931) | (0.057) | (0.057) | (0.074)* | (0.074)** |
| Natural log of household assets index value in US$ | | | 0.016 | 0.018 | | | -1.985 | -1.983 | 0.027 | 0.027 | 0.010 | 0.008 |
| | | | (0.014) | (0.014) | | | (1.251) | (1.288) | (0.026) | (0.025) | (0.030) | (0.030) |
| At least one member of HH has a bank account | | | 0.053 | 0.045 | | | 2.865 | 3.298 | 0.010 | 0.011 | -0.031 | -0.035 |
| | | | (0.042) | (0.045) | | | (3.504) | (3.603) | (0.071) | (0.070) | (0.092) | (0.092) |
| Share of HH members that slept under a net the previous night | | | 0.042 | 0.032 | | | -1.120 | -1.338 | -0.078 | -0.079 | -0.141 | -0.133 |
| | | | (0.038) | (0.041) | | | (3.386) | (3.483) | (0.067) | (0.067) | (0.081)* | (0.081) |
| HH ever received a free bednet | | | 0.007 | 0.026 | | | -2.022 | -2.793 | -0.047 | -0.044 | -0.073 | -0.074 |
| | | | (0.029) | (0.031) | | | (2.584) | (2.657) | (0.052) | (0.052) | (0.064) | (0.064) |
| Declared willingness to pay for a bed net at baseline (US$) | | | 0.012 | 0.011 | | | -0.138 | 0.050 | 0.013 | 0.012 | 0.000 | 0.001 |
| | | | (0.008) | (0.009) | | | (0.711) | (0.732) | (0.014) | (0.014) | (0.017) | (0.017) |
| Observations | 1122 | 1122 | 1110 | 1110 | 495 | 495 | 492 | 492 | 467 | 467 | 270 | 270 |
| Mean of Dep. Variable in non-free group | 0.39 | 0.39 | 0.39 | 0.39 | 30.97 | 30.97 | 30.97 | 30.97 | 0.61 | 0.61 | 0.70 | 0.70 |
| ***Estimated effect of a price increase:*** | | | | | | | | | | | | |
| from $0 to $1 | -0.35 | | -0.36 | | 12.85 | | 12.87 | | -0.08 | | -0.05 | |
| p-value | p<0.0001 | | p<0.0001 | | 0.001 | | 0.001 | | 0.301 | | 0.535 | |
| from $1 to $2 | -0.25 | | -0.25 | | 8.33 | | 8.31 | | -0.04 | | -0.12 | |
| p-value | p<0.0001 | | p<0.0001 | | 0.002 | | 0.002 | | 0.456 | | 0.093 | |

*Notes: Coefficient estimates obtained using linear regression with area fixed effects. Price varies from 0 to US$3.8. Baseline characteristics unavailable for 12 households. Columns 4 -12 include only households that redeemed the 1st LLIN voucher. 25 households were lost at follow-up. As shown in Appendix Table A1, this attrition was not differential across voucher price groups.*

**Table 3.** *Effect of 1st LLIN price on take-up of 2nd (uniformly-priced) LLIN*

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Redeemed 2nd LLIN voucher | | | | | | First-Stage (IV): Experimented with 1st LLIN | | | Second-Stage (IV): Redeemed 2nd LLIN voucher | | |
| 1st LLIN Price in US$ | -0.091 | | | -0.097 | | | -0.421 | | | | | |
| | (0.095) | | | (0.096) | | | (0.112)*** | | | | | |
| (1st LLIN Price in US$) squared | 0.047 | | | 0.044 | | | 0.099 | | | | | |
| | (0.063) | | | (0.064) | | | (0.075) | | | | | |
| (1st LLIN Price in US$) cubed | -0.008 | | | -0.007 | | | -0.009 | | | | | |
| | (0.011) | | | (0.011) | | | (0.013) | | | | | |
| 1st LLIN Price = 0 (Free) | | 0.050 | | | 0.062 | | | 0.420 | | | | |
| | | (0.046) | | | (0.047) | | | (0.057)*** | | | | |
| 1st LLIN Price ≤ 50 Ksh (High Subsidy) | | | 0.067 | | | 0.076 | | | 0.350 | | | |
| | | | (0.039)* | | | (0.040)* | | | (0.048)*** | | | |
| Experimented with 1st LLIN (instrumented with polynomial in price) | | | | | | | | | | 0.130 | | |
| | | | | | | | | | | (0.088) | | |
| Experimented with 1st LLIN (instrumented with "free" dummy) | | | | | | | | | | | 0.148 | |
| | | | | | | | | | | | (0.113) | |
| Experimented with 1st LLIN (instrumented with "High Subsidy" dummy) | | | | | | | | | | | | 0.217 |
| | | | | | | | | | | | | (0.116)* |
| Household level controls included | | | | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 599 | 599 | | 590 | 590 | | 590 | 590 | 590 | 590 | 590 | 590 |
| Mean of Dep. Variable in non-free group | 0.15 | 0.15 | | 0.15 | 0.15 | | 0.28 | 0.28 | | 0.15 | 0.15 | |
| Mean of Dep. Variable in non-"High Subsidy" group | | | 0.14 | | | 0.14 | | | 0.26 | | | 0.14 |
| ***Estimated effect of 1st voucher price increase:*** | | | | | | | | | | | | |
| from $0 to $1 | -0.05 | | | -0.06 | | | | | | | | |
| p-value | p=.2732 | | | p=.2168 | | | | | | | | |
| from $1 to $2 | 0.00 | | | -0.01 | | | | | | | | |
| p-value | p=.8861 | | | p=.7124 | | | | | | | | |
| F-Stat First Stage | | | | | | | 31.6 | 55.0 | 53.8 | | | |

*Notes: "Experimented with 1st LLIN" is a dummy equal to 1 if the household redeemed the 1st LLIN voucher and the net was seen hanging during at least one of the two surprise follow-up visits. Coefficient estimates obtained using linear regression with area fixed effects. Price of 1st LLIN varies from 0 to US$3.8.*
*Note that in the presence of contrast effects, the exclusion restriction for the instruments used in rows 6-8 will not hold.*

**Table 4.** *Effect of 1st LLIN voucher price on take-up of other health product*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Redeemed WaterGuard voucher | | First-Stage for IV: Experimented with 1st LLIN | | Redeemed WaterGuard voucher | |
| 1st LLIN Price = 0 (Free) | 0.063 | 0.066 | 0.412 | 0.423 | | |
| | (0.062) | (0.066) | (0.056)*** | (0.060)*** | | |
| Experimented with 1st LLIN (instrumented with "1st LLIN Price = 0") | | | | | 0.154 | 0.157 |
| | | | | | (0.152) | (0.156) |
| Household level controls included | | Yes | | Yes | | Yes |
| Observations | 265 | 264 | 277 | 275 | 265 | 264 |
| Mean of Dep. Variable in non-free group | 0.40 | 0.40 | 0.38 | 0.38 | 0.40 | 0.40 |
| F-Stat First Stage | | | 54.504 | 49.623 | | |

*Notes: Sample restricted to the 2 areas where WaterGuard vouchers were distributed. "Experimented with 1st LLIN" is a dummy equal to 1 if the household redeemed the 1st LLIN voucher and the net was seen hanging during at least one of the two surprise follow-up visits.*

**Table 5.** *Effect of relative voucher price on take-up*

| | (1) | (2) |
|---|---|---|
| | Redeemed 1st LLIN voucher | |
| Got smallest price offered in the area (maximum subsidy) | -0.117<br>(0.077) | -0.178<br>(0.083)** |
| Price Fixed Effects | Yes | Yes |
| Household level controls included | | Yes |
| Observations | 308 | 306 |
| Mean of Dep. Variable in non-free group | 0.61 | 0.61 |

*Notes: Sample restricted to households who received a voucher with a price between US$0.75 and US$1.4. Coefficient estimates obtained using linear regression. The results read as follows (column 2): for a given voucher price P within [$0.75-$1.4], households in an area where P was the smallest price offered are 17.8 percentage points less likely to have purchased the LLIN than households who faced the price P in an area where P was not the lowest price (i.e., where some households received a voucher for a price lower than P).*

**Table 6.** *Diffusion Effects: Non-Experimental Evidence*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Redeemed 1st LLIN voucher | | | |
| Number of HHs you know that redeemed their voucher | 0.124<br>(0.019)*** | | 0.123<br>(0.020)*** | |
| LLIN was recommended by at least one HH you know | | 0.176<br>(0.065)*** | | 0.164<br>(0.065)** |
| 1st LLIN Price in US$ | -0.694<br>(0.419)* | -0.635<br>(0.434) | -0.721<br>(0.423)* | -0.652<br>(0.439) |
| (1st LLIN Price in US$) squared | 0.242<br>(0.206) | 0.201<br>(0.213) | 0.255<br>(0.208) | 0.213<br>(0.215) |
| (1st LLIN Price in US$) cubed | -0.032<br>(0.030) | -0.025<br>(0.031) | -0.034<br>(0.030) | -0.027<br>(0.031) |
| 3rd Degree Polynomial in LLIN Price included | Yes | Yes | Yes | Yes |
| HH controls included | | | Yes | Yes |
| Observations | 483 | 483 | 474 | 474 |
| Mean of Dep. Variable | 0.39 | 0.39 | 0.39 | 0.39 |

*Notes: Coefficient estimates obtained using linear regression with area fixed effects. Sample restricted to households that received a positively priced voucher in the 4 areas where a follow-up survey was conducted among everyone (redeemers and non-redeemers). The mean (standard deviation) of the two independent variables of interest are: 0.6 (1.04) for the number of HHs known to have redeemed their voucher, and 0.11 (0.31) for whether the LLIN was recommended.*

**Table 7.** *Diffusion Effects: Experimental Evidence*

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Redeemed 1st LLIN voucher | | | | | | | | | | | |
| ***Within 250m radius*** | | | | | | | | | | | | |
| Share of study households with max subsidy | 0.124<br>(0.069)* | 0.114<br>(0.070) | | | | | | | | | | |
| Share of study households using LLIN (instrumented with *Share with max subsidy*) | | | 0.266<br>(0.148)* | 0.248<br>(0.152) | | | | | | | | |
| ***Within 500m radius*** | | | | | | | | | | | | |
| Share of study households with max subsidy | | | | | 0.188<br>(0.104)* | 0.220<br>(0.108)** | | | | | | |
| Share of study households using LLIN (instrumented with *Share with max subsidy*) | | | | | | | 0.362<br>(0.201)* | 0.443<br>(0.218)** | | | | |
| ***Within 750m radius*** | | | | | | | | | | | | |
| Share of study households with max subsidy | | | | | | | | | 0.198<br>(0.131) | 0.250<br>(0.136)* | | |
| Share of study households using LLIN (instrumented with *Share with max subsidy*) | | | | | | | | | | | 0.497<br>(0.332) | 0.699<br>(0.389)* |
| Total # of study households | 0.003<br>(0.003) | 0.004<br>(0.003) | 0.002<br>(0.003) | 0.003<br>(0.003) | 0.002<br>(0.001)* | 0.003<br>(0.001)** | 0.002<br>(0.001) | 0.002<br>(0.001)** | 0.001<br>(0.001)* | 0.002<br>(0.001)** | 0.001<br>(0.001) | 0.002<br>(0.001)** |
| 3rd Degree Polynomial in LLIN Price | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| HH controls | | Yes | | Yes | | Yes | | Yes | | Yes | | Yes |
| Observations | 987 | 980 | 987 | 980 | 987 | 980 | 987 | 980 | 987 | 980 | 987 | 980 |
| Mean of Dep. Variable | 0.394 | 0.394 | 0.394 | 0.394 | 0.394 | 0.394 | 0.394 | 0.394 | 0.394 | 0.394 | 0.394 | 0.394 |

*Notes: Sample restricted to households that received a positively priced voucher at baseline (1st voucher). Coefficient estimates obtained using linear regressions with area fixed effects. Note that in the presence of contrast effects, the exclusion restriction for the instruments used in rows 2, 4 and 6 will not hold.*

**Table A1.** *Attrition*

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Redeemed 1st Voucher but Missing in 1st Follow-Up | | | | Attrited before distribution of 2nd LLIN voucher | | | |
| 1st LLIN Price in US$ | 0.042 | | 0.034 | | 0.012 | | 0.030 | |
| | (0.087) | | (0.089) | | (0.064) | | (0.066) | |
| (1st LLIN Price in US$) squared | -0.034 | | -0.031 | | 0.011 | | 0.000 | |
| | (0.063) | | (0.065) | | (0.043) | | (0.044) | |
| (1st LLIN Price in US$) cubed | 0.007 | | 0.007 | | -0.004 | | -0.002 | |
| | (0.013) | | (0.013) | | (0.008) | | (0.008) | |
| 1st LLIN Price = 0 (Free) | | -0.027 | | -0.022 | | -0.017 | | -0.021 |
| | | (0.041) | | (0.042) | | (0.031) | | (0.032) |
| Single-Headed Household | | | -0.014 | -0.011 | | | -0.019 | -0.024 |
| | | | (0.038) | (0.038) | | | (0.032) | (0.032) |
| Distance from retail shop where voucher could be redeemed | | | 0.015 | 0.014 | | | -0.002 | -0.001 |
| | | | (0.007)* | (0.007)* | | | (0.007) | (0.007) |
| Age of household head | | | -0.003 | -0.003 | | | -0.001 | -0.001 |
| | | | (0.001)*** | (0.001)*** | | | (0.001) | (0.001) |
| Number of children (less than 18) | | | 0.000 | 0.000 | | | -0.006 | -0.006 |
| | | | (0.004) | (0.004) | | | (0.004) | (0.004) |
| Female head has completed primary school | | | -0.028 | -0.027 | | | -0.007 | -0.008 |
| | | | (0.030) | (0.030) | | | (0.026) | (0.026) |
| Natural log of household assets index value in US$ | | | -0.021 | -0.021 | | | -0.017 | -0.017 |
| | | | (0.013) | (0.013) | | | (0.010)* | (0.010) |
| At least one member of HH has a bank account | | | 0.012 | 0.010 | | | -0.034 | -0.038 |
| | | | (0.038) | (0.038) | | | (0.037) | (0.037) |
| Share of HH members that slept under a net the previous night | | | 0.014 | 0.016 | | | 0.021 | 0.021 |
| | | | (0.035) | (0.035) | | | (0.030) | (0.030) |
| HH ever received a free bednet | | | -0.032 | -0.032 | | | -0.009 | -0.009 |
| | | | (0.027) | (0.027) | | | (0.023) | (0.023) |
| Declared willingness to pay for a bed net at baseline (US$) | | | 0.002 | 0.002 | | | -0.006 | -0.006 |
| | | | (0.008) | (0.008) | | | (0.007) | (0.007) |
| Constant | | | 0.291 | 0.301 | | | 0.228 | 0.251 |
| | | | (0.097)*** | (0.092)*** | | | (0.076)*** | (0.070)*** |
| Observations | 499 | 499 | 496 | 496 | 644 | 644 | 635 | 635 |
| Mean of Dep. Variable in non-free group | 0.10 | | | | 0.07 | | | |
| ***Estimated effect of a price increase:*** | | | | | | | | |
| from $0 to $1 | 0.02 | | 0.01 | | 0.02 | | 0.03 | |
| p-value | 0.698 | | 0.797 | | 0.539 | | 0.402 | |
| from $1 to $2 | -0.01 | | -0.01 | | 0.02 | | 0.01 | |
| p-value | 0.777 | | 0.813 | | 0.389 | | 0.523 | |

*Notes: Coefficient estimates obtained using linear regression with area fixed effects. Price varies from 0 to US$3.8. Baseline characteristics are mossing for a few households. The sample in columns 1-4 is restricted to those who redeemed their 1st voucher. The sample in columns 5-8 is restricted to households in the 4 study areas where the 2nd voucher was distributed and the 1-yr follow-up was done with both redeemers and non-redeemers of the 1st voucher.*

**Table A2.** *Effect of price on observed usage of first LLIN*

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Enumerator saw study LLIN hanging at 1st follow-up | | | | Enumerator saw study LLIN hanging at 2nd follow-up | | | |
| 1st LLIN Price in US$ | -0.079 | | -0.072 | | -0.139 | | -0.099 | |
| | (0.154) | | (0.159) | | (0.132) | | (0.136) | |
| (1st LLIN Price in US$) squared | 0.009 | | -0.003 | | 0.016 | | -0.025 | |
| | (0.113) | | (0.116) | | (0.104) | | (0.106) | |
| (1st LLIN Price in US$) cubed | -0.002 | | 0.001 | | 0.004 | | 0.012 | |
| | (0.023) | | (0.023) | | (0.021) | | (0.021) | |
| 1st LLIN Price = 0 (Free) | | 0.103 | | 0.106 | | 0.153 | | 0.158 |
| | | (0.071) | | (0.073) | | (0.053)*** | | (0.056)*** |
| Household level controls included | | | Yes | Yes | | | Yes | Yes |
| Observations | 470 | 470 | 467 | 467 | 275 | 275 | 273 | 273 |
| Mean of Dep. Variable | 0.67 | | | | 0.82 | | | |
| ***Estimated effect of a price increase:*** | | | | | | | | |
| from $0 to $1 | -0.07 | | -0.07 | | -0.12 | | -0.11 | |
| p-value | 0.315 | | 0.326 | | 0.038 | | 0.063 | |
| from $1 to $2 | -0.07 | | -0.07 | | -0.06 | | -0.09 | |
| p-value | 0.174 | | 0.139 | | 0.219 | | 0.095 | |

*Notes: Coefficient estimates obtained using linear regression with area fixed effects. Price varies from 0 to US$3.8. Baseline willingness to pay and education level not available for all households.*

**Table A3.** *Health Effect*

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Had malaria in the month preceding the 1-yr Follow-up Survey | | | | | | | | | | | |
| 1st LLIN Price in US$ | 0.091 | | | 0.113 | | | | | | | | |
| | (0.056) | | | (0.060)* | | | | | | | | |
| (1st LLIN Price in US$) squared | -0.056 | | | -0.067 | | | | | | | | |
| | (0.039) | | | (0.041) | | | | | | | | |
| (1st LLIN Price in US$) cubed | 0.008 | | | 0.010 | | | | | | | | |
| | (0.007) | | | (0.007) | | | | | | | | |
| 1st LLIN Price = 0 (Free) | | -0.029 | | | -0.040 | | | | | | | |
| | | (0.026) | | | (0.027) | | | | | | | |
| 1st LLIN Price ≤ 50 Ksh (High Subsidy) | | | -0.020 | | | -0.032 | | | | | | |
| | | | (0.023) | | | (0.023) | | | | | | |
| Experimented with 1st LLIN (instrumented with polynomial in price) | | | | | | | -0.006 | -0.023 | | | | |
| | | | | | | | (0.058) | (0.056) | | | | |
| Experimented with 1st LLIN (instrumented with "free" dummy) | | | | | | | | | -0.074 | -0.099 | | |
| | | | | | | | | | (0.066) | (0.067) | | |
| Experimented with 1st LLIN (instrumented with "High Subsidy" dummy) | | | | | | | | | | | -0.063 | -0.097 |
| | | | | | | | | | | | (0.074) | (0.070) |
| Household level controls included | | | | Yes | Yes | Yes | | Yes | | Yes | | Yes |
| Observations | 961 | 961 | 961 | 946 | 946 | 946 | 961 | 946 | 961 | 946 | 961 | 946 |
| Mean of Dep. Variable in non-free group | 0.093 | | | | | | | | | | | |
| ***Estimated effect of 1st voucher price increase:*** | | | | | | | | | | | | |
| from $0 to $1 | 0.044 | | | 0.057 | | | | | | | | |
| p-value | p=.111 | | | p=.051 | | | | | | | | |
| from $1 to $2 | -0.018 | | | -0.017 | | | | | | | | |
| p-value | p=.377 | | | p=.39 | | | | | | | | |

*Notes: Sample restricted to the four areas where the first year follow-up was conducted for both redeemers and non-redeemers of the 1st LLIN voucher. Coefficient estimates obtained using linear regression with area 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.*

**Table A4.** *Exposure Variables*

*Panel A. Summary Statistics*

| | Mean | Std. Dev | Min | Max | Median |
|---|---|---|---|---|---|
| *Within 250m radius* | | | | | |
| Share with max subsidy | 0.22 | 0.22 | 0.00 | 1.00 | 0.20 |
| Share using LLIN | 0.23 | 0.25 | 0.00 | 1.00 | 0.19 |
| # with max subsidy | 1.39 | 1.50 | 0 | 8 | 1 |
| # using LLIN | 1.51 | 1.85 | 0 | 10 | 1 |
| Total # of sampled households | 5.96 | 5.24 | 0 | 31 | 5 |
| *Within 500m radius* | | | | | |
| Share with max subsidy | 0.23 | 0.16 | 0.00 | 1.00 | 0.22 |
| Share using LLIN | 0.25 | 0.19 | 0.00 | 1.00 | 0.24 |
| # with max subsidy | 4.40 | 3.53 | 0 | 17 | 4 |
| # using LLIN | 5.05 | 4.54 | 0 | 21 | 4 |
| Total # of sampled households | 18.92 | 13.28 | 0 | 63 | 18 |
| *Within 750m radius* | | | | | |
| Share with max subsidy | 0.23 | 0.13 | 0.00 | 1.00 | 0.23 |
| Share using LLIN | 0.26 | 0.16 | 0.00 | 1.00 | 0.26 |
| # with max subsidy | 8.53 | 5.76 | 0.00 | 25 | 8 |
| # using LLIN | 9.62 | 7.25 | 0.00 | 32 | 8 |
| Total # of sampled households | 35.55 | 20.65 | 0.00 | 82 | 38 |

*Panel B. Exogeneity of Price to Social Network Variables*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | 1st LLIN Price in US$ | | | | | |
| *Within 250m radius* | | | | | | |
| Share with max subsidy | 0.234 | | | | | |
| | (0.134)* | | | | | |
| # with max subsidy | | 0.023 | | | | |
| | | (0.031) | | | | |
| Total # of sampled households | 0.002 | -0.002 | | | | |
| | (0.006) | (0.009) | | | | |
| *Within 500m radius* | | | | | | |
| Share with max subsidy | | | 0.164 | | | |
| | | | (0.204) | | | |
| # with max subsidy | | | | 0.001 | | |
| | | | | (0.017) | | |
| Total # of sampled households | | | 0.001 | 0.001 | | |
| | | | (0.002) | (0.004) | | |
| *Within 750m radius* | | | | | | |
| Share with max subsidy | | | | | 0.275 | |
| | | | | | (0.257) | |
| # with max subsidy | | | | | | 0.006 |
| | | | | | | (0.012) |
| Total # of sampled households | | | | | 0.000 | -0.001 |
| | | | | | (0.001) | (0.003) |
| Observations | 987 | 987 | 987 | 987 | 987 | 987 |

*Notes: Coefficient estimates obtained using linear regression with area fixed effects. Sample restricted to households that received a positive-price voucher.*

**Table A5.** *Diffusion Effects: Experimental Evidence with Alternative Specification*

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Redeemed 1st LLIN voucher | | | | | | | | | | | |
| *Within 250m radius* | | | | | | | | | | | | |
| # of study households with max subsidy | 0.013 | 0.011 | | | | | | | | | | |
| | (0.016) | (0.016) | | | | | | | | | | |
| # of study households using LLIN (instrumented with # *with max subsidy*) | | | 0.030 | 0.024 | | | | | | | | |
| | | | (0.037) | (0.037) | | | | | | | | |
| *Within 500m radius* | | | | | | | | | | | | |
| # of study households with max subsidy | | | | | 0.008 | 0.010 | | | | | | |
| | | | | | (0.009) | (0.009) | | | | | | |
| # of study households using LLIN (instrumented with # *with max subsidy*) | | | | | | | 0.016 | 0.019 | | | | |
| | | | | | | | (0.017) | (0.017) | | | | |
| *Within 750m radius* | | | | | | | | | | | | |
| # of study households with max subsidy | | | | | | | | | 0.008 | 0.009 | | |
| | | | | | | | | | (0.006) | (0.006) | | |
| # of study households using LLIN (instrumented with # *with max subsidy*) | | | | | | | | | | | 0.020 | 0.024 |
| | | | | | | | | | | | (0.016) | (0.017) |
| Total # of study households | 0.001 | 0.002 | -0.005 | -0.002 | 0.000 | 0.001 | -0.002 | -0.002 | 0.000 | 0.000 | -0.004 | -0.005 |
| | (0.004) | (0.005) | (0.010) | (0.010) | (0.002) | (0.002) | (0.005) | (0.005) | (0.002) | (0.002) | (0.005) | (0.005) |
| 3rd Degree Polynomial in LLIN Price | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| HH controls | | Yes | | Yes | | Yes | | Yes | | Yes | | Yes |
| Observations | 987 | 980 | 987 | 980 | 987 | 980 | 987 | 980 | 987 | 980 | 987 | 980 |
| Mean of Dep. Variable | 0.394 | 0.394 | 0.394 | 0.394 | 0.394 | 0.394 | 0.394 | 0.394 | 0.394 | 0.394 | 0.394 | 0.394 |

*Notes: Sample restricted to households that received a positively priced voucher at baseline (1st voucher). Coefficient estimates obtained using linear regressions with area fixed effects. Note that in the presence of contrast effects, the exclusion restriction for the instruments used in rows 2, 4 and 6 will not hold.*