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

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Abstract

Short-run, targeted subsidies for health products are common in poor countries. How do they affect long-run adoption? Standard economic theory predicts that they may increase long-run adoption through experience and social learning effects. Those effects will be muted, however, if subsidized products are unused or misused. Subsidies have also been argued to generate "entitlement effects": people may refuse to pay for products that were once free. A field experiment was designed to estimate the relative importance of these competing effects. We find that, for a health product with high private returns (an antimalarial bednet), positive experience and social learning effects largely dominate.

JEL codes: C93, D12, H23, H42. Keywords: technology adoption; subsidies; social learning; anchoring.

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1 Introduction

Between nine and ten million children under five die every year in the world.[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] A major outstanding question is how to increase availability and 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, most of these products generate positive health externalities, and private investment in them will 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. Short-run subsidies are well-suited for such technologies. But for other products, such as anti-malarial bednets, water treatment kits, or condoms, sustained adoption is required to generate the hoped-for health impacts. A key question is whether one-time subsidies for such technologies increase or dampen private investments in them in the long-run.

The standard neoclassical model of consumer behavior predicts that free or highly subsidized distribution of a product in the short-run 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. 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.

However, these positive learning effects require that people make use of a product or technology that they receive for free or at a highly subsidized price. This might not be the case. Households who 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. Ashraf, Berry and Shapiro (*forthcoming*) find evidence of such negative selection effects of subsidies for a water-chlorination product in urban Zambia.

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), who show that otherwise comparable households spend less money in rent in a city they have just moved to if the city they came from has cheaper housing. Under such reference-dependent preferences, 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. For example, Kremer and Miguel (2007) find that parents in Kenya who were exposed to free deworming treatment for their children for a year were extremely unwilling to pay for deworming once it stopped being free. Reference-dependent preferences might also generate a "relative price effect" for those not targeted by the subsidy, if their reservation price is affected by the subsidized price they see being offered to others.

The view that these negative effects might dominate, making short-run subsidies detrimental to long-run adoption, is not uncommon among development practitioners. As the Boston Globe put it: *"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]

In this paper, we report on a field experiment designed to test the relative importance of these potential and competing effects of subsidies in the adoption and diffusion of a

[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/.

new health technology. The technology considered is the Olyset long-lasting insecticide-treated bed net (LLIN), a recent innovation in malaria control. The experiment involved 1,120 households in Kenya and included two phases. In Phase 1, subsidy levels for LLINs were randomly assigned across households within six villages, with the maximum subsidy level randomized across villages. Households had three months to acquire the LLIN at the subsidized price they had been assigned to. Prices varied from $0 to $3.80. (For comparison, the average daily wage for casual agricultural work in the study area is estimated at $1.85). After a few months, a subset of households were offered the opportunity to acquire another health product (a water treatment product) at a uniform positive price. In Phase 2, a year later, all households in four villages were given a second opportunity to acquire an LLIN, but this time everyone faced the same price ($2.30). Phase 2 was unannounced, therefore at the time individuals made their purchasing decision in Phase 1, they were not aware that they would receive a second chance to acquire the product a year later. The LLIN was not available outside of the experiment, but other types of nets were available on the market at the retail price of $1.50. This experimental design allows us to test multiple hypotheses on the effects of temporary subsidies on demand, both over time and across individuals.

First, we can study the importance of the entitlement effect by examining whether having been exposed to a full or very large subsidy for an LLIN in Phase 1 reduces willingness to pay for an LLIN in Phase 2, a year later. We find no support for the hypothesis that entitlement effects can offset the standard positive effects. Instead, we find suggestive evidence that gaining access to a free or highly subsidized LLIN in the first year increases households' reported as well as observed willingness to pay for an LLIN a year later. In the context of subsidies for bednets, experience and income effects thus seem to dominate entitlement effects.

Second, we can test for the presence of cross-product entitlement effects, namely, whether being exposed to a full or very large subsidy for an LLIN generates an expectation that other health products should also be heavily subsidized. We find that households who received a free LLIN in Phase 1 were, if anything, slightly more likely than other households to invest in the water-treatment product offered at the end of Phase 1. This suggests that cross-entitlement effects are outweighed by income and possibly complementarity effects.

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

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

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

More broadly, this paper contributes to two literatures. First, the paper contributes to a now large literature on the role of learning-by-doing and social learning in technology adoption in poor countries. The evidence so far, mostly non-experimental and mostly focused on agricultural technologies, is rather mixed and suggests that the role of social learning is likely to vary greatly with the context and the product considered.[4]

The second literature this paper contributes to is the empirical reference-dependence literature estimating how the willingness to pay for a product can be affected by anchors (Ariely, Loewenstein and Prelec, 2003), previously encountered prices (Simonsohn and Loewenstein, 2006; Mazar, Koszegi and Ariely, 2009), or the range of options that are offered to them (McFadden, 1999; Heffetz and Shayo, 2009).

Our experiment brings together these two existing strands of literature and provides evidence that, for essential preventative health products in a poor country, the total effect of short-run subsidies on long-run adoption is positive. Previously encountered prices matter, but more so through their effect on available knowledge about the product than through contrast or entitlement effects.

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

[4] 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 network. Oster and Thornton (2008) find evidence of peer effects in the usage of a new female hygiene product provided for free.

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 consider a technology for which health-effective adoption requires not only acquiring the technology each period, but also actively using it throughout the period. In addition to anti-malarial bednets, examples of such technologies include water chlorination, water filter, iron pills, condoms, among others. At the time people decide whether to acquire the technology, they are uncertain about both the effectiveness of the technology and the cost associated with using the technology. They choose to acquire it if the expected private benefits exceed the total expected private costs (monetary cost + usage cost). As soon as they acquire the technology, people learn its true usage cost (say, by using it for a day). They then decide to use it consistently if the expected private benefits exceed the observed usage cost.

People have two sources of information: their own experimentation with the technology, and the experience of their neighbors. Learning about the usage cost is immediate upon ownership of the product, but learning about the effectiveness takes time.

2.1 Assumptions

2.1.1 Preferences

Utility of individual i is composed of two additive terms: intrinsic utility and gain-loss utility. Intrinsic utility is a function of absolute outcomes, expected private benefits and expected private costs. Gain-loss utility captures reference-dependence.

As in Kremer and Miguel (2007), suppose that the total private benefit to using the health technology depends on the individual's health level γ, the effectivess of the technology ϕ, and an indiosyncratic individual specific taste for health μ_i. Individual health level at period t, γ_{it}, may depend on individual characteristics X_i, and on the overall rate of adoption of the health technology among neighbors at time t (e.g., malaria is transmitted from person to person, so one is less likely to get malaria if one's neighbors are malaria-free because they have adopted an antimalarial technology).

There are two types of costs associated with the technology: the financial cost of acquiring the product (price p); and the time or utility costs of using the product, denoted by c.

Following Kőszegi and Rabin (2006), we formalize reference-dependence as follows. Denote $\hat{p}_{it}$ individual's i reference price for the technology at time t. Paying a price p_{it} for the technology generate gain-loss utility $r(\hat{p}_{it} - p_{it})$. To allow for loss aversion, we allow r to be kinked at zero. The simplest way to formalize it is to consider that r is two-piece linear, and has a slope $\lambda_G \geq 0$ for gains (when $\hat{p}_{it} \geq pi_t$), and a slope of $\lambda_L \geq \lambda_G$ for losses (when $\hat{p}_{it} < p_{it}$).

Let $\hat{\phi}_{it}$ and $\hat{c}_{it}$ denote the individual's beliefs in period t about the technology's effectiveness ϕ and the usage cost c, respectively, conditional on prior beliefs and any signals received. Let $T_{it} \in \{0, 1\}$ be an indicator variable for usage of the technology in period t. Then the individuals' expected private benefit from adoption can be expressed as:

$$E[U(T_{it} = 1) - U(T_{it} = 0)] = \hat{\phi}_{it} h(\gamma_{it})\mu_i - p_{it} - \hat{c}_{it} + r(\hat{p}_{it} - p_{it}) \tag{1}$$

where U is individual utility, conditional on the treatment choices of other individuals.[5] Finally, let $n_{it} \in \{0, 1\}$ be an indicator variable for purchase of the technology in period t.

2.1.2 Information Sets

When the new technology is introduced, all individuals have a common prior belief about its effectiveness, denoted $\hat{\phi}_{i0} \sim N(\phi_0, \sigma_0^2)$, $\forall i$. The mean ϕ_0 may be greater or equal than the actual effectiveness ϕ. In particular, ϕ_0 could be less than ϕ if the status quo technology (e.g., earlier types of moquito nets) is less effective than the new technology.[6]

[5] We assume that individuals do not consider the additional motive of adopting in order to learn more about the costs or the effectiveness of the technology. Such a motive would make the problem intractable.

[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, this would reinforce the standard learning effects of subsidies.

Likewise, all individuals have a common prior belief about the cost of using the new technology, denoted $\hat{c}_{i0} \sim N(c_0, \sigma_0^2)$, $\forall i$. Here again, c_0 may be greater or equal than the actual cost c, and is likely to depend on the cost of using the status quo technology.

To update beliefs, individuals use both their own experience and that of J peers. All individuals who *buy* the technology obtain a perfect signal about its usage cost c. All individuals who *use* the technology obtain a signal about effectiveness. These signals are noisy due to unobservable individual time-specific shocks to health status. Let these signals have mean ϕ and variance σ_ϵ^2.

We consider that each individual i has a location on a two-dimensional map, and that J signals are drawn from the set of individuals within a distance d in each direction. If a fraction w_{it} of individuals in this radius adopted (i.e., bought *and* used) the product in period t, individual i receives $w_{it}J$ signals from social contacts, and $T_{it} \in \{0, 1\}$ personal signal. Bayesian individual i who receives $\bar{T}_{it} = w_{it}J + T_{it}$ signals will weight her prior beliefs and signals received such that the posterior belief on expected effectiveness becomes:

$$\hat{\phi}_{it} = \left[\left\{ \frac{\sigma_T^2}{\sigma_T^2 + \sigma_0^2} \right\} \cdot \phi_{i0} + (1 - \left\{ \frac{\sigma_T^2}{\sigma_T^2 + \sigma_0^2} \right\}) \cdot \phi_s \right] \tag{2}$$

where $\hat{\phi}_{i0}$ is the mean of the prior distribution, ϕ_s is the sample average of signals received through the social network, and $\sigma_T^2 \equiv \sigma_\epsilon^2 / \bar{T}_{it}$ denotes the variance of the sample average. The more neighbors use the technology and generate a signal, the lower the variance of the sample average and the value of both the sample average and posterior beliefs approach the true expected effectiveness, ϕ.

Individuals can also learn about the true cost of using the technology from their neighbors, if they have not learned it by acquiring the product themselves. We consider that for individual i who does not know the true cost of using the product at time $t-1$, the belief about the cost of usage at time $t+1$ is updated as follows:

$$\hat{c}_{it} = n_{it}c + (1 - n_{it})[(1 - \zeta)^{\omega_{it}J} \hat{c}_{it-1} + (1 - (1 - \zeta)^{\omega_{it}J})c]$$

where $\omega_{it}$is the share of contacts who acquired the product and ζ is the probability that one of them tells individual i what the true usage cost is.

Finally, we need to consider how the reference price $\hat{p}_{it}$ evolves. We assume that prior to the introduction of the technology, all individuals have a common reference point $\hat{p}_0$, based on the cost of the status quo technology. After the technology is introduced in period k, we suppose that individual i in market m revises her reference price to $\hat{p}_{i,k+t} = \min\{p_{mk}\}, \forall t \geq 1$. In other words, the individual "anchors" around the lowest price observed in the nearby market.

To summarize, the timing of information revelation and decision making is as follows:

Beginning of Period t	**Period t**	**Beginning of Period t+1**
Beliefs φ_{it-1}, c_{it-1}	Observe outcomes	Beliefs φ_{it}, c_{it}
Reference point p_{it-1}	Observe neighbors' outcomes	Reference point p_{it}
Face price p_t	Update: form c_{it}, φ_{it}	Face price p_{t+1}
Choose whether to buy		Choose whether to buy
If bought technology:		
Observe c		
Choose whether to use		
Observe if neighbors buy		

2.2 Analytical results

In this section we establish some static and dynamic predictions of the model that can be tested empirically. We begin by solving for the optimal purchase and usage decisions.

When the technology is introduced at time t, individual i will purchase the technology if

$$E[U(T_{it} = 1) - U(T_{it} = 0)] \geq 0 \Leftrightarrow \hat{\phi}_{it} h(\gamma_{it}) \mu_i - p_{it} - \hat{c}_{it} + r(\hat{p}_t - p_{it}) \geq 0$$

Those who buy the technology will decide to use it if the observed usage cost c satisfies:

$$\hat{\phi}_{it} h(\gamma_{it}) \mu_i - c \geq 0$$

Thus individual i will buy the technology but not use it if and only if:

$$c \geq \hat{\phi}_{it} h(\gamma_{it}) \mu_i \geq \hat{c}_{it} + p_{it} - r(\hat{p}_t - p_{it}) \quad (3)$$

If condition (3) holds, then individual i will own the product and learn the usage cost c, but will not get a personal signal about the effectiveness ϕ.

2.2.1 Comparative Statics: the Role of Prices and Beliefs

Price Our key variable of interest is price. All else equal, a lower price increases the likelihood that people buy the product, and therefore learn the true usage cost. Whether a lower price increases the likelihood of adoption is unclear, however, and depends on the usage cost. If people underestimate the usage cost, condition (3) may hold. The lower the price, the more likely it is to hold: people who: (1) underestimate the cost of usage and (2) expect low benefits, might be induced to buy the technology if the price is low, but they may not use it once they learn the true usage cost. There would then be no learning about the effectiveness of the product for those subsidy recipients.

In the experiment below, we find that adoption decreases monotonically with price (increases monotonically with the subsidy level). We take it as evidence that people tend to overestimate the usage cost of LLINs and therefore high subsidies can help increase immediate adoption and learning.

In contrast, Ashraf, Berry and Shapiro (forthcoming) find that higher subsidies increase the fraction of people who acquire a water chlorination product but do not use it. This suggests that a fraction of people underestimate the cost of using chlorine in their water (for example, they underestimate how bad their water will taste if they use chlorine), and as a result, condition (3) holds for them.

Beliefs Another key variable whose evolution drives changes in adoption is the belief about the effectiveness of the technology. It is straightforward to show that a higher expected effectiveness increases the probability that an individual acquires the technology, and chooses to use it, all else equal.

For technologies that are radically new (e.g., in that they rely on unknown scientific principles), individuals might start with pessimistic beliefs 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 inseciticide 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.

In contrast, in a context like ours below, where the status quo technology is already relatively good and the new technology is similar in spirit 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, as in our application), individuals may have relatively optimistic priors about the effectiveness of the new technology.

Reference point In the presence of a status quo technology, and if $r(\cdot)$ is non-zero, people anchor around the price of the status quo technology. All else equal, this reduces adoption of the new technology if the price of the new technology is above the price of the status quo technology, and increases adoption of the new technology if the price is below the price of the status quo technology.

2.2.2 Dynamic Effects

Over Time For individual i, the impact of having faced price p_{it} in period t on expected private benefits of adoption in period $t+1$ (when the price has become $p_{it+1} > p_{it}$) is the sum of three terms:

$$\begin{aligned} \frac{\partial E[U(T_{it+1}=1)-U(T_{it+1}=0)]}{\partial p_{it}} &= \frac{\partial \hat{\phi}_{it}}{\partial T_{it}} \cdot \frac{\partial T_{it}}{\partial p_{it}} \cdot h(\gamma(w_{it}, X_i) \cdot \mu_i \\ & -\frac{\partial n_{it}}{\partial p_{it}}(c-\hat{c}_{it-1}) \\ & +\lambda_L \frac{\partial \hat{p}_{it}}{\partial p_{it}} \end{aligned} \qquad (4)$$

The first term on the right-hand side represents learning by doing about the technology's effectiveness. It can be positive or negative depending on whether the individual prior overestimate or underestimate the true private benefits of adoption, and zero if the price in period t is such that individual i does not experiment with the product at period t. The second term represents learning about the cost of using the product. Again, it can be positive or negative depending on the difference between the prior and the true usage cost. The third term is the positive anchoring effect.

In the experiment below, we find a negative effect of the period 1 price (the sum of the four effects is negative). We take it as evidence that lower prices (higher subsidies) enable learning, and that the anchoring effect is inexistent or too small to dominate the learning effect.

Across Households The impact of the share of neighbors who adopt the product on the expected private benefits of adoption is the sum of three effects:

$$\begin{aligned}\frac{\partial E[U(T_{it}=1)-U(T_{it}=0)]}{\partial w_{it}} &= \left[\frac{-\sigma_T^2\sigma_0^2}{(\sigma_T^2+\sigma_0^2)^2 w_{it}}\right]\cdot(\phi_{i0}-\phi_s)\cdot h(\gamma(w_{it},X_i)\cdot\mu_i \\ &\quad -(1-n_{it})[\ln(1-\zeta)(1-\zeta)^{w_{it}J}J(c-\hat{c}_{it-1}) \\ &\quad +\phi_{it}\frac{\partial h}{\partial\gamma}\cdot\frac{\partial\gamma}{\partial w_{it}}\end{aligned} \qquad (5)$$

The first term on the right-hand side represents social learning about the technology's effectiveness. It can be positive or negative depending on whether the individual prior underestimate or overestimate the true private benefits of adoption. The second term represents social learning about the cost of using the product. Again, it can be positive or negative depending on the difference between the prior and the true usage cost, and it can be zero if the individual learned the cost of usage by acquiring the product herself. The third term is the health externality, which is zero for non-infectious diseases and negative for infectious diseases, since having more neighbors protected reduced the chances of transmission.

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 dominate the learning effect.

3 Background and Experimental Design

3.1 Background on Malaria and Bednet distribution in Kenya

Over the past two decades, the use of insecticide-treated nets (ITNs) has been established through multiple randomized trials as an effective and cost-effective malaria control strategy for sub-Saharan Africa (Lengeler, 2004). But coverage rates with ITNs remain low. Until recently, one of the key challenges to widespread coverage with ITN was the need for regular re-treatment with insecticide every 6 months, a requirement few households complied with (D'Alessandro, 2001). This problem was solved recently through a scientific breakthrough: long-lasting insecticidal nets (LLINs), whose insecticidal properties last at least as long as the average life of a net (4-5 years), even when 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, subsidized by the social marketing organization PSI and sold through the retail sector–was not well known.[7]

In our study sample, 80% of households owned at least one bednet (of any kind) at baseline, but given the large average household size, the coverage rate at the individual level was still low, with only 41% of household members regularly sleeping under a net (Table 1). About 33% of households had an LLIN of the brand PermaNet® at baseline. The PermaNet LLINs were received free from the government during a mass distribution scheme targeting parents of children under 5 and conducted in conjunction with the Measles campaign of July 2006, ten months before the onset of this study. These PermaNets differ substantially from the Olyset LLIN used in our experiment: 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 nets with the naked eye, while Olyset nets can.

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

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 Kenyan Shillings (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; one area had a minimum price of 40 Ksh; two areas had a minimum price of 50 Ksh; and one area had a minimum price of 70 Ksh.

After the random assignment to subsidy levels had been performed in office, trained enumerators visited each sampled household. A baseline survey was administered to the female and/or male head of each consenting household.[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

[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).

[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 presented with household controls include controls for the randomized gender assignment.

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 the visit. In addition, willingness to pay to replace the study LLIN was assessed by asking households the following question: "If you didn't have this net, up to how much would you be willing to pay to get a net like this, now that you are familiar with it?"

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

[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.

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 budget reasons.

3.4 Verifying Randomization

A baseline survey was administered at households' homes between April and October 2007, prior to the fist voucher distribution. The baseline survey assessed household demographics, socioeconomic status, and bednet ownership and coverage. Table 1 presents summary statistics on 15 household characteristics, 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} is a baseline characteristics 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 succesful at making the price assignment orthogonal to observable baseline characteristics.[14]

3.5 Verifying Compliance

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

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

[14] Alternative specifications (linear price effect, dummy for "Free 1st LLIN") yields similar results (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

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

The effects of subsidies on take-up and usage of the Phase 1 LLIN are presented in Figure 1. Panel A of Figure 1 shows that the take-up of the first voucher is highly sensitive to price: take-up is quasi-universal for free LLIN vouchers (at 97.5%), but drops to 60-70% when the price is between 40 and 90 Ksh (between \$0.6 and \$1.4), and further drops to around 30% when the price crosses the 100 Ksh threshold (\$1.5). In contrast, Panel B of Figure 1, which shows usage rates (among those who redeemed their voucher), suggests that the likelihood that people put the LLIN to use within two months or within a year does not increase with price.[15]

These two main findings—take-up is highly sensitive to price but usage is independent of price—are robust to adding household-level controls (regression analysis available upon request). In the terms of the framework presented in Section 2, these findings suggest there are no households for whom condition (3) holds. To the contrary, households seem to overestimate the usage cost of the LLIN, and this dampens their demand at even modest positive prices. These results thus confirm that the findings obtained among pregnant women by Cohen and Dupas (2010) hold for the general population and over time.

4.2 Long-Run Effects of Direct Exposure

This section tests whether households who benefitted from a free or highly subsidized LLIN in Phase 1 were more or less willing to pay for a LLIN in Phase 2, when the price was high

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

for everyone. We test this using both declared preferences and revealed preferences.

First, we look at how households' declared willingness to pay for a bed net was affected by the subsidy. This is presented in Figures 2 and 3, which are restricted to the sample of households that redeemed their first voucher. Figure 2 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. Figure 2 shows that the willingness to pay increased substantially and significantly for all households, and especially for those households who received large subsidies. While part of this increase could be imputed to a general increase in awareness of malaria issues in Kenya over time, or to an increase in households' wealth level over time, the effect is too large to be explained by a simple time trend, suggesting that the large subsidies might have enabled households to learn the benefits associated with the net.[16]

In Figure 3, we directly test for the presence of "anchoring" by looking at the gap between households' declared willingness to pay (both before and after) and the price paid in Phase 1. We show the distribution of this gap separately for three broad price categories (free, price < 100 Ksh and price ≥ 100 Ksh). The first row shows the distribution of the gap "before" (before households received the first voucher) and the second row shows the distribution of the gap "after" (at follow-up). The evidence in Figure 3 suggets that households who paid a positive price anchored somewhat around the offered price: at follow-up, the distribution of the gap narrows around zero. 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.

Declared willingness to pay might suffer from social desirability bias, however. For this

[16] 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.

reason, it is important to also look at revealed preferences, namely, the take-up of the second LLIN. The price of the second voucher was uniform across all households (at 150 Ksh). Figure 4 presents the average purchase rate for the second LLIN offered, for each Phase 1 price group. The confidence intervals are large, but the average take-up was higher among the higher subsidy groups (free and 40-50 Ksh price groups).

The regression analysis presented in Table 2 confirms this result. Columns 1 through 6 estimate the following reduced form equations:

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

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

The take-up in the '1st LLIN Free' group is 6.2 percentage points (41%) higher than in 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 10-12 of Table 2 estimate the following equation:

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

where U_{hj1} indicates whether household h used an LLIN in Phase 1 (i.e., not only bought the LLIN in Phase 1 but also used it), and is instrumented with either the price faced in Phase 1 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 phased in Phase 1 was 50Ksh or lower (high subsidy, column 12). The three possible first-stage estimations are presented in columns 7-9 of Table 2.

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

Columns 3 and 6 of Table 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. The IV estimates (column 12) is also significant at the 10 percent level, although as discussed above the IV specification is not valid in the presence of entitlement effects.

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

4.3 Experience or Income Effect?

The previous section suggests that households who received a free or highly subsidized LLIN are not less likely to buy a second one after a year. Rather, they appear *more* likely to buy a second net, despite the fact that most of them already own one. In Section 2, the mechanism generating this positive effect on willingness to pay for an LLIN is the experience effect (the subsidy enables households to learn about the benefits of a technology). In the presence of credit-constraints (ignored in the theoretical framework), the positive effect could also come from a wealth effect, which itself can come from a mechanical effect of the subsidy on the intertemporal budget constraint (those who paid less for the LLIN in year 1 have more money available to invest in an LLIN in year 2); and from a positive health effect on disposable income (households that received the subsidy and adopt the product are less likely to suffer from malaria; this increases their productivity and decreases their malaria treatment expenditures and therefore increases their disposable income). Indeed, we find some suggestive evidence, presented in Appendix Table A2, that the incidence of malaria among household heads (either the male or the female) was lower among households who received a cheaper LLIN voucher in Phase 1. This effect is not surprising given the large medical literature showing large private returns to bednet use (Lengeler, 2004). Given the existing evidence of a link from health to productivity at the micro level (Strauss and Thomas, 1998), this health effect among household heads could potentially have generated an income effect.

We do not have data on income itself (precise income data is typically difficult to measure among the self-employed, who make the great majority of our sample). Instead, in order to test for the relative importance of the income / budget constraint effects, we distributed uniformly-priced vouchers for a chlorine-based water-treatment product called WaterGuard® to all study households in the two communities where the LLIN subsidy in Phase 1 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.[17]

If the experience effect is the main channel behind the positive effect of willingness to pay for the second LLIN observed in Table 2, the take-up of the WaterGuard voucher should be

[17] 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.

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 a 15% increase only, in contrast with the 41% increase in take-up observed for the second LLIN among recipients of a free LLIN in Phase 1. The effect on the treated (those who actively used the free LLIN) is greater in magnitude (+15 percentage points, or 37%), but still lower than that observed for the second LLIN (90%).

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

4.4 Cross-Product Entitlement Effects?

Development practitioners often worry that subsidies for one product lead to entitlement effects vis-a-vis other products. In particular, households might expect that the government or NGO that subsidized product A will also soon start to subsidize product B (if product B belongs to the same class of product, say health products), and thus adopt a "wait and see" stance. To test whether this is the case in the Kenyan context, we can exploit the WaterGuard voucher experiment conducted 5 months after the first LLIN vouchers were distributed.

Overall, the results presented in Table 3, showing that the take-up of WaterGuard was not lower among recipients of free LLINs, suggest that cross-product entitlement effects are likely to be limited. In other words, households who get a chance to receive a free LLIN do not seem to expect that other health technologies should be given to them for free in order for them to experiment with them.

4.5 Effects of Indirect Exposure

4.5.1 Negative Subsidy Spillovers? Testing for "relative price" effects

Since prices in Phase 1 were randomized within areas, households who did not receive the maximum subsidy level offered in the area might have gotten upset or disappointed. They might have felt that they got "the bad end of the bargain", or felt that, since they didn't receive the higher subsidy, it might mean that the product was not really targeted at them, and thus they could do without it. Alternatively, those who got the "best deal" in the village might have felt particularly encouraged to purchase the LLIN.

To test this hypothesis, which we call the "relative price effect" hypothesis, we exploit the fact that the highest subsidy level offered in Phase 1 (i.e., the lowest price offered in Phase 1) randomly varied across areas. We can thus compare households who faced a given price (e.g. 50 Ksh) in two areas, one where 50 Ksh was the lowest price offered, and one where 50 Ksh was not the lowest price because other households received a voucher for a free LLIN. This is done in Table 4. The sample in this table is restricted to those who received a price in the following set: {40, 50, 70}. At each of these three prices, households in some areas got the best possible price in their area, while households in other areas did not (because other households got a free or cheaper bednet in their area). Table 4 estimates the following equation:

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

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

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

The coefficient estimates in Table 4 suggest that, contrary to the relative price effect hypothesis, households that could see other households in their area get a lower price were more likely, on average, to redeem their voucher. The negative effect becomes quite large and even significant when household controls are added. (The jump in the magnitude of the coefficient when household controls are added suggests that there might be differences across areas that are partly swept by the household controls.) Holding price constant, households that were the "luckiest" in their area and got the smallest price offered (the highest subsidy) are 18 percentage points (30%) less likely to have redeemed their voucher. In other words, households were more likely to redeem their voucher if they had neighbors who benefitted from a lower price than their own price.

While we do not find evidence of a "relative price effect" in the context of this experiment, we acknowledge that the experimental design at hand might have dampened the likelihood of such effects. This is because prices (subsidies) were randomly allocated, and therefore may not have carried any signal about potential heterogeneity in the returns to using the product. In actual distribution programs, subsidies are often targeted to those who benefit from the product most (e.g, pregnant women in the case of antimalarial bednets). When the targeting rule can be clearly identified by communities, the risk of "relative price" effect might be heightened. [18]

The results in Table 4 are consistent with a model of social learning where households learn from others about the benefits of a new technology. The next section directly tests for such social learning effects by exploiting within-area variation in exposure.

[18] Note that it could also be dampened, however – if everyone agrees that pregnant women deserve priority, households without pregnant women might be willing to pay a higher price than those with a pregnant woman. In contrast, in our experimental set-up, in which two households with comparable observable characteristics could face very different prices, strong feelings of unfairness might have arisen and led to a heightened risk of "relative price effects".

4.5.2 Positive Subsidy Spillovers? Testing for diffusion effects

Non-experimental Evidence Table 5 presents non-experimental social effect estimates. The sample is restricted to households who received a positively priced voucher in Phase 1.[19] Column 1 regresses whether households redeemed their first LLIN voucher on the number of households they know had redeemed their voucher. Knowing one more household who redeemed the voucher is correlated with a take-up higher by 12 percentage points (32%). The take-up is higher by 18 percentage points when the household declares that at least one other household recommended the LLIN to them. These results could be entirely driven by omitted variable bias, however. It is likely that households who redeemed their vouchers were more likely to ask their neighbors if they redeemed theirs too, for example. For these reasons, we next turn to experimental estimates of social learning.

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

Using GIS coordinates, we computed, for each household in the sample, the number of sampled households that live within a given radius, and the number and share of them who received a voucher for a given subsidy level. In particular, for households who faced a positive price, we computed the share of households within a given radius who received the maximum subsidy offered in the area (i.e., the share of households who received a voucher for a free LLIN in the two areas where the subsidy reached 100%; the share of households who received a voucher for an LLIN at 40 Ksh in the area where the lowest price as 40Ksh; etc.). We use three different radii to define social networks or neighborhoods: 250 meters, 500 meters, and 750 meters. Appendix Table 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. Panel B of Table A3 tests whether

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

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).

Figure 5 plots the coefficients of OLS regressions, where the dependent variable is whether or not a given household purchased at least one LLIN and the independent variable is the share (panel A) or the number (panel B) of study households within a 500m radius of the given household who received the maximum subsidy offered in the area. Both specifications show take-up of at least one 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 6 reports results from estimating regressions similar to those presented in Table 2 (columns 1 to 4), but including various measures of social exposure to LLIN, and restricting the sample to households that did not receive a free LLIN (i.e, households that received a positive-price voucher). For each radius, we run the following specification:

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

The regressor of interest is $ShareMin_{j1}$, the share of neighbors (within a given radius) who received the minimum price (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.

The results in Table 6 are quantitatively unchanged across all three radius choices. The results suggest that the higher the proportion of neighbors who received the high subsidy, the more likely the household is to have redeemed the voucher and purchased the LLIN. When looking at the results using the 'within 500m radius' definition of social networks, we find that, if all of a household's neighbors sampled for the study received the maximum subsidy, the probability of redeeming the voucher increases by 22 percentage points. This implies that households who did not themselves receive a maximum subsidy are over 50% more likely to invest in the LLIN if all of their sampled neighbors received the maximum subsidy. This is a non-trivial effect since the average price households had to pay for the LLIN was 120 Ksh ($1.85), a relatively large sum for rural households in the areas of study.

In Columns 3-4, 7-8 and 11-12 of Table 6, the independent variable is the share of sampled households within a given radius who are using the LLIN. To overcome the obvious endogeneity issue, we instrument the share using an LLIN with the share of sampled households within that radius who received the maximum subsidy level. 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 $ShareMin_{j1}$.

The results confirm that households learn through their neighbor's experimentation with the product. As discussed earlier, the exclusion restriction does not hold in the presence of contrast effects, therefore the preferred specification remains the reduced form specification.

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 a zero or very low price. The results are unchanged. 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.

Another possible specification is to 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 5 (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.

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

5 Conclusion

It is often argued that 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 increase learning about the technology if subsidy recipients do not use 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 can lead to entitlement effects for those who receive the subsidy, and to "relative price" effects for those who are not targeted by the subsidy.

This paper used a randomized field experiment to estimate the net effect of one-time subsidies on long-run adoption for a health product with high private returns (the 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.

While the randomized experimental design enables us to cleanly identify price effects, a key question concerns the external validity of our findings. 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, individuals might have vastly underestimated both the usage cost and effectiveness. In such a case, subsidies might not have affected immediate adoption and learning.

Furthermore, because the subsidy was provided by a local research organization, households in the study might have been less likely to exhibit entitlement 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 prices could have larger non-standard effects on demand. The findings that subsidies increase learning about the quality of a new product are robust to this caveat, however.

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.

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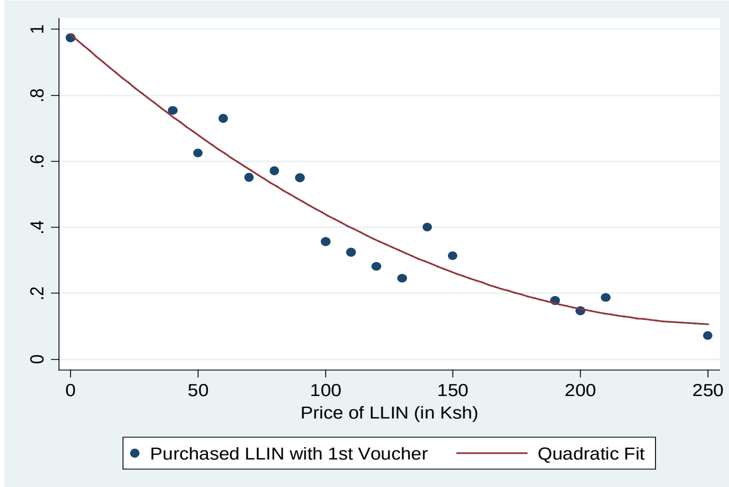
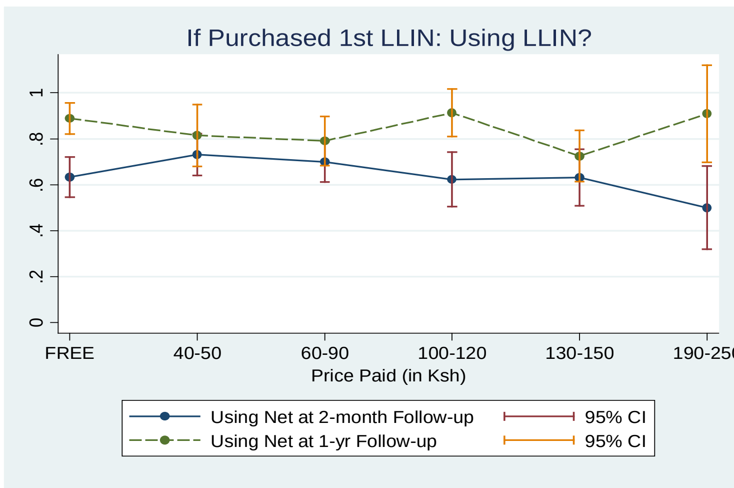
Figure 1

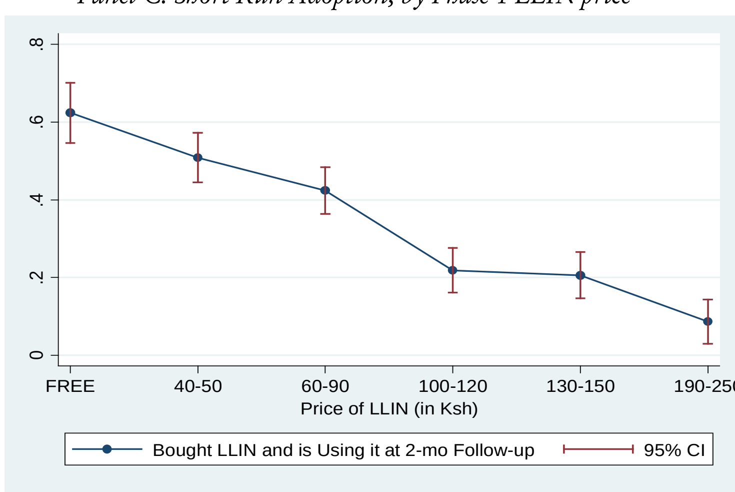
Panel A: Share of study households who purchased the LLIN in Phase 1



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



Panel C: Short-Run Adoption, 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 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 2
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)

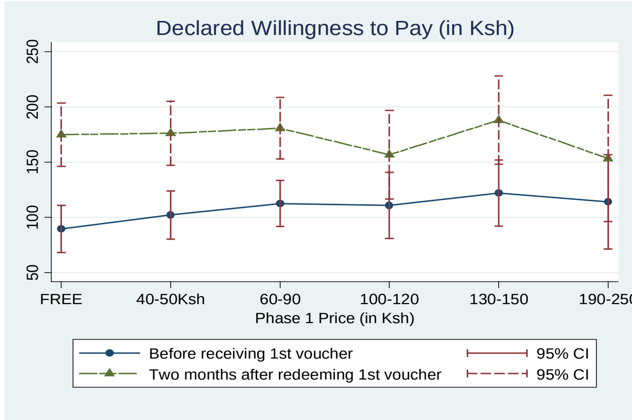


Figure 3
Anchoring around Phase 1 Price ? Gap between Declared WTP and Price Paid (subsample of households who redeemed 1st LLIN voucher)

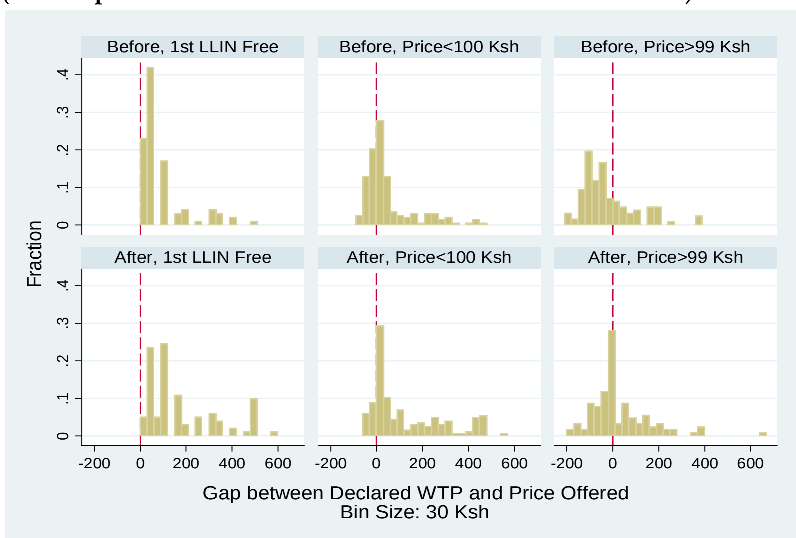
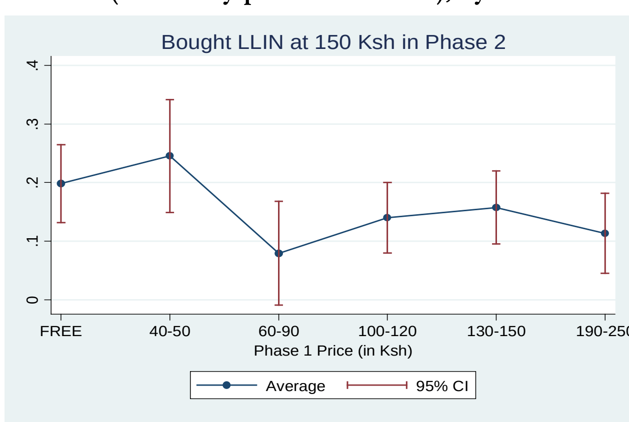


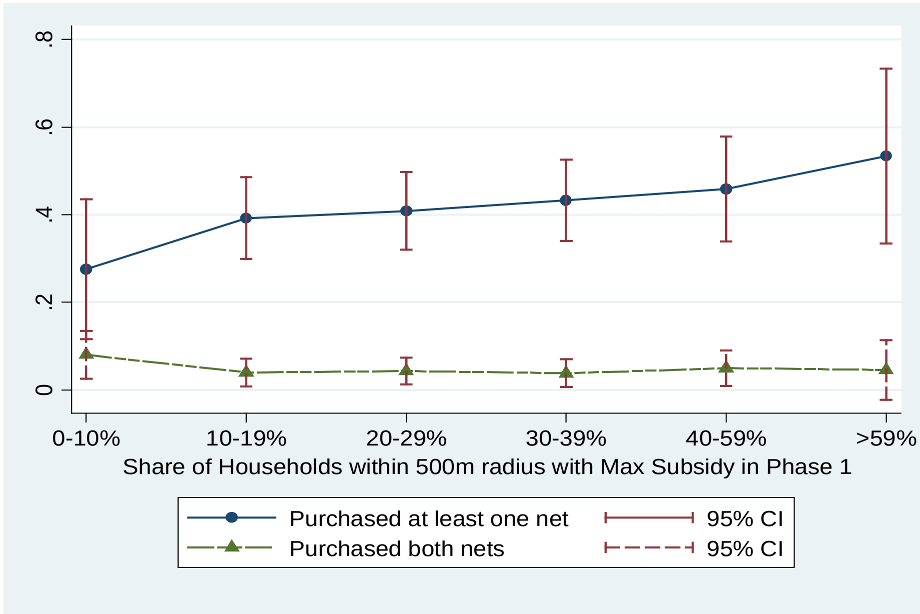
Figure 4
Redemption of 2nd LLIN Voucher (uniformly priced at 150Ksh), by 1st LLIN voucher price group



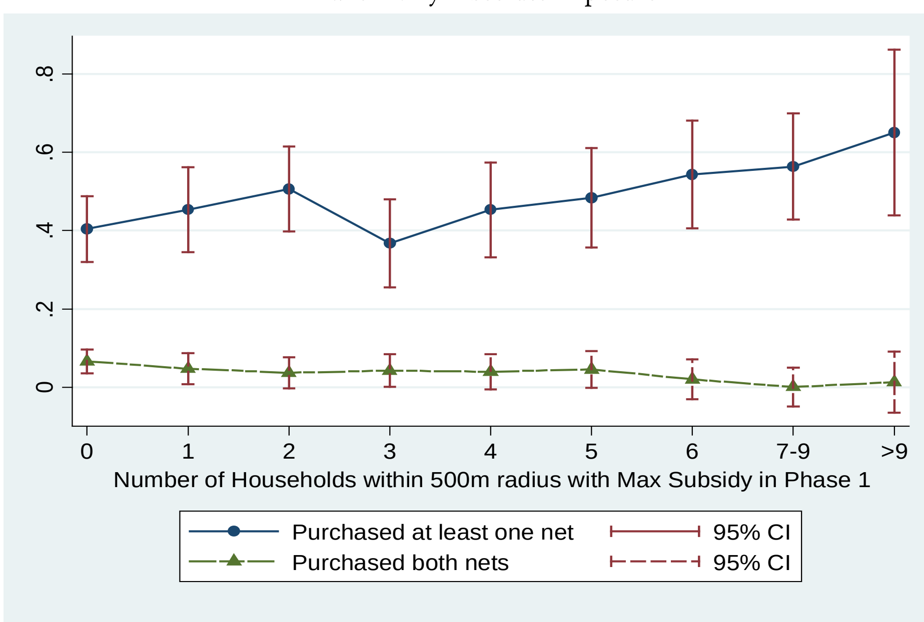
Notes: Figures 2 and 3 include data from 429 households. Figure 4 includes data from 599 households. Note that the two samples are not comparable across Figures: Figures 2 and 3 include only those who redeemed their 1st voucher, across all 6 study areas. Figure 4 includes all households (whether or not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

Figure 5
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 variables 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 (less than 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	0.00		0.15	0.15	
Mean of Dep. Variable in non-"High Subsidy" group			0.14			0.14			0.26			0.14
Estimated effect of 1st voucher price increase:												
from $0 to $1	-0.03			-0.04								
p-value	p=.4199			p=.255								
from $1 to $2	-0.02			-0.03								
p-value	p=.2299			p=.1475								
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.

Note that in the presence of contrast effects, the exclusion restriction for the instruments used in rows 5-7 will not hold.

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

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

	(1)	(2)	(3)	(4)	(5)	(6)
	Redeemed WaterGuard		First-Stage for IV:		Redeemed WaterGuard	
1st LLIN Price = 0 (Free)	0.060	0.082	0.421	0.435		
	(0.062)	(0.064)	(0.056)***	(0.057)***		
Experimented with 1st LLIN (instrumented with "1st LLIN Price = 0")					0.147	0.191
					(0.151)	(0.149)
Household level controls included		Yes		Yes		Yes
Observations	264	263	275	273	264	263
Mean of Dep. Variable in non-free group	0.40	0.40	0.37	0.37	0.40	0.40
F-Stat First Stage			56.758	58.403		

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

Table 4. *Effect of relative voucher price on take-up*

	(1)	(2)
	Bought 1st LLIN	
Got smallest price offered in the area (maximum subsidy)	-0.126	-0.169
	(0.076)*	(0.082)**
2nd Degree Polynomial in LLIN Price included	Yes	Yes
Household level controls included		Yes
Observations	308	306
Mean of Dep. Variable in non-free group	0.61	0.61

Notes: Sample restricted to households who received a voucher with a price between US$0.75 and US$1.4. Coefficient estimates obtained using linear regression. The results read as follows (column 2): for a given voucher price P within [$0.75-$1.4], households in an area where P was the smallest price offered are 17.8 percentage points less likely to have purchased the LLIN than households who faced the price P in an area where P was not the lowest price (i.e., where some households received a voucher for a price lower than P). Household level controls in column 2 include all 15 variables presented in Table 1.
Significant at 10%; ** significant at 5%; * significant at 1%.*

Table 5. *Diffusion Effects: Non-Experimental Evidence*

	(1)	(2)	(3)	(4)
	Bought 1st LLIN			
Number of HHs you know that redeemed their voucher	0.122		0.122	
	(0.019)***		(0.020)***	
LLIN was recommended by at least one HH you know		0.181		0.174
		(0.065)***		(0.065)***
2nd Degree Polynomial in LLIN Price included	Yes	Yes	Yes	Yes
HH controls included			Yes	Yes
Observations	481	481	472	472
Mean of Dep. Variable	0.39	0.39	0.39	0.39

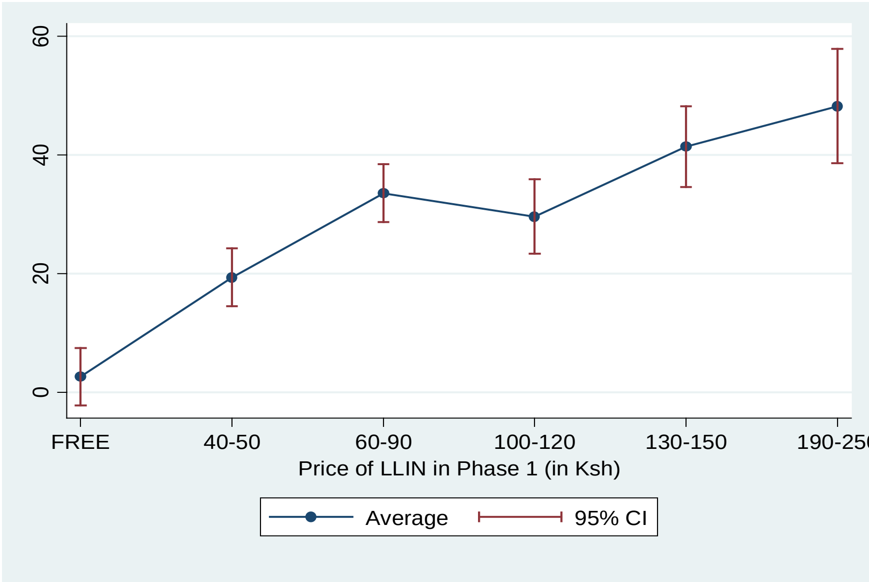
Notes: Coefficient estimates obtained using linear regression with area fixed effects. Sample restricted to households that received a positively priced voucher in the 4 areas where a follow-up survey was conducted among everyone (redeemers and non-redeemers). The mean (standard deviation) of the two independent variables of interest are: 0.6 (1.04) for the number of HHs known to have redeemed their voucher, and 0.11 (0.31) for whether the LLIN was recommended. Household level controls in columns 3-4 include all 15 variables presented in Table 1.
Significant at 10%; ** significant at 5%; * significant at 1%.*

Table 6. *Diffusion Effects: Experimental Evidence*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Bought 1st LLIN											
Within 250m radius												
Share of study households with max subsidy	0.115	0.108										
	(0.069)*	(0.069)										
Share of study households using LLIN (instrumented with *Share with max subsidy*)			0.252	0.238								
			(0.151)*	(0.153)								
Within 500m radius												
Share of study households with max subsidy					0.188	0.226						
					(0.104)*	(0.107)**						
Share of study households using LLIN (instrumented with *Share with max subsidy*)							0.363	0.456				
							(0.202)*	(0.219)**				
Within 750m radius												
Share of study households with max subsidy									0.218	0.278		
									(0.129)*	(0.135)**		
Share of study households using LLIN (instrumented with *Share with max subsidy*)											0.532	0.764
											(0.321)*	(0.381)**
Total # of study households within 500m radius (/10)	0.020	0.027	0.014	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)**
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	0.393	0.393	0.393	0.393	0.393	0.393	0.393	0.393	0.393	0.393	0.393	0.393

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. Note that in the presence of contrast effects, the exclusion restriction for the instruments used in rows 2, 4 and 6 will not hold. Household level controls include all 15 variables presented in Table 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		0.044	
	(0.043)		(0.028)	
(1st LLIN Price in US$) squared	0.007		-0.012	
	(0.013)		(0.007)*	
1st LLIN Price = 0 (Free)		-0.012		-0.018
		(0.039)		(0.031)
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		0.032	
p-value	0.750		0.142	
from $1 to $2	0.005		0.009	
p-value	0.780		0.450	

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)	(9)	(10)	(11)	(12)
	Had malaria in the month preceding the 1-yr Follow-up Survey											
1st LLIN Price in US$	0.027			0.035								
	(0.025)			(0.025)								
(1st LLIN Price in US$) squared	-0.009			-0.011								
	(0.006)			(0.006)*								
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 polynomial in price)							0.000	-0.010				
							(0.059)	(0.057)				
Experimented with 1st LLIN (instrumented with "free" dummy)									-0.074	-0.086		
									(0.066)	(0.067)		
Experimented with 1st LLIN (instrumented with "High Subsidy" dummy)											-0.063	-0.092
											(0.074)	(0.068)
Household level controls included				Yes	Yes	Yes		Yes		Yes		Yes
Observations	961	961	946	946	961	946	961	946	961	946	961	946
Mean of Dep. Variable in non-free group	0.093											
Estimated effect of 1st voucher price increase:												
from $0 to $1	0.018			0.024								
p-value	p=.364			p=.218								
from $1 to $2	0.000			0.003								
p-value	p=.97			p=.815								

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 3-6, 8, 10, and 12 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	0.60	0.53	0	3	1
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	1.89	1.33	0	6	2
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	3.56	2.07	0.00	8	4

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	0.233					
	(0.134)*					
# with max subsidy		0.023				
		(0.031)				
Total # of sampled households	0.020	-0.019				
	(0.055)	(0.087)				
Within 500m radius						
Share with max subsidy			0.161			
			(0.204)			
# with max subsidy				0.000		
				(0.017)		
Total # of sampled households			0.011	0.013		
			(0.022)	(0.043)		
Within 750m radius						
Share with max subsidy					0.273	
					(0.257)	
# with max subsidy						0.006
						(0.012)
Total # of sampled households					-0.001	-0.011
					(0.015)	(0.032)
Observations	985	985	985	985	985	985

Notes: Coefficient estimates obtained using linear regression with area fixed effects. Sample restricted to households that received a positive-price 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	0.090								
	(0.084)								
Share of study households with price 100-120	0.105								
	(0.077)								
Share of study households with price 130-150	0.021								
	(0.083)								
# of study households with max subsidy		0.000	0.003						
		(0.013)	(0.014)						
# of study households with price 60-90			0.000						
			(0.014)						
# of study households with price 100-120			-0.010						
			(0.012)						
# of study households with price 130-150			-0.005						
			(0.015)						
Within 500m radius									
Share of study households with max subsidy				0.224					
				(0.114)*					
Share of study households with price 60-90				-0.002					
				(0.115)					
Share of study households with price 100-120				-0.047					
				(0.115)					
Share of study households with price 130-150				0.057					
				(0.136)					
# of study households with max subsidy					0.010	0.011			
					(0.009)	(0.009)			
# of study households with price 60-90						-0.006			
						(0.009)			
# of study households with price 100-120						-0.004			
						(0.012)			
# of study households with price 130-150						0.006			
						(0.010)			
Within 750m radius									
Share of study households with max subsidy							0.251		
							(0.144)*		
Share of study households with price 60-90							-0.064		
							(0.147)		
Share of study households with price 100-120							-0.024		
							(0.143)		
Share of study households with price 130-150							-0.098		
							(0.172)		
# of study households with max subsidy								0.009	0.012
								(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	0.006	0.021	0.027	0.001	0.012
(/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	0.393	0.393	0.393	0.393	0.393	0.393	0.393	0.393	0.393

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%.*