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

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Abstract

Short-run subsidies for health products are common in poor countries. How do they affect long-run adoption? We present a model of technology adoption in which people learn about a technology's effectiveness by using it (or observing others using it) for some time, but people quit using it too early if they face higher-than-expected usage costs (e.g., side effects). The extent to which one-off subsidies increase experimentation, and thereby affect learning and long-run adoption, then depends on people's priors on these usage costs. One-off subsidies can also affect long-run adoption through reference-dependence: People might anchor around the subsidized price and be unwilling to pay more for the product later. We estimate these effects in a two-stage randomized field experiment in Kenya. We find that, for a new technology with a lower usage cost than the technology it replaces, short-run subsidies increase long-run adoption through experience and social learning effects. We find no evidence that people anchor around subsidized prices.

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

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1 Introduction

Between nine and ten million children under age five die every year, most of them in developing countries.[1] It is estimated that nearly two thirds of these deaths could be averted using existing preventative technologies, such as vaccines, insecticide-treated materials, vitamin supplementation, or point-of-use chlorination of drinking water.[2] An important question yet to be answered is how to increase adoption of these technologies.

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

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

Free or highly subsidized distribution of a product in the short run may increase demand in the long run if the product is an experience good. Beneficiaries of a free or highly subsidized sample will be more willing to pay for a replacement after experiencing the benefits and learning the true value of the product if they previously had underestimated these benefits. This learning might 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. Furthermore, if short-run adoption of the product leads to positive health and productivity effects, beneficiaries of a subsidized sample might have more cash-on-hand to invest in sustained adoption.

[1] Black et al, 2003.
[2] Jones et al., 2003.

These positive effects hinge upon people's making use of a product or technology that they receive for free or at a highly subsidized price. This might not be the case, however. Households that are not willing to pay a high monetary price for a product might also be unwilling to pay the non-monetary costs associated with using the product on a daily basis (Chassang, Padro i Miquel and Snowberg, 2009; Ashraf, Berry and Shapiro, *forthcoming*).

Furthermore, consumers could take previously encountered prices as reference points, or anchors, which would affect their subsequent 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). Under such reference-dependent preferences, subsidies could generate an "entitlement effect": 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.

The view that these negative effects might dominate the standard positive learning effects is quite prevalent among development practitioners. As the Boston Globe summarized: *"The Holy Grail of international development has long been sustainability – [...] for several decades it's been the conventional wisdom that unless people spend money on something they will be unlikely to value it – or use it. Give things away and they will be taken for granted, it's thought."*[3] For example, the non-profit organization One Acre Fund, working with rural households in East Africa, lists as its core value: "*We don't give handouts - we empower permanent life change. Lasting change must rely on the poor themselves.*"[4] There is, however, no rigorous evidence to date as to what short-run subsidies do to long-run adoption of new technologies.

This paper aims to inform this debate in two steps. First, we provide a model for understanding the role of prices in the adoption of technologies for which adoption requires not only acquisition, but also repeated usage of the technology once acquired. We generate predictions as to the circumstances and technology characteristics under which short-run subsidies will increase the long run level of adoption. Second, we test the predictions of the model through a field experiment for one specific technology (long-lasting antimalarial bednets) in one specific

[3]Christopher Shea, "A Hand Out, not a Hand Up", November 2007. Article retrived on 12/13/2009 at http://www.boston.com/news/education/higher/articles/2007/11/11/a_handout_not_a_hand_up/

[4]Retrieved on March 2, 2009 at this link: http://www.oneacrefund.org/how_it_works/core_values

set of circumstances.

The key feature of our model is that households are uncertain about two elements of a newly introduced health technology or health product: the effectiveness of the product, and its non-monetary cost of usage (for example, how "hot" it is to sleep under a bednet, or how bad a deworming pill tastes). People immediately learn the cost of usage upon buying the product, say by using the product for one day. In contrast, learning about the effectiveness takes some time: people who use the product receive (publicly observable) signals about its effectiveness, but these signals are imprecise, in particular in environments where individual-specific health shocks are common. In this context, we show that subsidies may have no effect on the adoption of the product if people initially *underestimate* its non-monetary usage cost. This is because subsidies affect the purchase decision of households who have a low prior about the product's effectiveness. These households will likely not use the product once they learn its true usage cost. In this context, higher subsidies may not generate additional signals about the product's effectiveness and therefore may not affect the dynamic adoption process. In contrast, subsidies will increase the level of experimentation and thus learning about the new product if people initially *overestimate* the non-monetary usage cost. This, in turn, will speed up the diffusion process and increase long-run adoption, unless people exhibit reference-dependent preferences and anchor around subsidized prices.

To test these predictions, we conducted a field experiment in Kenya with a new technology whose non-monetary usage cost was likely *overestimated* by households at baseline. The technology we introduced is the Olyset long-lasting insecticide-treated bed net (LLIN), a recent innovation in malaria control. The Olyset LLIN is significantly more comfortable to sleep under than traditional bednets, and also more effective in the long run. The experiment involved 1,120 households in Kenya and included two phases. In Phase 1, subsidy levels for Olyset LLINs were randomly assigned across households within six villages. Households had three months to acquire the LLIN at the subsidized price they had been assigned to. Prices varied from $0 to $3.80, which is about twice the average daily wage for casual agricultural work in the study area. In Phase 2, a year later, all households in four villages were given a second opportunity to acquire an Olyset LLIN, but this time everyone faced the same price ($2.30). 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 traditional nets were available on the market at the retail price of $1.50.

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

We first test whether subsidies increased the level of experimentation. We find very strong evidence for this prediction, consistent with the fact that the new technology introduced was less costly to use than households anticipated. We then test whether this higher level of experimentation led to positive or negative updating about the private returns to LLIN use. To this end, we examine how full or very large subsidies for an LLIN in Phase 1 affect willingness to pay for an LLIN in Phase 2. We find 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. This suggests the presence of a learning effect which dominates any potential anchoring effect. We then test specifically for the presence of anchoring. We find some evidence of anchoring at higher prices, but no anchoring around zero or very low prices. Overall, our evidence suggests important learning-by-doing effects among subsidy receipients.

We then turn to studying the social effects of subsidies. To avoid the classic reflection problem in the estimation of social effects (Manski, 1993), we exploit the exogenous variation in the density of households who received a free or highly subsidized LLIN in Phase 1 as a source of exogenous variation in exposure to signals about the returns to using the product. We find that households facing a positive price in Phase 1 were more likely to purchase the LLIN when the density of households around them who received a free or highly subsidized LLIN was greater, suggesting social learning effects.

Overall, our results suggest that, consistent with the model prediction given the characteristics of the technology studied, the total effect of short-run subsidies on long-run adoption of LLINs is positive. Previously encountered prices matter, but more so through their effect on available knowledge about the product than through entitlement effects.

The model helps reconcile our findings with those of two previous studies, Ashraf, Berry and Shapiro (*forthcoming*) and Kremer and Miguel (2007), which both found results somewhat opposite to ours. Ashraf, Berry and Shapiro find that subsidies for a water chlorination product in urban Zambia increased the rate of purchase but did not increase the overall rate of experimentation with the product. Their result is consistent with the case of our model in which people underestimate the cost of using chlorine at the time they make the decision to purchase it (e.g., they underestimate the chlorinated taste of the water). In that case, those induced to purchase the product by a subsidy are not motivated to use it once they learn its true usage cost.

Kremer and Miguel (2007) use a randomized evaluation of a school-based deworming program in Kenya to estimate 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 also consistent with our model. Deworming pills generate important negative side effects, making the non-monetary cost of deworming relatively high. Households in the Kremer and Miguel study likely underestimated these costs initially. Subsidies for deworming enabled households to learn the true usage cost of deworming and revise their beliefs about the private returns to using the drug downwards, thus leading to lower long-run adoption.

Our findings also help shed light on the Kremer and Miguel (2007) result 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. 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 effects likely played a negligible role in the demand drop that they observed following the price increase.

Our paper contributes to a growing 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.[5] Our paper

[5] 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. There are few empirical studies of social learning outside agriculture. 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

also contributes to the empirical "psychology and economics" literature, testing behavioral economics in the field (see DellaVigna, 2009, for a review), and complements earlier papers that have estimated, in rich countries, how the willingness to pay for a product can be affected by anchors (Ariely, Loewenstein and Prelec, 2003), previously encountered prices (Simonsohn and Loewenstein, 2006; Mazar, Koszegi and Ariely, 2009), or the range of options available (McFadden, 1999; Heffetz and Shayo, 2009). Finally, our paper makes a contribution to the literature on experimentation and experience goods pricing (Bergemann and Valimaki, 2000, 2006).

The remainder of the paper is as follows. Section 2 presents a model of technology adoption in the presence of ex-ante uncertainty about both the effectiveness of the technology and its usage cost. Section 3 presents some background information on malaria and the preventative technology studied in the application, and then describes the experimental design. Section 4 presents the results on the direct effect of subsidies, and Section 5 presents the results on their indirect effects via social learning. Section 6 concludes.

2 Theoretical Framework

This section presents a general framework for understanding the adoption of a new preventative health technology. The goal is to clarify the potential channels through which a one-time subsidy can change the long-run level of adoption, and to provide empirically implementable tests of their relative importance. We use an experimentation model similar to those in Moscarini (2005) and Moscarini and Smith (2001).

We consider a technology for which health-effective adoption requires not only acquiring the technology, but also repeatedly using it over its lifespan. In addition to anti-malarial bednets (which can last for 3-4 years and are supposed to be used nightly), examples of such technologies include water chlorination products (typically sold in bottles large enough to treat a 6-member household's drinking water daily for 1 month), iron pills (sold in bulk), or water filters (that have a lifespan of 6 months and should be used daily). We consider that, at the time households decide whether to acquire the technology, they are uncertain about both the effectiveness of

network. Oster and Thornton (2009) find evidence of peer effects in the usage of a new female hygiene product provided for free.

the technology and the cost associated with using the technology. Households have two sources of information: their own experimentation with the technology, and the experience of their neighbors. Learning about the usage cost is relatively quick upon ownership of the technology (one only needs to use it a few times to learn its usage cost), but learning about the health effectiveness takes time, as households receive only noisy signals.

2.1 Set-up

2.1.1 Information

Benefits The average effectiveness μ of a new health technology is ex–ante uncertain. Individuals have a prior belief on μ, concentrated on two points: $p_0 = \Pr(\mu = 1) = 1 - \Pr(\mu = 0) \in (0, 1)$, where $\mu = 1$ if the technology is "good" (i.e., more effective than the status quo technology by 1 util-equivalent), and $\mu = 0$ if the technology is "bad" (i.e., not more effective than the status quo technology.)

If households own and use the technology, they get a signal about its quality. The signal is subject to idiosyncratic noise that keeps μ hidden and creates the inference problem. We consider that it is a normal random variable, a Brownian Motion with drift μ and known variance σ^2. Over time, individuals observe the signals $\langle X_t \rangle$, generating a filtration $\left\{\mathcal{F}_t^X\right\}$, and update their belief in a Bayesian fashion from the prior p_0 to the posterior $p_t \equiv \Pr(\mu = 1|\mathcal{F}_t^X)$. The Brownian diffusion is characterized by:

$$
\begin{aligned}
\mathbb{E}(dp_t) &= 0 \\
\mathbb{E}(dp_t^2) &= (\frac{1}{\sigma})^2 p_t^2 (1 - p_t)^2 dt
\end{aligned}
$$

where $\frac{1}{\sigma}$ is the signal-to-noise ratio.

To update beliefs, households use both their own experience and that of J neighbors. We consider that each household i has a location on a two-dimensional map, and that J signals are drawn from the set of households within a distance d in each direction. If a fraction w_{it} of households in this radius experimented (i.e., bought and used) the technology in period t, household i receives $w_{it}J$ signals. We consider that σ decreases with the number of signals received: the more neighbors experiment, the higher the signal to noise ratio.

Costs There are two types of costs associated with the technology: the monetary cost of acquiring the product (price m); and the time and/or utility cost of using it, denoted by c, with $0 < c < 1$.

Households readily observe m, but c is uncertain ex-ante. Households' prior on c is a probability distribution denoted $F(.)$. As soon as they acquire the technology, they learn the true c. They can also learn c from their neighbors. We consider that each household has a probability λ to learn the true c from each neighboring household that acquired the technology and learned c. In other words, household i's belief about c at time t is:

$$F_t^i(c) = \left\{ \begin{array}{l} c \text{ if } i \text{ acquired the technology at any time prior to } t \\ c \text{ if } i \text{ learned } c \text{ from a neighbor} \\ F_0^i(.) \text{ if } i \text{ never acquired the technology and didn't learn from any neighbor} \end{array} \right\}$$

If i has $w_{it}J$ neighbors who acquired the technology at time t, the likelihood that household i hasn't learned c from a neighbor by time t is $(1-\lambda)^{w_{it}J}$.

2.1.2 Payoffs

Time is continuous and the interest rate is r. Conditional on owning the technology and using it, and given the belief p_t about the effectiveness, a household's expected instantaneous payoff at time t is $\Pi_t = p_t - c$. The household's value of using the technology if it is good with probability p_t solves the Bellman equation:

$$V_1(p_t, c) = r\Pi(p_t, c)dt + (1 - rdt) \cdot \mathbb{E}(V_1(p_t + dp_t, c))$$

Applying Ito's lemma: $V_1(p_t + dp_t, c) = V_1(p_t) + V_1'(p_t)\mathbb{E}(dp_t) + \frac{1}{2}V_1''(p_t)\mathbb{E}(dp_t^2)$, we obtain the following ordinary differential equation:

$$rV_1(p_t, c) = r(p_t - c) + \frac{1}{2}p_t^2(1-p_t)^2\frac{1}{\sigma_1^4}V_1''(p_t, c)$$

The first term represents the current payoff, and the second term corresponds to the option value of experimenting: the speed of learning $\frac{1}{2}p^2(1-p)^2\frac{1}{\sigma^4}$ is converted into payoff units by the convexity of $V_1''(p_t)$, because information spreads posterior beliefs and empowers more informed decisions later on.

Households that don't experiment themselves can still learn about the effectiveness from neighbors. Their value function solves:

$$rV_0(p_t, c) = \frac{1}{2}p_t^2(1-p_t)^2\frac{1}{\sigma_0^4}V_0''(p_t, c)$$

where $\sigma_0 > \sigma_1$. In other words, the signal-to-noise ratio is lower when people do not receive their own signal and purely rely on their neighbors' signals.

2.1.3 Optimal Experimentation (conditional on purchase)

Lemma 1 *The value function of the household is an increasing and convex function of* p:

$$V_1(p, c) = \begin{Bmatrix} p - c + \psi(c)p^{1/2-\sqrt{1/4+2r\sigma^2}}(1-p)^{1/2+\sqrt{1/4+2r\sigma^2}} & \textit{if } p \geq \underline{p}(c) \\ V_0(p, c) = 0 & \textit{if } p \leq \underline{p}(c) \end{Bmatrix}$$

where $\psi(c)$ *and the threshold* $\underline{p}(c) \in [0, c]$ *uniquely solve the boundary conditions* $V_1(\underline{p}) = V_0(\underline{p}) = 0$ *(value matching) and* $V_1'(\underline{p}) = V_0'(\underline{p}) = 0$ *(smooth pasting).*

The value function $V_1(p, c^*)$ is drawn in Figure 1A. Below the threshold $\underline{p}$, households choose not to use the technology, even conditional on owning it. For any $p \geq \underline{p}(c)$, they use the technology. The threshold $\underline{p}(c)$ is lower than the "myopic" threshold c, since the option value of experimentation make households willing to take a negative current payoff.

2.1.4 Optimal Purchase Decision

The top panel of Figure 1b plots $V_1(p, c)$ as a function of c, for a given prior p_t. The function is decreasing and convex, and shifts outwards as p_t increases.

Proposition 1 *Call* $\bar{c}(p)$ *the inverse of* $\underline{p}(c)$. *If the household has the prior* p_t *on the effectiveness and the prior distribution* $F(.)$ *on the cost of usage* c, *the household will buy the technology at price* m_t *if and only if*

$$m_t \leq \int_0^{\bar{c}(p)} V_1(p_t, c)dF(c)$$

Since $V_1(p, c)$ is increasing in the prior on p, the likelihood of purchase increases with the prior p_t. For a given prior p_t, whether a household buys the technology will depend on its prior

$F(.)$ on the usage cost. The bottom panel of Figure 1b plots three possible prior distributions. For a given prior p_t, households with prior distribution $F_1(.)$ will be willing to pay a higher price for the technology than households with the prior distributions $F_2(.)$ or $F_3(.)$.

When the new technology is first introduced, the priors p and $F(.)$ might depend on the availability of comparable technologies. In a context like the one in which conducted our field experiment, where the status quo technology is already relatively good and the new technology is similar to the old technology (e.g., the status quo technology is an insecticide-treated net and the new technology is a long-lasting insecticide treated net), individuals may have relatively optimistic priors about the effectiveness of the new technology. In contrast, for technologies that are radically new (e.g., in that they rely on unknown scientific principles), individuals might start with pessimistic beliefs (low p) and adoption might be very limited, unless the technology is heavily subsidized for at least some individuals. A good example is that of insecticide-treated curtains, that provide the insecticide halo necessary to repel mosquitoes but do not provide the intuitive "physical barrier" against mosquitoes that people tend to think is the critical component of a bed net.

2.2 Static Effects of Price Subsidies on Experimentation Level

The dynamic effects of price subsidies will depend on their static effects on the level of experimentation: the more households experiment with the technology, the faster the learning about effectiveness will be. In our model, the effect of a price subsidy on the total amount of experimentation (and hence learning) is never going to be negative, but it might not be strictly monotonic. Here is why:

A subset S_A of households that get enticed to buy the product by the subsidy will experiment with the product once they have it: those are the households that initially overestimate the usage cost. Indeed, for a given prior on the effectiveness p, lower prices crowd in people with higher priors on the usage cost c. Those who get surprised by a realized usage cost c^* lower than expected can only increase their level of experimentation as a result.

On the other hand, a subset S_B of households that get enticed to buy the product by the subsidy will not experiment with the product once they have it: those are the households that initially underestimate the usage cost. Indeed, for a given prior on c, lower prices crowd in people with lower priors on p. Those who get surprised by a realized usage cost c^* that is higher than expected will not experiment if their prior on the effectiveness is too low, namely if $p \leq \underline{p}(c^*)$.

Overall, this suggests that the share of households that experiment with the product among those who acquire it might not be a monotonic function of the subsidy level. As a result, short-run adoption (and hence learning) might not be strictly increasing in the subsidy level. In particular, if $S_A = \{\varnothing\}$ for subsidy levels above a certain threshold, then any further increase in the subsidy above that threshold would not lead to any increase in experimentation: the level of experimentation would level off.[6]

In the field experiment below, we find that adoption (acquisition + experimenation) is strictly increasing with the subsidy level (strictly decreasing with the price level). We take it as evidence that, in our context, the subset S_A is not empty. In addition, we find that the share of people experimenting with the technology (among those who own it) does not decrease with the subsidy level. This implies that the subset S_A is larger than the subset S_B : people tended to overestimate the usage cost of our technology at baseline, rather than underestimate it, and therefore high subsidies increased immediate adoption and learning.

In contrast, Ashraf, Berry and Shapiro (*forthcoming*) find that higher subsidies increased the fraction of people who acquired a water chlorination product but did not use it. This suggests that, in their context, subset S_B was larger than subset S_A : a relatively large fraction of people underestimated the cost of using chlorine in their water (they underestimated how bad their water would taste if they used chlorine), and as a result, they chose not to experiment once they learned the true usage cost.

2.3 Dynamic Effects

We have shown that the magnitude of the static effect of subsidies on the level of experimentation (and thus learning) will depend on the priors' distributions. This implies that the dynamic effect of subsidies on adoption will also depend on the priors. But besides the learning effect,

[6] The prior belief about the effectiveness of the product could also be an increasing function of the observed price (Bagwell and Riordan, 1991). If people face different prices, they might start with heterogeneous priors. We abstract from this here, and shut down this mechanism in the experiment by informing everyone of the unsubsidized price. Alternatively, the prior belief about the effectiveness could be an increasing function of the subsidy size. If so, the experimentation level would strictly increase with the subsidy level.

there are two additional dynamic effects through which short-run subsidies might affect longer-run demand: an anchoring effect and an income effect. Below, we first describe these effects, and then describe what our empirical tests can tell us about them.

2.3.1 Anchoring Effect

Let's now consider that the utility of individual i is composed of two additive terms: intrinsic utility and gain-loss utility. Intrinsic utility is a function of absolute outcomes. Gain-loss utility captures reference-dependence. Following Kőszegi and Rabin (2006), we formalize reference-dependence as follows. Denote $\hat{m}_{it}$ household i's reference price for the technology at time t, then paying a price m_t for the technology generates gain-loss utility $r(\hat{m}_{it} - m_t)$. To allow for loss aversion, we allow the function r to be kinked at zero. For example, r could be two-piece linear, with a slope $\lambda_G \geq 0$ for gains (when $\hat{m}_{it} \geq m_{it}$), and a slope of $\lambda_L \geq \lambda_G$ for losses (when $\hat{m}_{it} < m_{it}$).

The decision to purchase the bednet is now slightly modified compared to proposition 2.

Proposition 2 *At any period t, given reference price $\hat{m}_{it}$, household i will buy the technology at price m_t if and only if*

$$m_t \leq \int_0^{\bar{c}(p)} V(p_0, c)dF(c) + r(\hat{m}_{it} - m_t)$$

This means that, for a given set of priors p and $F(c)$, households who face a price higher (lower) than their reference point for the technology will be less (more) likely to purchase it than those who face a price equal to their reference point.

We consider that the reference price $\hat{m}_{it}$ evolves as follows. We assume that prior to the introduction of the technology, all households have a common reference point $\hat{p}_0$, based on the cost of the status quo technology. After the new technology is introduced in period k, we suppose that household i revises its reference price to $\hat{m}_{i,k+t} = p_{ik}, \forall t \geq 1$. In other words, the household "anchors" around the first offer price it receives.

2.3.2 Income (via Health) Effect

If the health technology is effective, households who experiment with it are likely to get healthier over time. This positive health effect could affect disposable income in two ways. First, healthier households are likely to be more productive and able to generate higher income. Second, healthier households are likely to spend less on malaria treatment expenditures. If subsidies increase the rate of adoption of the new health technology, they might thus increase disposable income as a result.

In addition, for many health technologies, adoption by some households generates positive health externalities for others. For example, in the case of malaria, having more neighbors using a protective device such as a bednet reduces one's own chance of infection (since malaria is transmitted from human to human). There might thus be an indirect effect of subsidies on the health (and thus income) level of those not directly targeted by the subsidy.

2.3.3 Overall direct effect of one-time subsidies on long-run adoption

For household i, the impact of having faced price m_{it} in period t on the household's reservation price $\overline{m}_i$ at a subsequent period $t + \Delta t$ (when the price has become $m_{i,t+\Delta t} > m_{i,t}$) is the sum of three effects:

$$\frac{\partial \overline{m}_{i,t+\Delta t}}{\partial m_{i,t}} = \text{ learning + anchoring + income}$$

The first effect is learning-by-doing about the technology's effectiveness and its usage cost. For both parameters, the learning effect can be zero if the price in period t is such that household i does not acquire the product and does not experiment with the product between time t and time $t + \Delta t$. If the household acquires the product *and* experiments with it, the learning effect will be positive or negative depending on whether the household's initial priors were overestimates or underestimates. The second effect is the anchoring effect, which is positive or zero. The third effect is the income effect, which is negative or zero. The income effect can result from the health effect, or from a mechanical effect of the subsidy on the intertemporal budget constraint: those who paid more for the technology in period t have less money available to invest in the technology again in period $t + \Delta t$.

In the experiment below, we find a negative effect of the period t price on willingness to pay at $t + \Delta t$. In other words, we find that the sum of the three effects in the equation above is negative. We take it as evidence that the anchoring effect is at best modest, and overwhelmed by the sum of learning and income effects. We then specifically test for the presence of anchoring and find no evidence of anchoring around zero or very low prices. We also specifically test for the presence of an income effect, and find inconclusive evidence. Overall, our results suggest that the main effect through which short-run subsidies affect later adoption is a learning effect, which in our context appears positive (people learned the technology was better than what they initially thought).

2.3.4 Overall indirect effect of one-time subsidies on long-run adoption

For household i, the impact of having a given share $s_{-i,t}$ of neighbors that receive a high subsidy level in period t on the household's reservation price $\overline{m}_i$ at a subsequent period $t + \Delta t$ is the sum of three effects:

$$\frac{\partial \overline{m}_{i,t+\Delta t}}{\partial s_{-i,t}} = \text{ learning } + \text{ income } + \text{ free-riding}$$

The first effect represents social learning about the technology's effectiveness and its usage cost. The larger the number of neighbors who receive a low price, the larger the number of neighbors who acquire the product and learn the usage cost. If those neighbors are "badly" surprised by the usage cost and do not experiment with the product (they belong to subset S_B), there will be no learning about the effectiveness. But if neighbors experiment with the product, the faster the learning about the effectiveness (the higher the signal-to-noise ratio). This social learning effect will induce higher or lower adoption in the future depending on whether the household prior p_t was too optimistic or too pessimistic to start with.[7] The other two effects come from the positive health externality. One one hand, having more neighbors experimenting with the product can increase one's own health and thus productivity level and income realization. This corresponds to the second effect, which is likely to be positive. On the other hand, the returns

[7] Having more neighbors that own the product but do not use it (i.e., more neighbors in subset S_B) could actually lead to "mislearning", if people only know their neighbors' ownership status and not their usage status. In other words, if household A knows that neighboring household B owns the product but doesn't know that B is not using the product, household A might think that it receives signals on the effectiveness by observing household B's health level. This would lead household A to incorrectly revise its belief about the effectiveness downwards.

to using the product oneself are lower if one benefits from one's neighbors' own protective behavior. This could lead one to free ride – the third effect, which is likely to be negative.

In the experiment below, we find a positive social effect overall (the sum of the three effects is positive). We take it as evidence that information effects are positive and that the health externality is either too small, or too unobservable by individuals, to generate a free-riding effect that can dominate the learning effect.

3 Experimental Set-Up and Design

3.1 Background on Insecticide-Treated Nets

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 the net is used and washed regularly. The first prototype LLIN, the Olyset® Net, was approved by WHO in 2001, but did not get mass produced until 2006. At the time this study started in Kenya in 2007, the Olyset Net, the LLIN used in this experiment, was not available for sale, and its effectiveness–relative to that of regular ITNs available for sale–was unknown.

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

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

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 1,120 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.[8] 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 0 to 250 Ksh, or at the prevailing exchange rate of 65 Ksh to US$1 at the time, from 0 to US$3.8.[9] 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.

[8] Since Kenya introduced Free Primary Education in 2003, school participation is high. The net primary enrollment rate was estimated at 80% in 2005 and is probably higher now.

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

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.[10] At the end of the interview, the respondent 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.[11] 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. 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. 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.[12]

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 months on average after the voucher had been redeemed. During the follow-up visit, enumerators asked to see the net that was purchased with the voucher, so as to ascertain that it was a study-supplied Olyset LLIN. The follow-up survey also checked whether households had been 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

[10] 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 include controls for the randomized gender assignment.

[11] 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.

[12] 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 visit.

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 as mentioned earlier, it is noticeably 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.[13] 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 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 for the willingness to pay for a replacement LLIN.

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

3.4 Verifying Randomization

A baseline survey was administered at households' homes between April and October 2007, prior to the first voucher distribution. The baseline survey assessed household demographics, socioeconomic status, and bednet ownership and coverage. Table 1 presents summary statistics on 15 household characteristics, and their correlation with the randomized 1^{st} LLIN price assignment. Specifically, we regress each baseline characteristic on a quadratic in the price faced in Phase 1 and a set of area fixed effects:

$$x_{hj} = \tau_1 P_{hj1} + \tau_2 (P_{hj1})^2 + \upsilon_j + \varepsilon_{hj}$$

where x_{hj} represents a baseline characteristic of household h in area j and P_{hj1} is the price faced by household h in Phase 1. We report the coefficient estimates and standard errors for τ_1 (column 3) and τ_2 (column 4). All of the coefficient estimates are small in magnitude and none can be statistically distinguished from zero, suggesting that the randomization was successful at making the price assignment orthogonal to observable baseline characteristics.[14]

3.5 Verifying Compliance with Study Protocol

All households that redeemed their vouchers declared, when interviewed at follow-up, that they had been charged the assigned price when they redeemed their voucher at the shop. This suggests that participating retailers respected the study protocol.

The sales logs kept by participating retailers show that, in total over Phase 1 and Phase 2, 95% of the redeemed vouchers were redeemed by a member of the household that had received the voucher. Only two of the individuals that redeemed a voucher declared having paid to acquire the voucher. This suggests that there was almost no arbitrage between households prior to voucher redemption.

To check 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

[14] Alternative specifications (linear price effect, dummy for "Free 1st LLIN", dummies for each price groups in Figure 1) also show balance across price groups (results available upon request).

were easily recognizable). These home visits were conducted after both Phase 1 and Phase 2. Overall, more than 90% of households that had redeemed a voucher could show the LLIN during the spot check.

4 Results: Learning-by-Doing

4.1 Static Effects of Subsidies on Experimentation

The static effects of subsidies on take-up and usage of the Phase 1 LLIN are presented in Figure 2. Panel A 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 70 to 55% 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, which shows usage rates (among those who redeemed their voucher), suggests that the likelihood that people experimented with their LLIN does not increase with the price paid.[15] As a result, as shown in Panel C, the adoption rate (take-up + experimentation) drops quite rapidly as the price increases (as the subsidy level decreases). These results are robust to adding household-level controls (regression analysis available upon request), and are very similar to those obtained among pregnant women by Cohen and Dupas (2010).[16]

In terms of the framework presented in Section 2, these findings suggest that most households that were enticed to buy the LLIN by the subsidy were overestimating the usage cost at baseline. In other words, subset S_A seems much larger in our context than subset S_B. In this context, higher subsidies are likely to generate important learning-by-doing effects: higher subsidies lead to a much higher share of people experimenting with the product and thereby obtaining a signal about its effectiveness.

[15] We group households into five price (subsidy) groups to avoid running into small sample problems when estimating usage rates (especially at higher prices).

[16] Appendix Table A1 shows that attrition at follow-up was not correlated with price, and therefore the estimates of the effect of price on adoption are unbiased. Appendix Figure A1 shows that, not suprisingly, the time needed to redeem the voucher increased with price.

4.2 Dynamic Effects of Subsidies on Long-Run Adoption

4.2.1 Overall Effect

This section tests whether households who benefitted from a free or highly subsidized LLIN in Phase 1 were more or less willing 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 bednet was affected by the subsidy. This is presented in Figure 3, which is restricted to the sample of households that self-selected into buying the LLIN in Phase 1. Figure 3 presents two averages for each Phase 1 price group: 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.[17] Figure 3 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.[18]

Declared preferences might suffer from social desirability bias, however. Furthermore, they are only available for the self-selected (hence biased) sample of those who acquired the first LLIN. For these reasons, we now turn to studying revealed preferences, namely, the take-up of the second LLIN. We have that information for all households, whether or not they puchrased the first LLIN.

The price of the second voucher was uniform across all households (at 150 Ksh). Figure 4 presents the average purchase rate for the second LLIN offered, for each Phase 1 price group.

[17] Ex-ante willingness to pay increases with the offered price since only households that acquired the first LLIN are included.

[18] 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 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 2 confirms this result. Columns 1 through 6 estimate the following reduced form equations:

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

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

The take-up in the '1st LLIN Free' group is 6.1 percentage points (41%) higher than in the non-free groups, suggesting a learning-by-doing effect (Table 2, 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 3 and 6 of Table 2 present a specification with a "high subsidy" dummy (1st LLIN price $\leq$ 50 Ksh). As was apparent in Figure 4, 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, both without and with household level controls.

Columns 10-12 of Table 2 estimate the following equation:

$$Y_{hj2} = \beta_5 \widehat{U_{hj1}} + X_h^{'}\gamma + \upsilon_j + \varepsilon_{hj}$$

where $\widehat{U_{hj1}}$ indicates whether household h experimented with an LLIN in Phase 1 (i.e., not only bought the LLIN in Phase 1 but also used it), instrumented with either the price faced in Phase 1 and its square (column 10); a dummy indicating whether the price faced in Phase 1 was zero (full subsidy, column 11); or a dummy indicating whether the price faced 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 2.

The estimates of β_5 in these instrumental variable 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" is 0.14 in column 11 and below 0.1 in column 12). 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 anchoring effects. Thus our preferred specifications are the reduced form specifications presented in columns 1-6.

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

4.2.2 Directly Testing for Anchoring

In Figure 5, we directly test for the presence of anchoring by looking at the gap between households' declared willingness to pay for an LLIN at follow-up, and the price paid in Phase 1. We show the distribution of this gap separately for each price group. The first row shows the distribution of the gap "before" (before households received the first voucher and observed the Phase 1 price) and the second row shows the distribution of the gap "after" (at follow-up). A gap of zero means that people declared, at follow-up, being willing to pay exactly the price they were randomly assigned in Phase 1. As in Figure 3, we have this data only for the self-selected sample of households who purchased the LLIN in Phase 1. The evidence in Figure 5 suggests that households who paid a positive price anchored somewhat around the offered price: at follow-up, the distribution of the gap narrows around zero for those in positive price groups. This is not the case for households that received a free LLIN in Phase 1, however. For those, the density at zero is lower at the follow-up than at baseline, suggesting no anchoring at all (utmost left panel, Figure 5).

Note that, as the subsidy was provided by a local research organization, households in the study might have been less likely to exhibit "anchoring" effects than they would have if the subsidy had been implemented nation-wide by the government. On the other hand, since the implementing agency was local, households might have thought they could induce it to provide high subsidies for everyone by boycotting higher prices. It is difficult to gauge the direction of the bias, and it is possible that in other contexts subsidies could lead to anchoring.

4.2.3 Income (via Health) Effect?

Section 4.2.1 has shown that households who received a free or highly subsidized LLIN in Phase 1 were not less likely to buy an LLIN a year later. Rather, they appeared somewhat *more* likely to invest in an LLIN in Phase 2, despite the fact that most of them already owned one. As shown in Section 2, two possible mechanisms may have generated this positive effect on willingness to pay for an LLIN: (1) an experience effect (the subsidy enables households to learn about the net benefits of the technology); and (2) an income effect via a health effect. We find some suggestive evidence, presented in Appendix Table A2, that the incidence of malaria among household heads (either the male or the female) was lower among households who received a high LLIN subsidy 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), this health effect among household heads could potentially have generated an income effect. In this section, we try to estimate how big a role the income effect had in the increased willingness to pay for LLINs among high-subsidy recipients.

We do not have data on income itself (precise income data is typically difficult to measure among the self-employed, who make the great majority of our sample). Instead, in order to test for the presence of an income effect, 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 reached 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.[19]

If the experience effect is the main channel behind the positive effect on willingness to pay for the second LLIN observed in Table 2, 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 and 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 3 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 2. The take-up of the WaterGuard voucher was 40% on average, and therefore a 6 percentage points increase corresponds to just a 15% increase, 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 income effects played at best a limited role in the positive impact of LLIN subsidies on willingness to pay for LLINs observed in section 3.3.

5 Results: Social Learning

Given the large differences in LLIN take-up across price groups, the random assignment of households to price groups in Phase 1 generates an exogenous source of geographic variation in the density of households that had a chance to experiment with an LLIN. As shown in Figure 2, households randomly assigned to a low price (high subsidy) were much more likely to buy

[19]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.

an LLIN in Phase 1 than households assigned a high price. The time needed for households to acquire the LLIN was also much lower when the subsidy was higher. Appendix Figure 1 shows that households that received a voucher for a free LLIN typically redeemed it within a few days. In contrast, those who were assigned a high price were very unlikely to redeem their voucher, and if they did, they took two months to redeem it. All in all, across neighborhoods within a given village, the "exposure" to LLINs varied with the share of households that received a high subsidy level. Since this share was exogenously determined by the random assignment, we can exploit this variation to estimate social effects without running into the reflection problem identified by Manski (1993).

Using GIS coordinates, we compute, for each household in the sample, the number of sampled households that live within a given radius, and the number and share of them who received a voucher for a given subsidy level. In particular, for households who faced a positive price, we compute 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 was 40Ksh; etc.). We use three different radii to define social networks or neighborhoods: 250 meters, 500 meters, and 750 meters. Appendix Table A3 presents summary statistics on these density 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.[20]

Figure 6 plots the coefficients of OLS regressions, where the dependent variable is whether or not a given household purchased the LLIN in Phase 1 and the independent variable is the share (panel A) or the number (panel B) of study households within a 500m radius of the given

[20] Panel B of Table A3 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 A3, Panel B, columns 3-6).

household who received the maximum subsidy offered in the area in Phase 1. Both specifications show take-up of the Phase 1 LLIN 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 4 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 positively priced voucher).[21] For each radius, we run the following specification:

$$Y_{hj1} = \beta ShareMax_{j1} + \delta_1 P_{hj1} + \delta_2 {P_{hj1}}^2 + X_h^{'}\gamma + \upsilon_j + TotalHH_j + \varepsilon_{hj}$$

The regressor of interest is $ShareMax_{j1}$, the share of neighbors (within a given radius) who received the maximum subsidy offered in area j in Phase 1. The total number of study households within 500 meters ($TotalHH_j$) 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. Since the density measures may be spatially correlated, we present standard errors corrected for spatial dependence in brackets, in addition to presenting the White standard errors in parentheses. We use the spacial dependence correction proposed by Conley (1999).

The results in Table 4 are quantitatively unchanged across all three radius choices and across the two standard error formulas. 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 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 4, the independent variable is the share of sampled households within a given radius who are using the LLIN. To overcome the obvious endogeneity

[21] Since 97.5% of households who received a free voucher redeemed it, and did so within a few days, adding households who received a free voucher in this analysis doesn't add information.

issue, we instrument the share using an LLIN with the share of sampled households within that radius who received the maximum subsidy level. In other words, we run:

$$Y_{hj1} = \gamma \widehat{ShareU}_j + X_h^{'}\gamma + \upsilon_j + TotalHH_{,j} + \varepsilon_{hj}$$

where $ShareU_j$, the share of households within a given radius who are using an LLIN, is instrumented by $ShareMax_{j1}$. The estimates of γ are positive and significant in all specifications, which confirms that households learn through their neighbors' experimentation with the product.

In Appendix Table A4, we report results from two alternate specifications. First, we include the full distribution of prices around household i, rather than just the share with the maximum subsidy level. The results are unchanged in substance. Second, we look at levels, rather than densities: the regressor of interest is the total number of households within the radius who have received the high subsidy, instead of the share. The results are somewhat weaker, but the overall pattern is consistent with social learning.

Finally, we look at how take-up of the second LLIN (redemption of the Phase 2 voucher) was affected by exposure via neighbors in Phase 1. The results are presented graphically in Figure 6 (dashed line) and suggests that redemption in Phase 2 was not affected by exposure via neighbors, except at very high levels of exposure, where exposure seems to have a negative effect (though an insignificant one). This is likely due to a simple budget constraint effect: households who were encouraged to buy an LLIN in Phase 1 by their neighbors had less cash on hand to acquire a second LLIN in Phase 2. Overall, these results suggest that exposure through neighbors increased the likelihood that households bought at least one LLIN, but had no impact on the likelihood that households bought both LLINs.

Social learning or mimicry? The social diffusion effects we observe occured within three months (the timeframe households had to redeem their first voucher). Do these effects correspond to social learning or to pure mimicry? In other words, what could households have learned from their neighbors within that timeframe? Did they learn about the attributes of the product or did they feel that owning the product was important for social status?

Given the product studied, a pure mimicry effect is unlikely. Bednet ownership and usage is not publicly observable. Even if neighbors visit each other's house, they do not see the sleeping area, which is typically separated from the "living room" by a wall or a curtain, or in a separate structure. For this reason, a household can easily pretend to own an LLIN, a claim that the neighbors cannot easily verify. In this context, it is unlikely that LLIN ownership could have been taken as or become a strong indicator of social status.

A more reasonnable explanation for the diffusion effects we observe is that households *learned* about the product's qualities by talking with their neighbors. But did they learn about the health or the non-health attributes, or both? That is, did they learn about the high health effectiveness or the low usage cost or both? We cannot perfectly answer this question. While households that acquired an LLIN reported fewer malaria episodes during the 1-year follow-up survey (see Appendix Table A2), we do not have any data to check whether the early redeemers had had time to observe a decrease in malaria incidence within the first three months, and whether they shared that information with their neighbors before the neighbors' vouchers expired. However, qualitative data collected during the 1-year follow-up survey suggests that discussions among neighbors about the LLIN involved both its health and non-health attributes. About 25% of households who reported hearing about the Olyset LLIN from their neighbors said the neighbors mentioned the LLIN was effective against mosquitoes / malaria; 46% reported hearing about how comfortable and strong the LLIN was; and 29% reported their neighbors said the net was "good" or "better than other nets" but did not give more details as to what aspects their neighbors said. Overall, while we cannot rule mimicry as a possible explanation for the social effects we observe, social learning appears the most likely factor.

6 Conclusion

It is often argued that 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 arguments: (1) subsidies may not foster learning about the technology if subsidy recipients do not use the technology (in fact, it might even hinder learning if subsidy recipients *misuse* the technology); and (2) previously encountered prices may act as "anchors" that affect people's valuation of a product independently of its intrinsic qualities.

This paper used a randomized field experiment to estimate the effect of a one-time, targeted subsidy on the long-run adoption of a new health product with high private returns (the long-lasting antimalarial bednet). We find that 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. We contrast our findings with those of two previous randomized studies that found opposite results for two other health technologies. We argue that all three sets of results are consistent with a simple model of technology adoption in which households are initially uncertain about both the health effectivness of a new technology and its non-monetary usage cost. In such a model, the effect of subsidies on learning and adoption depends on whether households initially overestimate or underestimate the usage cost, and how high the private returns to adoption are.

In our experiment, the technology we introduced was relatively comparable to the status quo technology, therefore households in the sample are likely to have used their beliefs about the usage cost and effectiveness of the status quo technology as priors for the new technology. Because the new technology had both lower usage costs and higher private returns than the status quo, high subsidies helped recipients learn that the true usage cost was lower than expected, thereby increasing immediate adoption and enabling faster learning about the effectiveness of the new technology. In contrast, for a technology or product that would have had no comparable status quo technology, as that studied by Ashraf, Berry and Shapiro (*forthcoming*), individuals might have vastly underestimated both the usage cost and effectiveness. In such a context, subsidies can have only a limited impact on immediate adoption, and thus on learning and long-run adoption. For a technology that has only low private returns but high social returns, as is the case in the Kremer and Miguel (2007) experiment, short-run subsidies enable households to learn they should not privately invest in the product. In such a context, subsidies will dampen long-run adoption.

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 some class of preventative health products, learning by doing and social learning are important channels through which short-term, targeted subsidies can translate into sustained levels of adoption. This provides a rationale for subsidies even for technologies that do not generate positive externalities.

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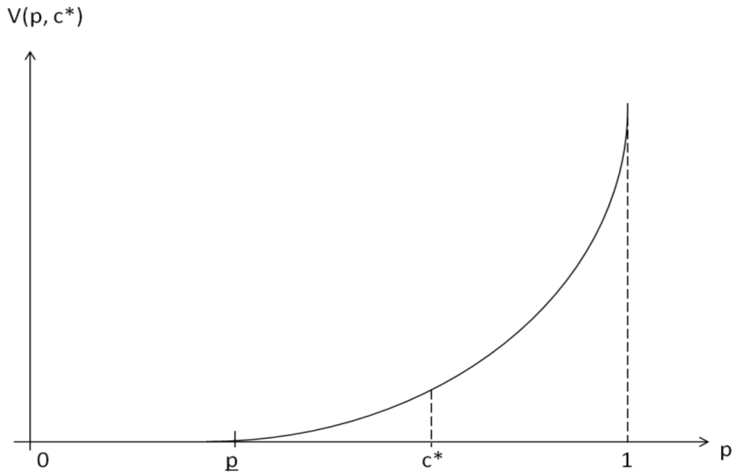
Figure 1.

1a. Value Function V(p, c*)



1b. Value Function V(p_t, c)

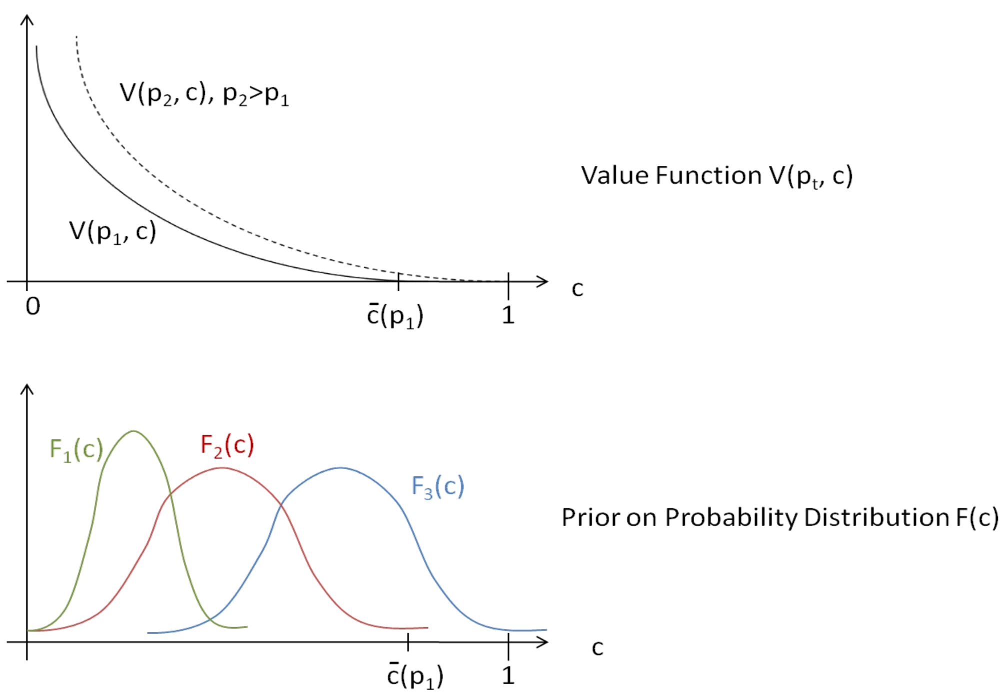
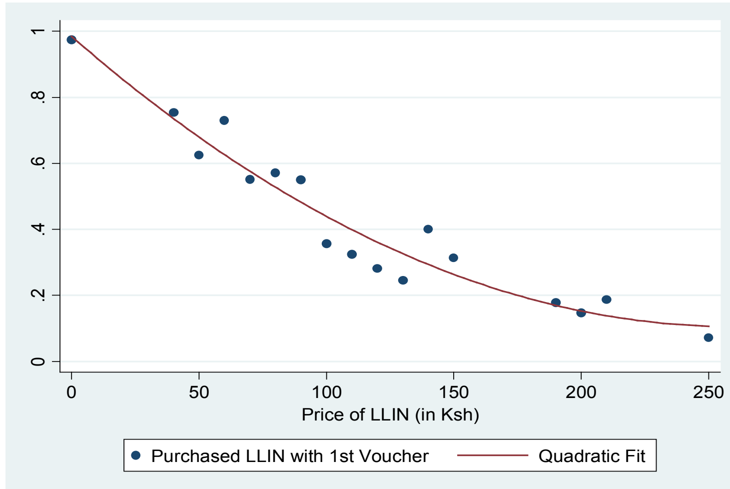
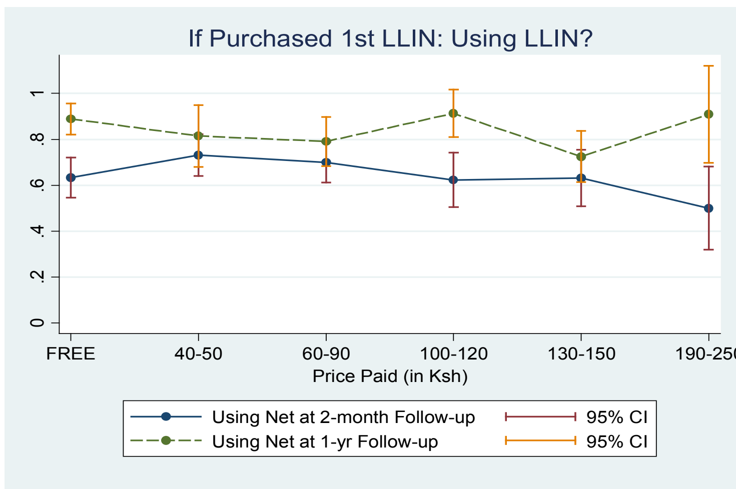


Figure 2

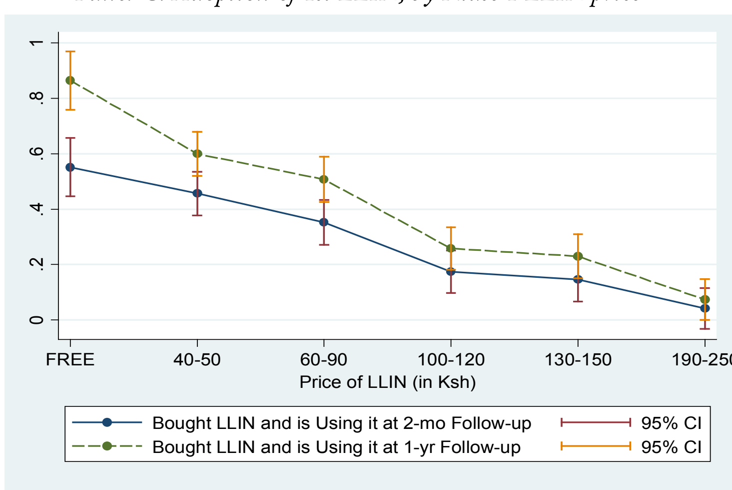
Panel A: Share of study households that purchased the LLIN in Phase 1



Panel B: Among households that bought LLIN in Phase 1, share using the net at follow-up



Panel C: Adoption of 1st LLIN, by Phase 1 LLIN price



Notes: Data from 1,120 households (Panels A and C), 479 households (Panel B, solid line), 273 households (Panel B, dashed line). The second follow-up was conducted in only 4 of the 6 study areas. Usage is self-reported (see Table 2, cols. 7-9, for results on observed usage.) The exchange rate at the time of the study was around 65 Ksh to US$ 1 .
The number of sampled households in each price group is as follows. FREE: 117 obs; 40-50 Ksh: 173 obs; 60-90 Ksh: 196 obs; 100-120 Ksh: 215 obs; 130-150 Ksh: 199 obs; 190-250 Ksh: 220 obs.

Figure 3
Ex-Ante and Ex-Post Declared Willingness To Pay (in Ksh) for LLIN, by Phase 1 price groups
(subsample of households who redeemed 1st LLIN voucher)

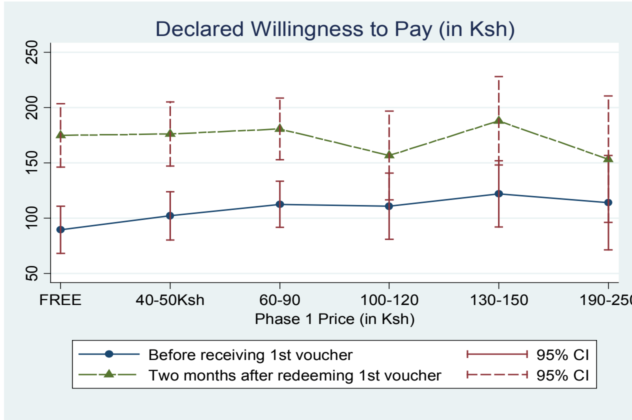


Figure 4
Redemption of 2nd LLIN Voucher (uniformly priced at 150Ksh), by 1st LLIN voucher price group

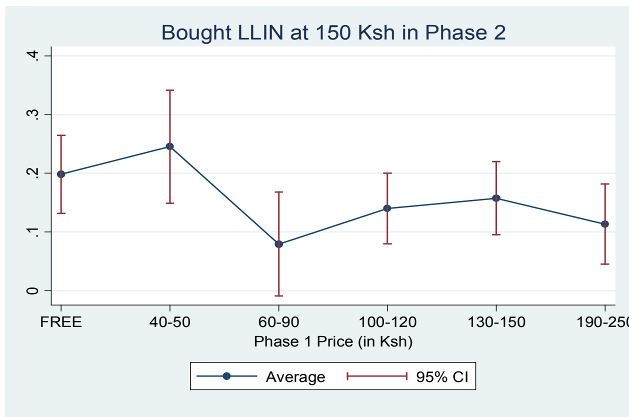
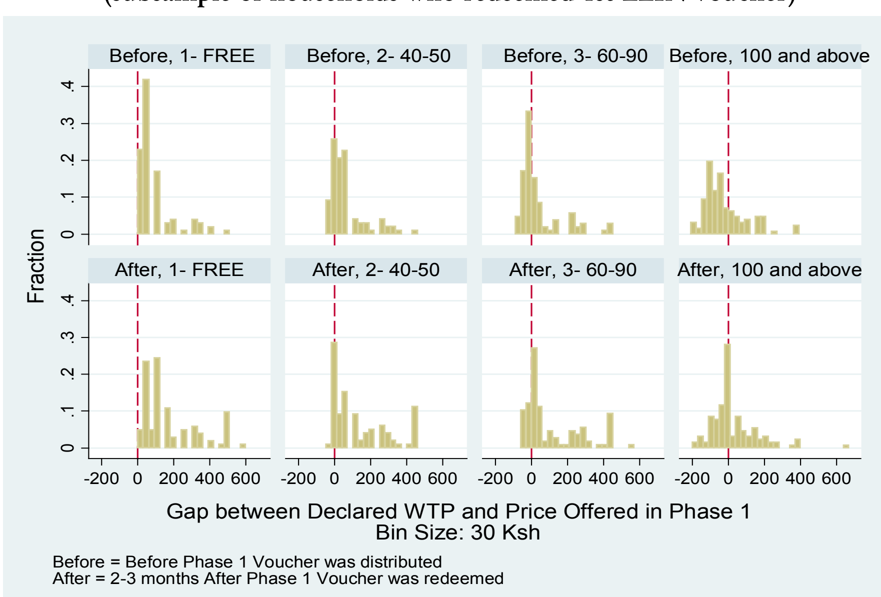


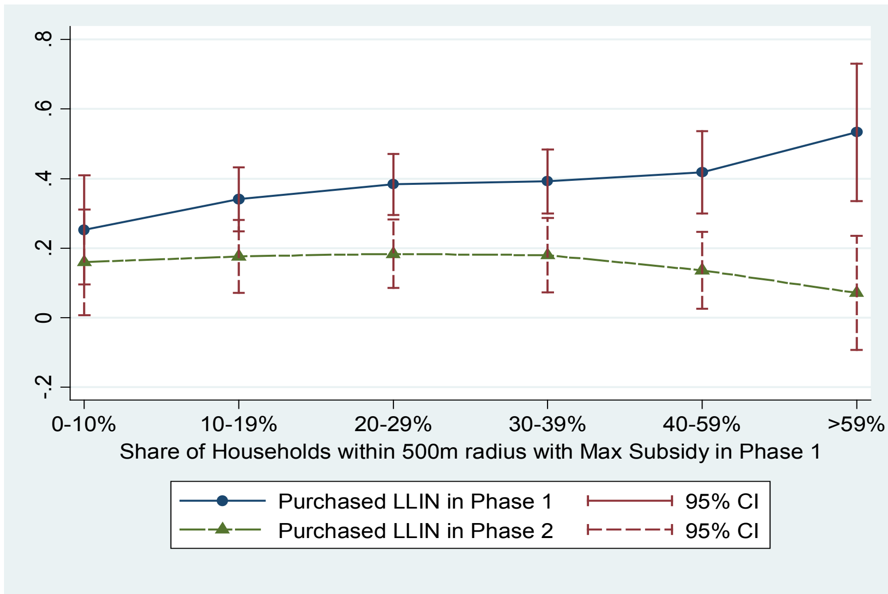
Figure 5
Anchoring around Phase 1 Price ? Gap between Declared WTP and Price Paid
(subsample of households who redeemed 1st LLIN voucher)



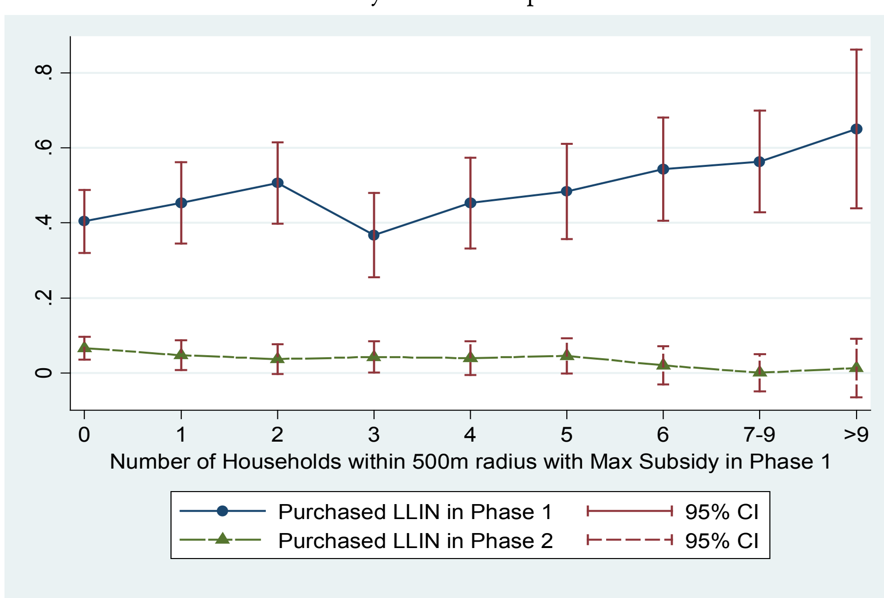
Notes: Figures 2 and 4 include data from 429 households. Figure 3 includes data from 599 households. Note that the two samples are not comparable across figures: Figures 2 and 4 include only those who redeemed their 1st voucher, across all 6 study areas. Figure 3 includes all households (whether or not 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 acquired the first LLIN are included.

Figure 6
LLIN Purchases Among Households facing a Positive Price in Phase 1, by Level of Exposure

Panel A: By Density of Exposure



Panel B: By Absolute Exposure



Notes: Sample restricted to the 985 households that received a positively priced LLIN voucher in Phase 1, and for whom GIS coordinates could be collected. Each graph plots the coefficients and confidence intervals obtained through OLS regressions. The dependent variable is a dummy equal to 1 if the household purchased at least one LLIN (solid line) or two LLINs (dashed line). 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. *Baseline Characteristics of Participating Households*

	(1)	(2)	(3)	(4)	(5)
Household (HH) demographics	Sample Mean	Sample Std. Dev.	*OLS Coeff on* 1st LLIN Price (in US$)	*OLS Coeff on* (1st LLIN Price in US$) squared	*P-value Joint Test* (Price and Price Squared)
Household size	7.11	2.749	-0.150	0.030	0.843
			(.282)	(.071)	
Age of Household Head	45.715	13.155	-1.232	0.032	0.032
			(1.326)	(.331)	
Number of children (under 18) currently living in household	5.447	2.852	-0.129	0.016	0.745
			(.291)	(.073)	
Socio-Economic Status					
Female head has completed primary school	0.248	0.432	0.030	-0.006	0.776
			(.044)	(.011)	
Number of household members with an income-generating activity	1.762	1.036	0.063	-0.034	0.063
			(.107)	(.027)	
Household assets index value (in US $)	338.227	324.965	25.991	-5.142	0.682
			(33.069)	(8.265)	
Electricity at home	0.019	0.136	0.010	-0.003	0.671
			(.014)	(.004)	
At least one member of HH has a bank account	0.12	0.325	0.000	0.003	0.603
			(.033)	(.008)	
Bednet Ownership at Baseline					
Number of bednets owned	1.738	1.51	-0.130	0.038	0.575
			(.154)	(.038)	
Share of HH members that slept under a net the previous night	0.408	0.368	-0.023	0.009	0.469
			(.038)	(.009)	
HH owns a circular PermaNet LLIN*	0.327	0.47	-0.036	0.016	0.733
			(.068)	(.023)	
HH ever received a free bednet	0.323	0.468	-0.026	0.003	0.681
			(.048)	(.012)	
Has ever shopped at shop where voucher has to be redeemed	0.623	0.485	0.052	-0.014	0.437
			(.045)	(.011)	
Declared willingness to pay for a bed net (in US$)	1.561	1.533	0.172	-0.027	0.300
			(.158)	(.039)	
Distance from shop where voucher has to be redeemed (in km)	1.832	1.659	0.051	-0.013	0.952
			(.164)	(.041)	
Number of households	1120				

Notes: Columns 3 and 4 show coefficient estimates and their standard errors for two independent variables (the 1st LLIN price, column 3, and its square, column 4) estimated through linear regressions with area fixed-effects. Standard errors are presented in parentheses.
** The LLINs subsidized during the experiment were family-size rectangular Olysets.*

Table 2. *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)
	Bought 2nd LLIN						First-Stage (IV): Experimented with 1st LLIN			Second-Stage (IV): Bought 2nd LLIN		
1st LLIN Price in US$	-0.029			-0.043			-0.362					
	(0.042)			(0.042)			(0.049)***					
(1st LLIN Price in US$) squared	0.003			0.006			0.051					
	(0.010)			(0.011)			(0.012)***					
1st LLIN Price = 0 (Free)		0.048			0.061			0.432				
		(0.046)			(0.046)			(0.055)***				
1st LLIN Price ≤ 50 Ksh (High Subsidy)			0.065			0.076			0.361			
			(0.039)*			(0.039)*			(0.047)***			
Experimented with 1st LLIN (instrumented with polynomial in price)										0.123		
										(0.085)		
Experimented with 1st LLIN (instrumented with "free" dummy)											0.141	
											(0.107)	
Experimented with 1st LLIN (instrumented with "High Subsidy" dummy)												0.212
												(0.111)*
Household level controls included				Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	599	599	599	590	590	590	590	590	590	590	590	590
Mean of Dep. Variable in non-free group	0.15	0.15		0.15	0.15		0.27	0.27		0.15	0.15	
Mean of Dep. Variable in non-"High Subsidy" group			0.14			0.14			0.26			0.14
F-Stat First Stage							51.5	61.3	59.0			

*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. Household level controls in columns 3-11 include all 15 variables presented in Table 1. Standard errors in parentheses. *Significant at 10%; ** significant at 5%; *** significant at 1%.*

Table 3. *Effect of 1st LLIN voucher price on take-up of other product*

	(1)	(2)	(3)	(4)	(5)	(6)
	Bought WaterGuard		First-Stage for IV: Experimented with 1st LLIN		Second-Stage (IV): Bought WaterGuard	
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. Standard errors in parentheses. *Significant at 10%; ** significant at 5%; *** significant at 1%.*

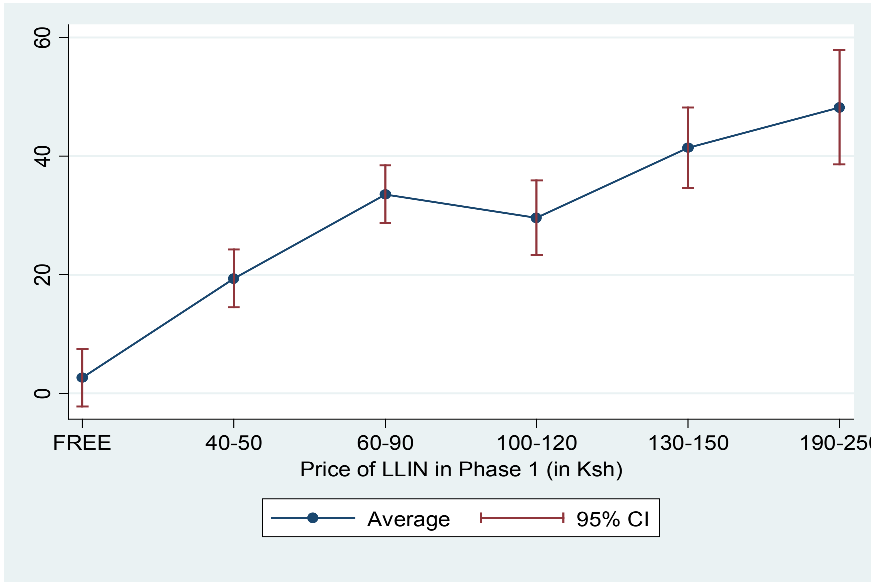
Table 4. *Diffusion Effects through networks*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Household Bought 1st LLIN											
Within 250m radius of household...												
Share of study households with max subsidy	0.114	0.107										
	(0.069)*	(0.069)										
	[0.073]	[0.074]										
Share of study households using LLIN (instrumented with *Share with max subsidy*)			0.250	0.237								
			(0.151)*	(0.153)								
Within 500m radius of household...												
Share of study households with max subsidy					0.179	0.215						
					(0.104)*	(0.107)**						
					[0.105]*	[0.107]**						
Share of study households using LLIN (instrumented with *Share with max subsidy*)							0.347	0.436				
							(0.202)*	(0.219)**				
Within 750m radius of household...												
Share of study households with max subsidy									0.216	0.274		
									(0.129)*	(0.135)**		
									[0.129]*	[0.136]**		
Share of study households using LLIN (instrumented with *Share with max subsidy*)											0.528	0.755
											(0.321)*	(0.381)**
Total # of study households within 500m radius (/10)	0.020	0.027	0.015	0.023	0.019	0.027	0.016	0.025	0.019	0.027	0.017	0.028
	(0.011)*	(0.012)**	(0.012)	(0.013)*	(0.011)*	(0.012)**	(0.012)	(0.012)**	(0.011)*	(0.012)**	(0.012)	(0.012)**
	[0.011]*	[0.012]**			[0.011]*	[0.012]**			[0.012]	[0.0125]*		
2rd 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	985	978	985	978	985	978	985	978	985	978	985	978
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. All regressions also include a quadratic in own price. Household level controls include all 15 variables presented in Table 1.

*White standard errors presented in parentheses. Standard errors corrected for spatial dependence are presented in brackets. *Significant at 10%; ** significant at 5%; *** significant at 1%.*

Figure A1
Number of Days needed to Redeem 1st LLIN Voucher, by 1st LLIN voucher price group



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

Table A1. *Attrition*

	(1)	(2)	(3)	(4)
	Bought 1st LLIN but Missing in 1st Follow-Up		Attrited before distribution of 2nd LLIN voucher	
1st LLIN Price in US$	-0.017		0.035	
	(0.043)		(0.028)	
(1st LLIN Price in US$) squared	0.007		-0.010	
	(0.013)		(0.007)	
1st LLIN Price = 0 (Free)		-0.012		-0.012
		(0.039)		(0.030)
Household level controls included				
Observations	492	492	642	642
Mean of Dep. Variable in non-free group	0.09	0.09	0.07	0.07
Estimated effect of a price increase:				
from $0 to $1	-0.010		0.025	
p-value	0.750		0.239	
from $1 to $2	0.005		0.006	
p-value	0.780		0.621	

Notes: Coefficient estimates obtained using linear regression with area fixed effects. Price varies from 0 to US$3.8. Baseline characteristics are missing for a few households. The sample in columns 1 and 2 is restricted to those who redeemed their 1st voucher. The sample in columns 3 and 4 is restricted to households in the 4 study areas where the 2nd voucher was distributed.
Significant at 10%; ** significant at 5%; * significant at 1%.*

Table A2. *Health Effect*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Had malaria in the month preceding the 1-yr Follow-up Survey							
	OLS	OLS	OLS	OLS	IV	IV	IV	IV
1st LLIN Price = 0 (Free)	-0.029		-0.035					
	(0.026)		(0.027)					
1st LLIN Price ≤ 50 Ksh (High Subsidy)		-0.020		-0.030				
		(0.023)		(0.022)				
Experimented with 1st LLIN (instrumented with "free" dummy)					-0.065		-0.088	
					(0.077)		(0.068)	
Experimented with 1st LLIN (instrumented with "High Subsidy" dummy)						-0.076		-0.095
						(0.068)		(0.070)
Household level controls included			Yes	Yes			Yes	Yes
Observations	961	961	946	946	961	961	946	946
Mean of Dep. Variable in non-free group	0.093							
Mean of Dep. Variable in non-"High Subsidy" group		0.100						

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 and gender fixed effects. Sample includes up to two observations per household (male and female head). Standard errors are clustered at the household level. Price varies from 0 to US$3.8. Household level controls in columns 3,4,7 and 8 include all 15 variables presented in Table 1.
Significant at 10%; ** significant at 5%; * significant at 1%.*

Table A3. *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 positively priced voucher.
Significant at 10%; ** significant at 5%; * significant at 1%.*

Table A4. *Diffusion Effects: Experimental Evidence with Alternative Specifications*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Bought 1st LLIN								
Within 250m radius									
Share of study households with max subsidy	0.138								
	(0.073)*								
Share of study households with price 60-90	0.096								
	(0.084)								
Share of study households with price 100-120	0.102								
	(0.077)								
Share of study households with price 130-150	0.025								
	(0.083)								
# of study households with max subsidy		0.000	0.003						
		(0.013)	(0.014)						
# of study households with price 60-90			0.001						
			(0.014)						
# of study households with price 100-120			-0.010						
			(0.012)						
# of study households with price 130-150			-0.004						
			(0.015)						
Within 500m radius									
Share of study households with max subsidy				0.216					
				(0.114)*					
Share of study households with price 60-90				0.009					
				(0.115)					
Share of study households with price 100-120				-0.049					
				(0.115)					
Share of study households with price 130-150				0.063					
				(0.136)					
# of study households with max subsidy					0.010	0.010			
					(0.009)	(0.009)			
# of study households with price 60-90						-0.006			
						(0.009)			
# of study households with price 100-120						-0.003			
						(0.012)			
# of study households with price 130-150						0.006			
						(0.010)			
Within 750m radius									
Share of study households with max subsidy							0.249		
							(0.144)*		
Share of study households with price 60-90							-0.051		
							(0.147)		
Share of study households with price 100-120							-0.030		
							(0.143)		
Share of study households with price 130-150							-0.096		
							(0.172)		
# of study households with max subsidy								0.009	0.011
								(0.005)*	(0.005)**
# of study households with price 60-90									-0.006
									(0.005)
# of study households with price 100-120									0.000
									(0.006)
# of study households with price 130-150									0.000
									(0.005)
Total # of study households within 500m radius	0.022	0.028	0.036	0.027	0.008	0.020	0.027	0.002	0.013
(/10)	(0.013)*	(0.015)*	(0.020)*	(0.012)**	(0.022)	(0.057)	(0.012)**	(0.018)	(0.026)
Observations	978	978	978	978	978	978	978	978	978
Mean of Dep. Variable	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. All regressions also include a quadratic in own price and all the household level variables presented in Table 1.
Significant at 10%; ** significant at 5%; * significant at 1%.*