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

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February 14, 2012

Abstract

Short-run subsidies for health products are common in poor countries. How do they affect long-run adoption? A common fear among development practitioners is that one-off subsidies may negatively affect long-run adoption through reference-dependence: People might anchor around the subsidized price and be unwilling to pay more for the product later. But one-off subsidies could also boost long-run adoption through learning and income effects. This paper provides a theoretical framework outlining the types of products and the contexts in which these various effects may be important, and uses a two-stage randomized field experiment in Kenya to estimate their relative importance for one product, a new antimalarial bednet. Findings suggest that a one-time subsidy has a positive impact on willingness to pay a year later, and a positive impact on adoption among neighbors of subsidy recipients.

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

*I am grateful to Jean-Marc Robin, four anonymous referees, Christian Hellwig, Adriana Lleras-Muney and Aprajit Mahajan for detailed suggestions, and to Sandra Black, Sylvain Chassang, Jessica Cohen, Esther Duflo, Giacomo De Giorgi, Frederico Finan, Seema Jayachandran, Robert Jensen, Rohini Pande, Jonathan Robinson, Justin Sydnor, and numerous seminar participants for helpful comments and discussions. I thank Katie Conn, Moses Baraza, and their field team for their outstanding project implementation and data collection. The study was funded by the Acumen Fund, the Adessium Foundation, the Exxon Mobil Foundation, a Dartmouth Faculty Burke Award and UCLA Economics department. The Olyset nets used in the study were donated by Sumitomo Chemical. All errors are my own.

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1 Introduction

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

A commonly proposed way to increase adoption in the short run is to distribute those essential health products for free or at highly subsidized prices (WHO, 2007; Sachs, 2005). There are two main economic rationales to do so. First, given the infectious nature of the diseases they prevent, most of these products generate positive health externalities, and without a subsidy private investment in them is socially suboptimal. Second, when the majority of the population is poor and credit-constrained, subsidies may be needed to ensure widespread access (Cohen and Dupas, 2010; Tarozzi et al., 2011).

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

A short-run subsidy may increase demand in the long run if the product is an experience good. Beneficiaries of a free or highly subsidized sample will be more willing to pay for a replacement after experiencing the benefits and learning the true value of the product if they previously had underestimated these benefits. This learning might 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 make repeat purchases.

These positive effects hinge upon people's making use of a product or technology that they receive for free or at a highly subsidized price. This might not be the case, however. Households that are not willing to pay a high monetary price for a product might also be unwilling to pay the non-monetary costs associated with using the product on a daily basis. In other words, subsidies may undermine the "screening effect" of prices (Ashraf, Berry and Shapiro, 2010; Chassang, Padro i Miquel and Snowberg, forthcoming). Subsidies could also reduce the potential for psychic effects associated with paying for a product, such as the "sunk cost" effect, whereby people who have paid more for a product feel more compelled to put it to good use.[3]

Even if people use products they receive as free trials, they might be unwilling to pay a higher

[1] Black et al, 2003.

[2] Jones et al., 2003.

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

price for the product once the subsidy ends or is reduced. This could happen if people take previously encountered prices as reference points, or anchors, that affect their subsequent reservation price (Koszegi and Rabin, 2006). Such effects, known in psychology as "background contrast effects" and first identified experimentally by Simonson and Tversky (1992), have recently been observed outside the lab by Simonsohn and Loewenstein (2006). Under such reference-dependent preferences, one-time subsidies for health products could generate a sort of "entitlement effect" that would dampen long-run adoption.

The view that these negative effects might dominate the standard positive learning and health effects is quite prevalent among development practitioners. As the Boston Globe summarized: *"The Holy Grail of international development has long been sustainability – [...] for several decades it's been the conventional wisdom that unless people spend money on something they will be unlikely to value it – or use it. Give things away and they will be taken for granted, it's thought."*[4] There is, however, no rigorous evidence to date as to what short-run subsidies do to long-run adoption of new technologies.

To inform this debate, we start by presenting a simple model for understanding the role of prices in the adoption of technologies for which adoption requires not only acquisition, but also repeated usage of the technology once acquired. The model generates predictions as to the technology characteristics and the contexts under which subsidies in one period will increase or decrease the level of adoption next period. We then estimate the long-run impact of a short-run subsidy for one specific technology (an innovation in malaria control) through a field experiment in rural Kenya.

The key feature of the model is that health products or technologies have two characteristics that are unknown to households when they are first introduced: how much they impact available income and how much they impact quality of life. How much a health technology affects available income depends on how much it improves productivity and how much it reduces curative health expenditures, both of which depends on the technology's health effectiveness. How much a health technology affects the quality of life depends on how badly the health issue it tackles affects the marginal utility of consumption, and on the side effects of using the product (for example, how "hot" it is to sleep under a bednet, or how nauseous one becomes from swallowing a deworming pill). In environments where individual-specific health shocks are common, observing the effects on disposable income might take some time, while observing the side effects might be relativily quick. One-time subsidies will fail to boost the adoption of technologies whose quality-of-life impacts are negative if the subsidy is too short-lasting to enable people to observe and benefit from the health effectiveness. In contrast, one-time subsidies may affect the diffusion process of products whose effectiveness can be felt over the course of the subsidized trial. The potential magnitude of the effect will depend on the steepness of the demand function – by how much the subsidy affects adoption in the first period. This in turn depends on a number of factors, including anticipated learning and health spillovers. The sign of the effect will depend on whether people update positively or

[4] 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/

negatively on the product's attributes. For effective products with higher quality-of-life impacts than anticipated, one-off subsidies have the potential to boost subsequent adoption through both a learning and an income effect. If people exhibit reference-dependence, however, these positive effects might be trumped by a large negative anchoring effect.

To gauge the relative importance of these effects, we conducted a field experiment in Kenya with a new health product, the Olyset long-lasting insecticide-treated bed net (LLIN), a recent innovation in malaria control. The Olyset LLIN is significantly more comfortable to sleep under than traditional bednets, it is sturdier and more durable, and it stays effective for much longer. Given these characteristics, its long-run adoption should be boosted by the learning and income effects of a one-time subsidy, unless anchoring around the subsidized price is important. The experiment involved 1,120 households in Kenya and included two phases. In Phase 1, subsidy levels for Olyset LLINs were randomly assigned across households within six villages. Households had three months to acquire the LLIN at the subsidized price they had been assigned to. Prices varied from \$0 to \$3.80, which is about twice the average daily wage for casual agricultural work in the study area. In Phase 2, a year later, all households in four villages were given a second opportunity to acquire an Olyset LLIN, but this time everyone faced the same price (\$2.30). The LLIN was not available outside of the experiment, but traditional nets were available on the market at the retail price of \$1.50.

This experimental design allows us to estimate the effects of one-off subsidies on demand, both over time and across individuals.

We first test whether subsidies increased the level of adoption in the first phase. We find very large effects: adoption increases from 7% to over 60% when the price decreases from 350 Ksh (\$3.80) to 50 Ksh, and reaches 98% when the price drops to zero. This implies a very large potential for learning and health effects. We then estimate how the Phase 1 subsidy level affects willingness to pay for an LLIN in Phase 2. Statistical precision is somewhat limited by the sample size, but we find suggestive evidence that gaining access to a highly subsidized LLIN in the first year increases households' observed willingness to pay for an LLIN a year later: households who had to pay 50 Ksh or less in Phase 1 were 7.2 percentage points more likely to invest in a 150-Ksh LLIN in Phase 2 than those who faced a higher Phase-1 price (22.0% vs. 14.8%, corresponding to a 49 percent increase). This suggests the presence of positive learning and income (via health) effects which dominate any potential anchoring or entitlement effect. Suggestive follow-up survey evidence is consistent with the presence of both learning and health effects.

We then turn to studying the social effects of subsidies. To avoid the classic reflection problem in the estimation of social effects (Manski, 1993), we exploit the exogenous variation in the density of households who received a highly subsidized LLIN in Phase 1 as a source of exogenous variation in indirect exposure to the product. Here again statistical precision is limited, but we find suggestive evidence that households facing a positive price in Phase 1 were more likely to purchase the LLIN when the density of households around them who received a highly subsidized LLIN was greater. This suggests informational spillovers, and the timing of LLIN purchases as well as survey evidence

that households discussed the LLIN with each other is consistent with such social learning.

Overall, our results suggest that the total effect of short-run subsidies on long-run adoption of LLINs is positive. Previously encountered prices matter, but more so through their effect on available income and knowledge about the product than through an anchoring or entitlement effect.

The model helps reconcile our findings with those of two previous studies, Ashraf et al. (2010) and Kremer and Miguel (2007), which both found empirical results somewhat opposite to ours. Ashraf et al. (2010) find that subsidies for a water chlorination product in urban Zambia failed to increase even the short-run adoption rate of the product. Their result is consistent with the case of our model in which the perceived quality-of-life impact of the product is negative (people do not like drinking chlorine-tasting water) and the subsidized trial period is too short for people to learn about health effectiveness anyway. Kremer and Miguel (2007) use a randomized evaluation of a school-based deworming program in Kenya to estimate the role of peer effects in health technology adoption. They find that households were *less* likely to invest in deworming if they had a higher number of social contacts who benefited from free deworming in the past. Their negative effect is also consistent with our model. As deworming pills generate negative side effects, subsidies for deworming lead households to revise their beliefs about the private returns to using the drug downwards, leading to lower long-run adoption.

Our paper contributes to a growing literature on the role of learning-by-doing and social learning in technology adoption in poor countries. The evidence so far, mostly non-experimental and mostly focused on agricultural technologies, is rather mixed and suggests that the role of social learning is likely to vary greatly with the context and the product considered.[5] Our paper also contributes to the empirical "psychology and economics" literature, testing behavioral economics in the field (see DellaVigna, 2009, for a review), and complements earlier papers that have estimated, in rich countries, how the willingness to pay for a product can be affected by anchors (Ariely, Loewenstein and Prelec, 2003), previously encountered prices (Simonsohn and Loewenstein, 2006; Mazar et al., 2009), or the range of options available (McFadden, 1999; Heffetz and Shayo, 2009). Finally, our paper makes a contribution to the literature on experience goods pricing (Bergemann and Valimaki, 2000, 2006).

The remainder of the paper is as follows. Section 2 presents a the theoretical framework. Section 3 presents some background information on malaria and antimalarial bednets, and then describes the experimental design. Section 4 presents the empirical results. Section 5 concludes.

[5] Foster and Rosenzweig (1995) and Besley and Case (1997) find that a farmer's ability to reap profits from a new technology increases with 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 (2010) present evidence that social learning is important in the diffusion of knowledge regarding pineapple cultivation in Ghana, while the randomized experiment of Duflo et al. (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) find evidence of S-shaped diffusion of attitudes and behaviors with respect to contraception and AIDS among young women in rural Kenya. 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. Oster and Thornton (2009) find evidence of peer effects in the usage of a new female hygiene product provided for free.

2 Model

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 highlight how the relative importance of these channels depends on the characteristics of the technology, as well as the population's priors and ability to learn about these characteristics.

2.1 Basic Setup

We consider a two-period discrete choice model. In each period, the household has the option to adopt or not adopt a health product j. "Adopting" the product in a given period means *purchasing and using* the product over the course of the period. At the end of the period, the product needs to be replaced.

Adopting the health product in a given period can affect the household in two ways:

1. Adoption potentially improves (or dampens) the ability of the household to enjoy its contemporaneous consumption of other goods. To reflect this in the model, we use a Cobb-Douglas utility function in expenditures on goods and services (other than the health product considered) and the "quality-of-life" impact of the health product. Specifically, the product is characterized by a quality level $Q > 0$. When $Q = 1$, the product has no effect on the marginal utility of consumption. When $Q > 1$, the product increases the marginal utility of consumption: it improves health in a way that makes wealth more enjoyable.[6] When $Q < 1$, the product decreases the marginal utility of consumption, for example because it generates important negative side effects.

2. Adoption potentially increases the income of the household in the next period. The idea here is that a product that is effective at improving health will reduce curative health expenditures as well as increase the ability of the household to effectively supply labor. To reflect this, the product is characterized by an effectiveness level $e \geq 0$. When $e = 0$, usage of the product has no impact on next period's income. When $e > 0$, usage of the product increases next period's income by a factor e.

Note that these two attributes of the product (quality Q and effectiveness e) need not be related. A product might be very effective at reducing ill health while at the same time generating very bad side effects. A good example is the early generation of antiretroviral therapy for AIDS patients: it was very effective at prolonging life and thus increasing income in the future, but it had very bad side effects (so bad that some patients would stop taking their medicine). Likewise, treating one's water with diluted chlorine reduces the risk of diarrhea but having to consistently drink chlorine-tasting water might be considered by some as reducing the quality of life. On the other hand, there

[6] Using US data on the elderly, Finkelstein et al.(2008) find evidence that good health increases the marginal utility of consumption.

can be products that are totally ineffective ($e = 0$) but nevertheless increase the marginal utility of consumption ($Q > 1$). An exampe is pain killers, which reduce the pain but do not treat the underlying illness causing the pain.

How these two attributes Q and e are valued can vary across households. For example, how much the product can boost income will depend on the demographic composition of the household. Likewise, how much the product can boost the quality of life will depend on the household's tolerance for side effects. The fundamental ingredient of the model is the fact that in period 1, household h do not know its personal valuations of the attributes $\{Q_h, e_h\}$ of the product. It holds beliefs about them, but those beliefs can be underestimates or overestimates. There are however two ways in which households can learn about the product's attributes between periods 1 and 2: they can learn through own experimentation with the product, and they can learn by asking other experimenters about the characteristics of the product.[7]

The question of interest is: Can the social planner affect the "long run" (= period 2) adoption rate by manipulating the price in the short run (period 1)? If so, what is the period 1 price that maximizes period 2 adoption?

This is a complex problem which becomes quickly intractable without simplifying assumptions. Rather than derive the optimal policy, we thus focus on spelling out the conditions under which a period 1 subsidy will have a positive, negative or zero impact on the period 2 adoption rate, and the conditions under which this impact will be large or small.

In what follows, we start by making some functional-form and distributional assumptions that will allow (some) explicit solutions. We then derive analytical results in two steps. We first assume that households are myopic (they do not consider period 2 utility when making their period 1 choice) and do not exhibit reference-dependence. We then allow for myopic households to exhibit reference-dependence in period 2. In both cases, we show that the impact of period 1 subsidies on period 2 adoption is ambiguous, and depends on a few key parameters. In a third step, we consider the fully forward-looking case, where households take into consideration the option value of learning as well as potential income effects when making their period 1 choice. We cannot derive simple analytical solutions to this case given the discrete time setting but we provide a discussion of the key parameters that will determine whether the ultimate impact of period 1 subsidies on period 2 adoption is positive or negative. We then discuss how these key parameters differ, and therefore the model's predictions vary, across five types of health products for which temporary subsidies have been considered: water disinfectants, deworming medicine, antimalarial bednets, improved cookstoves and water filters.

[7] The fact that households can learn something from other households about their own valuations of the product's attributes, even those valuations are household-specific, reflects the fact that households can discuss the details of their experience with others. For example, a household might say: "the bednet really repels mosquitoes but the problem is that it gets hot underneath". Then a household that doesn't mind heat but cares about mosquitoes might be able to infer its own estimate of q from this.

2.2 Functional Forms and Distributional Assumptions

The household intertemporal utility is a log-linear function of two identical one-period utility functions, each of which is in turn log linear. The one-period utility functions are weighted exponentially to reflect time preference, the first year having a higher weight (normalized to 1) than the second ($\beta < 1$).

2.2.1 Per-period utilities

The per-period utility derived by household h from adopting product j in period t is given by:

$$U_h^j(y_{ht}, p_t, \tilde{q}_{ht}) = (y_{ht} - p_t)^\alpha \tilde{Q}_{ht} \exp(\epsilon_{ht}^j)$$

where y_{ht} is the household's income in period t, p_t is the price of the product in period t, $\tilde{Q}_{ht}$ is the quality-of-life impact of the product as perceived by household h at period t, and ϵ_h^j is a mean-zero stochastic term, assumed to be i.i.d and distributed according to a Type I extreme value distribution. The indirect utility $u_h^j = \log[U_h^j]$ can be written:

$$u_{ht}^j = \alpha \log(y_{ht} - p_t) + \tilde{q}_{ht} + \epsilon_{ht}^j \tag{1}$$

where $\tilde{q}_{ht}$=$\log \tilde{Q}$. The per-period utility derived by household h from not adopting the product in period t is:

$$u_{ht}^0 = \alpha \log(y_{ht}) + \epsilon_{ht}^0 \tag{2}$$

2.2.2 State variables and state transitions

In each time period $t \in \{1, 2\}$, household h:

- has beliefs $\{\tilde{q}_{ht}, \tilde{e}_{ht}\}$ about the product's true attributes $\{q_h, e_h\}$
- gets an income y_{ht}
- decides whether to adopt the product ($a_{ht} = 1$) or not ($a_{ht} = 0$)

Evolution of beliefs In period 1, we assume that all households have the same beliefs $\{\tilde{q_{1h}} = \bar{q}_1, \tilde{e}_{1h} = \bar{e}_1\}$. In period 2, household h's beliefs depends on whether h adopted the product in period 1 (this is *learning by doing*), as well the adoption rate among other households around household h (this is *social learning*). We consider a simple, adaptive learning process: perfect signals about the product's characteristics arrive with some probability to those who experiment with the product. Specifically:

- If household h adopts in period 1 ($a_{h1} = 1$), it has a chance λ_{qo} to get a perfect signal and learn its valuation of the product's quality q_h by the end of period 1, and a chance λ_{eo} to get a perfect signal and learn its valuation of the product's effectiveness e_h by the end of period 1.

- Household h can also learn from its neighbors. We call S_{h1} the share of households in the neighborhood of household h who adopt the product in period 1. For each of the product characteristics $c = q, e$, the household has a chance $\lambda_{cs} S_{h1}$ to learn about that characteristic from a neighbor.

We have the following relationships between period 2 beliefs and period 1 actions:

$$\tilde{q_{h2}} = a_{h1}\lambda_{qo} q_h + (1 - a_{h1}\lambda_{qo})\lambda_{qs} S_{h1} q_h + (1 - a_{h1}\lambda_{qo})(1 - \lambda_{qs} S_{h1})\bar{q_1} \quad (3)$$

$$\tilde{e_{h2}} = a_{h1}\lambda_{eo} e_h + (1 - a_{h1}\lambda_{eo})\lambda_{es} S_{h1} e_h + (1 - a_{h1}\lambda_{eo})(1 - \lambda_{es} S_{h1})\bar{e}_1 \quad (4)$$

The first terms in expressions (3) and (4) correspond to *learning by doing*. The second terms correspond to *social learning*. The third terms in (3) and (4) imply that beliefs about q_h and e_h remain unchanged between periods 1 and 2 if the household doesn't receive any information from either own experience or the experience of others.

Note that we allow the likelihood of learning about the quality q and the effectiveness e of the product to be different: in between period 1 and period 2, the chance of learning about the quality through own experience, λ_{qo}, may differ from the chance of learning about the effectiveness through own experience (λ_{eo}). Likewise for learning from others. This is important since for products with side effects, learning that $Q \leq 1$ ($q \leq 0$) might be very easy (people just observe the discomfort associated with product usage), while learning about the income impacts might be harder (e.g., if there are many competing diseases and observing that one specific product decreases illness risk takes time). We will come back to this point in section 2.6, when we compare health products to each other.

Evolution of income We don't allow for any borrowing or saving. In period 1, household h receives a known income y_{h1}. In period 2, household h receives an income:

$$y_{h2} = y_{h1}(1 + a_1 e_h + s(S_{h1}, a_1, e_h))$$

This formula allows for both a direct effect of own product usage on health and thus income ($a_1 e_h$), and a spillover effect of neighbor's usage (the health externality, ($s(S_{h1}, a_1, e_h)$). This health externality reflects the fact that, for infectious diseases, transmission rates go down the higher the share of people who adopt protection. The shape of this spillover effect is expressed by the function s. Assuming $s(\cdot) = 0$ shuts down any spillover.[8]

From the point of view of period 1, the only uncertainty on period 2 income comes from the fact that the household's valuation of the product's effectiveness $e_h \geq 0$ is not known. Note that absent any spillovers, a household that does not purchase the health product in period 1 has no uncertainty about its period 2 income: households who do not adopt the product in period 1 receive the same income in both periods: $y_{h2} = y_{h1}$.

[8] For many diseases, the medical literature suggests that $s(.)$ is S-shaped. The fact that a_1 is an argument in $s(.)$ is meant to allow the spillover effect to be lower for those who are using the product themselves.

2.3 Myopic Case

2.3.1 Period 1 Adoption

In the myopic case, the household considers only the period 1 utility when making the period 1 purchase decision. Dropping the h subscripts, the household chooses to adopt the product in period t at price $p_t < y_t$ if and only if $u_1^j \geq u_1^0$, that is if and only if:

$$\alpha \log(\frac{y_1 - p_1}{y_1}) + \bar{q}_1 \quad \geq \quad \epsilon_1^0 - \epsilon_1^j$$

Given the extreme value assumption on the error terms, their joint distribution is logistic, and the probability that the household adopts the product in period t can be written:

$$\Pr(a_1 = 1) = \Pr(p_1 < y_1) \times \frac{1}{1 + \exp(-\alpha \log(\frac{y_1 - p_1}{y_1}) - \bar{q}_1)} \tag{5}$$

and the share of the population that adopts in period 1 is $S_1 = \int_h \Pr(a_1 = 1|y_1) dy_{h1}$.

It is trivial to show that $\frac{\partial \Pr(a_1=1)}{\partial p_1} < 0$ (the probability that the household purchases the product decreases with its price), and that the demand function is steeper, the lower the household income (y). With a bit more algebra, one can show that for beliefs about $\bar{q}_1$ above a certain threshold ($\bar{q}_1 > -\alpha \log(\frac{y_1 - p_1}{y_1})$), the demand function becomes flatter as $\bar{q}_1$ increases.

2.3.2 Effect of Period 1 price on Period 2 Adoption

We are now ready to derive the comparative static of interest, namely, how period 2 adoption varies with period 1 price. By the law of total probability, we have:

$$\begin{aligned}
\Pr(a_2 = 1|p_1, p_2) &= \Pr(a_2 = 1|a_1 = 1, p_2) \times \Pr(a_1 = 1|p_1, p_2) \\
&\quad + \Pr(a_2 = 1|a_1 = 0, p_2) \times \Pr(a_1 = 0|p_1, p_2) \\
&= [\Pr(a_2 = 1|a_1 = 1, p_2) - \Pr(a_2 = 1|a_1 = 0, p_2)] \times \Pr(a_1 = 1|p_1) \\
&\quad + \Pr(a_2 = 1|a_1 = 0, p_2)
\end{aligned} \tag{6}$$

We are interested in the partial derivative $\frac{\partial \Pr(a_2=1|p_1,p_2)}{\partial p_1}$. We proceed in two steps in analyzing that partial derivative: we first ignore the spillover effects, and then add them back in.

Assuming no informational or health spillovers

In the absence of any spillover effect (that is, if we shut down both the informational spillover, $\lambda_{qs} = 0$, and the health externality, $s(\cdot) = 0$), then period 1 price does not affect the state variables (beliefs, income) through adoption by others. This means that $\Pr(a_2 = 1|a_1 = 1, p_2)$ and $\Pr(a_2 = 1|a_1 = 0, p_2)$ are independent of p_1, and p_1 only affects whether one adopts the product in period

1, $\Pr(a_1 = 1|p_1)$. We then have:

$$\frac{\partial \Pr(a_2 = 1|p_2,\ \text{no spill})}{\partial p_1} = \frac{\partial \Pr(a_1 = 1)}{\partial p_1} \times \Big[\Pr(a_2 = 1|a_1 = 1, p_2) - \Pr(a_2 = 1|a_1 = 0, p_2)\Big] \tag{7}$$

The first term in (7) corresponds to the extensive margin effect: how the price affects the probability of adoption in period 1 (the demand function). The second term in (7) corresponds to an "intensive margin" effect: how adoption in period 1 affects the probability of adoption in period 2 by affecting the state variables, namely, the beliefs about the product's attributes and income.

The extensive margin effect will affect the magnitude of the impact of the period 1 subsidy: The flatter the demand function in period 1, the smaller the extensive margin effect, and the lower the potential for an impact of a period 1 subsidy on period 2 adoption, whether positive or negative. The steeper the demand function, the larger the potential for an impact of period 1 subsidy on period 2 adoption.[9]

The intensive margin effect will determine the sign of the impact: We know that $\frac{\partial P(a_1=1)}{\partial p_1} \leq 0$ (demand is downward sloping), therefore the sign of (7) is the opposiste of the sign of the intensive margin effect. We thus have:

$$\begin{aligned} \frac{\partial P(a_2 = 1|p_2,\ \text{no spill})}{\partial p_1} &\leq 0 \\ \Leftrightarrow \Pr(a_2 = 1|a_1 = 1, p_2) - \Pr(a_2 = 1|a_1 = 0, p_2) &\geq 0 \\ \Leftrightarrow \alpha \log(\frac{y_1(1+e_h) - p_2}{y_1(1+e_h) - p_2(1+e_h)}) + \lambda_{qo}(q_h - \bar{q_1}) &\geq 0 \end{aligned} \tag{8}$$

A period 1 subsidy will boost period 2 adoption if (8) holds. The first term on the left-hand side of equation (8) corresponds to an income effect, and the second term corresponds to a learning effect. The income effect of a period 1 subsidy is always positive (since either the product improves health, or it doesn't, but we do not allow for the product to be harmful to health). The learning effect of the period 1 subsidy can be negative, if the product turns out to be of lesser quality than expected ($q_h - \bar{q_1} < 0$). Ultimately, the sign of the intensive margin effect depends on the sign of the learning effect, and if that effect is negative, on the relative magnitude of the learning and income effects. This in turn depends on the attributes of the product (q_h, e_h), on the prior about the product quality ($\bar{q_1}$), and on how easy it is to learn in one period (λ_{qo}).

- In the simplest case where the product's effectiveness on income is zero ($e_h = 0$), there is no income effect, and (8) holds if and only if $\lambda_{qo}(q_h - \bar{q_1}) \geq 0$. This implies that:

[9] Other (unmodeled) mechanisms through which the period 1 price could matter can be thought in terms of the steepness of the demand function in period 1. For example, it could be that households interpret the price in period 1 as a signal of quality (Bagwell and Riordan, 1991). This would make the period 1 demand function flatter, and therefore reduce the potential for an extensive margin effect of a subsidy. Alternatively, if housheolds interpret the *subsidy level* in period 1 as a signal of quality, then the period 1 demand function will be steeper, and the extensive margin effect of a subsidy will be heightened.

- Case 1: If people initially underestimate the quality of the product ($\bar{q_1} < q_h$), then a subsidy in period 1 weakly increases adoption in period 2.
- Case 2: Conversely, if people initially overestimate the quality of the product ($\bar{q_1} > q_h$), then a subsidy in period 1 weakly decreases adoption in period 2.
- The learning effect only comes about if $\lambda_{qo} \geq 0$, that is, if it is actually possible to learn about the quality of the product within one period. If learning about the product within one period is very difficult (λ_{qo} is small), the learning effect (whether positive or negative) is small.

- If the effectiveness of the product on income is non-zero ($e_h > 0$), then the income effect reinforces the positive learning effect of the period 1 subsidy on period 2 adoption in Case 1, and counterbalances the negative learning effect in Case 2. The higher the product's effectiveness e_h, the larger the income effect, and therefore the larger the downward update in quality can be before it dominates the income effect.

Allowing informational and health spillovers

The period 1 price affects not only the probability that one adopts the product in period 1, it also affects the probability that neighbors adopt the product. In the presence of spillovers, one's neighbors' adoption behavior affects one's beliefs about the product's attributes (through social learning) and period 2's income (through the health externality), and therefore the conditional period 2 adoption probabilities.

After some manipulation, the impact of period 1 price on period 2 adoption in the presence of spillovers can be written:

$$\frac{\partial \Pr(a_2 = 1|p_2)}{\partial p_1} = \frac{\partial \Pr(a_2 = 1|p_2,\ \text{no spill})}{\partial p_1} + \left[\frac{\partial \Pr(a_2 = 1|a_1 = 1, p_2)}{\partial S_1}\right] \times \frac{\partial S_1}{\partial p_1} \times \Pr(a_1 = 1) + \left[\frac{\partial \Pr(a_2 = 1|a_1 = 0, p_2)}{\partial S_1}\right] \times \frac{\partial S_1}{\partial p_1} \times (1 - \Pr(a_1 = 1)) \quad (9)$$

The first term on the right-hand side of equation (9) corresponds to the individual effect derived in the no-spillover case above. The second term on the right-hand side corresponds to the spillover effects (the informational spillover and the income spillover through the health externality) on the period 1 adopters. Finally, the third term on the right-hand side corresponds to the spillover effects on those who do not adopt in period 1. Just like the private income effect, the spillover income effect of subsidies is non-negative – it is either positive (if the product decreases the transmission rate of the disease) or zero if the product is ineffective or if the disease in question is non-infectious. Also just like the learning-by-doing effect, the information spillover of subsidies can be either positive or negative, depending on the true quality of the product.

2.4 Myopic Case with Reference-Dependence

To introduce reference-dependence, we modify the instantaneous utility derived by household h from purchasing product j in period 2 at price $p_2 > 0$ by adding the underlined term as follows:

$$U_h^j(y_{h2}, p_2, \tilde{q}_{h2}) = (y_{h2} - p_2)^\alpha \underbrace{\tilde{Q}_{h2} r^{g(p_r - p_2)}} \exp(\epsilon_h^j)$$

where p_r is the reference price, $r \geq 1$ is a "reference-dependence" parameter and $g()$ is an increasing function kinked at zero with $g(0) = 0$. If $r = 1$, there is no refence dependence and the utility function is similar to that studied above. If $r > 1$, the utility is lowered by perceived "losses" (paying more than the reference price) and increased by perceived "gains" (paying less than the reference price), but the kink at zero is such that "gains" are valued less than the "losses", as has been found empirically (Tversky and Kahneman, 1991).

If people take the period 1 price as the reference price (independently of their period 1 purchase decision), equation (7) becomes:

$$\begin{aligned} \frac{\partial \Pr(a_2 = 1 | p_2, \text{ no spill})}{\partial p_1} &= \frac{\partial \Pr(a_1 = 1)}{\partial p_1} \times \Big[\Pr(a_2 = 1 | a_1 = 1, p_2) - \Pr(a_2 = 1 | a_1 = 0, p_2)\Big] \\ &+ g'(p_1) \log r \times \frac{\Pr(p_2 < y_2) \times \exp\left[-\alpha \log(\frac{y_2 - p_2}{y_2}) - \tilde{q}_2 - g(p_1 - p_2) \log r\right]}{\left(1 + \exp\left[-\alpha \log(\frac{y_2 - p_2}{y_2}) - \tilde{q}_2 - g(p_1 - p_2) \log r\right]\right)^2} \end{aligned} \tag{10}$$

In addition to affecting the adoption rate in period 2 through the extensive and intensive margin effects discussed above, the period 1 price will also affect the conditional period 2 adoption through an anchoring effect (the term on the second line of equation 10). This anchoring effect of prices will be *positive* (or zero if people do not anchor), meaning that a higher subsidy in period 1 *lowers* adoption in period 2. Thus, even if the sum of the income and learning effects of higher period 1 prices is negative and therefore the term on the first line of (10) is negative, the total effect of period 1 price on period 2 adoption can be positive (i.e., subsidies can dampen future adoption) if anchoring is important.

2.5 Forward-Looking Case

So far we have assumed that the household is myopic: it does not anticipate the income effect (its period 1 prior on e_h is $\bar{e}_1 = 0$) and it does not consider the option value of learning when making its adoption decision in period 2. In the appendix, we discuss how allowing forward-looking behavior affects the results obtained so far. The main result is that forward-looking behavior reduces the steepness of the demand function in period 1, thereby reducing the extensive margin effect, but it leaves the intensive margin effect unchanged.

The magnitude of the change in the extensive margin effect depends on the expected income effect and on the option value of learning. These both depend on the actions of others (through the learning spillover and health spillover effects). In the presence of spillover effects, the returns to own

experimentation in period 1 are lower, the larger the share of the population that experiments in period 1, leading to free-riding. The larger the spillover effects, the higher the incentive to free-ride, thus the closer the period 1 demand function is to the myopic demand function, and the higher the potential extensive margin effect of the subsidy in period 1. The option value of learning also depends on households' period 1 beliefs with respect to the period 2 price, and this relationship is non-linear.

2.6 Model Summary, and Predictions for Five Types of Preventative Health Products

The goal of the model was to identify the key parameters that determine the sign and the magnitude of the impact of one-off subsidies on subsequent willingness to pay. We are now ready to list these parameters, and compare them across products.

As highlighted in equation (8), the key paramaters affecting the sign of the intensive margin effect are: the gap between the prior on the product's quality and its true quality ($q_{h1} - q_h$); the magnitude of the effectiveness of the product at increasing available income (e_h) within the first period; how easy it is to learn about the quality of the product through own experimentation(λ_{qo}) or from others (λ_{qs}); and the magnitude of the health externality ($s(S)$).

What's more, the magnitude of the effect depends on the steepness of the demand function, which itself depends on the magnitudes of the learning and health spillovers (e.g., incentives to free-ride), the priors regarding the quality-of-life parameter q, and the level of poverty.

By comparing these parameters across health products and contexts, one can make some predictions about when short-run subsidies may or may not boost long-run adoption. In what follows, we discuss the likely impacts of short-run subsidies on the adoption of three types of preventative health products for which subsidy programs have been studied: water disinfectants (subsidized in Ashraf et al. (2010), deworming medicine (subsidized in Kremer and Miguel (2007), and Olyset bednets (subsidized in the experiment described below). We also consider two technologies for which subsidies have been discussed but not yet studied empirically: improved cooking stoves ("clean" stoves, which reduce indoor air pollution) and water filters. The table below summarizes the predictions for a high poverty population.

The first difference between these products concerns the level and accuracy of priors on quality. Both water disinfectants and deworming pills have important negative side effects (water disinfectant makes the water tastes like chlorine, while deworming treatment makes children nauseous for a few days). It is unlikely that households without prior exposure to these products would anticipate such side effects, and therefore their priors on the quality of the product are likely overestimates. Olyset bednets, on the other hand, are much more comfortable than earlier generations of bednets. To the extent that people will assume all nets are equally comfortable, people's priors on the quality of Olysets are therefore likely to be underestimates when they are first introduced. With regards to cookstoves, households are likely to overestimate how difficult it is to adapt one's cooking to the new stove. This means that one-off subsidies for Olysets and cookstoves would trigger a positive

learning effect, whereas one-off subsidies for water disinfectant and deworming pill would trigger a negative learning effect. For water filters, the learning effect is likely to be zero, since uncertainty about the quality-of-life impact of using a water filter is likely negligible.

The second difference is in the strength of the income effect, which depends on the length of a "period" : a bottle of water disinfectant lasts only about 1 month for a standard household, whereas deworming treatment needs to be repeated only every 6 months, and bednets, cookstove and water filters have a lifespan of multiple years. When the household that got a free sample of water disinfectant needs to make a repurchase decision a month later, the household is unlikely to have experienced an increase in income. In contrast, by the time a bednet, filter or cookstove needs to be replaced a few years later, households will have had ample time to observe its impact on health and income. For deworming, the income effect is likely to be very modest despite the relatively long duration of a "period". That is because deworming is done in children, whose income-generating activity is very limited; and parents do not typically spend income on treating the symptoms on deworming, therefore deworming does not lead to important averted medical expenditures.

On the third dimension (how easy it is to learn about the quality-of-life impacts), the products are indistinguishable. For all five, learning how comfortable or uncomfortable the product is appears quite immediate. For example, one only needs to put water disinfectant in one's bottle once to discover the chlorinated taste; the side effects of taking a deworming pill are not subtle; one only needs to sleep under a bednet a few times to discover how much air can flow through it.

The fourth dimension concerns the magnitude of the health externality. The health externality is low for water disinfectants, water filters and cookstoves, high for deworming, and potentially high for bednets, although the evidence on this is limited and mixed (Hawley et al., 2003; Killen et al. 2007; Tarozzi et al., 2011).

Putting all this together, we make the following conjectures:

- A one-time subsidy for water disinfectant is unlikely to have a meaningful impact on subsequent adoption: it will have a possibly negative learning effect and only a small (if any) income effect. This is in line with the empirical evidence to date: Ashraf et al. (2011) find that Zambian households who are enticed to buy one bottle of disinfectant when it is subsidized end up not using it to purify their water, suggesting a potentially negative learning effect (they were put off by the chlorinated taste) or no learning at all (they did not even try it). Dupas et al. (2011) look at the long-run impact of giving just 1 free bottle of water disinfectant to mothers of young children in Kenya, and find no effect whatsoever on the probability that households use water disinfectant two years later. For such products, a longer subsidy (repeated free trials) might be necessary to boost adoption in the long-run.

- A one-time subsidy for deworming is likely to *dampen* subsequent adoption: it will have a negative learning effect and no income effect, and given the large externality the incentive to free-ride will be high. This is also in line with the evidence to date: Kremer and Miguel (2007) observe lower adoption rates of deworming treatment among households who have

more social contacts that received a deworming subsidy. For such products, a permanent subsidy might be the only way to ensure widespread adoption.

- One-time subsidies for Olyset bednets, cookstoves and water filters have the potential to boost subsequent adoption through both learning and income effects, but the overall impact will depend on the importance of anchoring. This is what the Olyset bednets experiment conducted in Kenya was set to test. The next two sections describe this experiment and its results.

Summary of Predictions for Five Preventative Health Products

Product	Water Disinfectant	Deworming Treatment	Olyset Bednet	Improved Cookstove	Water Filter
Prior on quality ($\tilde{q}_{h1}$)	High	High	Low	Low	High
Error in Prior on quality ($\tilde{q}_{h1} - q_h$)	+	+	−	−	0
Strength of Income effect within period 1 (e_h)	Low	Low	High	High	High
Arrival rate of information on quality (λ_{qo} and λ_{qs})	High	High	High	High	High
Strength of Health Externality $s(S)$	Low	High	Unclear	0	Low
Learning Effect	−	−	+	+	0
Income Effect	0	0	+	+	+
Sign of Intensive Margin Effect	−	−	+	+	+
Steepness of demand function	Low	High	High	High	High
Predicted Period 2 Effect of Period 1 Subsidy *(if no anchoring)*	−	−−	++	++	+

3 The Bednet Subsidy Experiment: Design

The remainder of the paper presents a randomized experiment conducted in Western Kenya over two phases, Phase 1 and Phase 2, which correspond to the two periods in the model. In Phase 1, households were randomly assigned a low or high subsidy for a long-lasting insecticide-treated bednet. In Phase 2, households were given a second subsidy, but this time the subsidy was low for everyone. This set-up enables us to estimate how the subsidy level faced in Phase 1 affects willingness to pay in Phase 2.

3.1 Background on Insecticide-Treated Bednets

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

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

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

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 enumeration areas. Participating households were sampled as follows. In each area, the school register was used to create a list of households with children.[10] Listed households were then randomly assigned to a subsidy level for an LLIN. The subsidy level varied from 100% to 40%; the corresponding final prices faced by households ranged from 0 to 250 Ksh, or at the prevailing exchange rate of 65 Ksh to US$1 at the time, from 0 to US$3.8.[11] Seventeen different prices were offered in total, but each area, depending on its size, was assigned only four or five of these 17 prices. Thus, if an area was assigned the price set {Ksh 50, 100, 150, 200, 250}, all the study households in the area were randomly assigned to one of these five prices according to a computer-generated random number. All price sets included high, intermediate, and low subsidy levels. However, the lowest price offered in a given area was randomly varied across areas, and drawn from the following set: {0, 40, 50, 70}. Only two areas had a price set that included free distribution for some households.

After the random assignment to subsidy levels had been performed in office, trained enumerators visited each sampled household. A baseline survey was administered to the female and/or male head of each consenting household.[12] At the end of the interview, the respondent was given a discount voucher for an LLIN corresponding to the randomly assigned subsidy level. The voucher indicated (1) its expiration date, (2) where it could be redeemed, (3) the final (post-discount) price to be paid to the retailer for the net, and (4) the recommended retail price and the amount discounted from the recommended retail price.[13] 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 nets subsidized in 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

[10] 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.

[11] 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).

[12] 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. All regressions below include controls for the randomized gender assignment.

[13] 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.

2 weeks.[14]

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 net. The follow-up survey also checked whether households had been charged the assigned price for the net. Usage was assessed as follows: (1) whether the respondent declared having started using the net, and (2) whether the net was observed hanging above the bedding at the time of the visit.

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.[15] 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 Phase 1.

By comparing the take-up rate of the second, uniformly-priced voucher across Phase 1 price groups, we can test whether being exposed to a high subsidy dampens or enhances willingness to pay for the product a year later. Note, however, that since 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.

3.4 Verifying Randomization

A baseline survey was administered at households' homes between April and October 2007, prior to the first voucher distribution. The baseline survey assessed household demographics, socioeconomic status, and bednet ownership and coverage. Table 1 presents summary statistics on 14 household characteristics, and their correlation with the randomized Phase 1 price assignment. Specifically, we regress each baseline characteristic on a quadratic in the price faced in Phase 1 and a set of area

[14] 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.

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

fixed effects:

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

where x_{hj} represents a baseline characteristic of household h in area j and P_{hj1} is the price faced by household h in Phase 1. We report the coefficient estimates and standard errors for τ_1 (column 3) and τ_2 (column 4). All of the coefficient estimates are small in magnitude and none can be statistically distinguished from zero, suggesting that the randomization was successful at making the price assignment orthogonal to observable baseline characteristics.[16]

3.5 Verifying Compliance with Study Protocol

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

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

To check for potential arbitrage after redemption (i.e., people selling the LLIN to their neighbor after having redeemed the voucher), we conducted unannounced home visits and asked to see the LLIN that had been purchased with the voucher (as mentioned above, the study-provided nets were easily recognizable). These home visits were conducted after both Phase 1 and Phase 2. Overall, more than 90% of households that had redeemed a voucher could show the LLIN during the spot check.

4 The Bednet Subsidy Experiment: Results

4.1 Impact of Phase 1 Subsidy on Phase 1 Adoption

As the model highlighted, the magnitude of the impact of the Phase 1 subsidy on the long-run adoption rate will depend on how much the subsidy affected the adoption rate in Phase 1 (the extensive margin effect). This is presented in Figure 1. Panel A shows that the demand function is quite steep: take-up is quasi-universal for free LLIN vouchers (at 97.5%), but drops to 70% and then 55% when the price goes to 40 Ksh ($0.6) and and 90 Ksh ($1.4), and further drops to around 30% when the price crosses the 100 Ksh threshold ($1.5). In contrast, Panel B, which shows usage rates (among those who redeemed their voucher), suggests that the likelihood that people used the LLIN is independent of the price paid.[17] As a result, as shown in Panel C, the adoption rate

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

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

(take-up × usage) drops substantially as the price increases (as the subsidy level decreases): after 12 months, adoption is above 90% under the full subsidy regime, just above 60% at the 50 Ksh price point, and lower than 10% when the price is 250 Ksh.

The regression results for Phase 1 adoption are presented in Table 2. They show that the results are virtually independent of the specification chosen (linear, logistic or probit) and are robust to household-level controls (see Panel B).[18]

The result that initial adoption is very sensitive to price is consistent with the result obtained among pregnant women by Cohen and Dupas (2010), in a separate study also in Western Kenya. It is also consistent with the results in Tarozzi et al. (2011), which find that bednet adoption in Orissa (India) decreases from 51% to 22% when the price increases from free to full but with the option to buy on credit. Compared to the two other products discussed in the model, our adoption function is not as steep as that observed in Kremer and Miguel (2007), which finds that increasing the price of deworming from 0 to 20 Ksh decreases adoption from 75% to 20%; but it is much steeper than the essentially flat adoption function observed in Ashraf et al. (2011) concerning water disinfectant: they find that increasing the price of the water disinfectant from 300 to 800 Zambian Kwacha leaves the adoption rate (take-up × usage) unaffected, at roughly 30%.

4.2 Impact of Phase 1 Subsidy on Phase 2 Willingness to Pay

The large extensive margin effect of the period 1 subsidy on period 1 adoption suggests a large potential for the period 1 subsidy to affect period 2 adoption through learning and income effects. This section tests whether households who benefited from a highly subsidized LLIN in Phase 1 were more or less willing to pay for a LLIN in Phase 2.

Recall that the price of the second LLIN was uniform across all households (at 150 Ksh). Figure 2 presents the average purchase rate for the second LLIN, for each Phase 1 price group. The confidence intervals are large, but the average take-up was higher among the higher subsidy groups (free and 40-50 Ksh price groups). The regression analysis presented in Table 3 confirms this result. We show results for three estimation methods (OLS, Logistic and Probit) and four possible period-1 price specifications, either without (Panel A) or with (Panel B) household-level controls. The period-1 price specifications we use are: (1) linear in price; (2) quadratic in price; (3) a dummy for having received a high subsidy (price of 50 Ksh or below); and (4) two dummies, one for having received a full subsidy ("Free") and one for having a high-but-not-full subsidy (price of 40 or 50 Ksh).

In both the linear and quadratic specifications, the estimated effect of phase-1 price on period-2 adoption is negative, but it is not significantly different from zero. We can however reject any meaningful *positive* effect, meaning that we can reject that lower phase-1 prices *dampen* phase-2 adoption. This suggests that anchoring or entitlement effects are not a first order concern.

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

Columns 3, 7 and 11 of Table 3 present the specification with the "high subsidy" dummy (1st LLIN price $\leq$ 50 Ksh). As was apparent in Figure 2, high-subsidy recipients in Phase 1 had a higher redemption rate in Phase 2 than those who received a lower subsidy. The effect of having received a high subsidy in Phase 1 is significant at the 10 percent level (the p-value is 0.06), both without and with household level controls. When we look separately at the effect of getting the full subsidy and the high-but-not-full subsidy (columns 4, 8 and 12), we find very similar point estimates for both subsidy groups, and comparable to the previous specification, but given the relative small sample size the standard errors increase and we cannot reject the null (though the 95% confidence intervals are [$-.024$; $+.164$] and [$-.037$; $+.187$] and we can reject any negative impact of more than a few percentage points).

Overall, the evidence points to a positive effect of a high Phase 1 subsidy on Phase 2 adoption, but the effect is not overly strong, and only at the margin of significance. Note, however, that the take-up of the second LLIN voucher among high-subsidy recipients reflects mostly 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 would 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 high-subsidy group than in the low-subsidy group is by itself suggestive that the willingness to pay in the high-subsidy group may have increased. Follow-up data on the usage of the LLIN obtained with the second voucher suggests that the second LLIN had indeed lower immediate returns for households: the LLIN acquired with the second voucher was 23% more likely to still be in its package at the time of the follow-up visit two to four months later. Respondents who had their LLIN in its package reported storing it for the future. As long as the discount factor is less than 1, this implies lower returns (everything else constant) to the second LLIN.

All in all, the results so far provide strong evidence that a one-time subsidy for the Olyset LLIN *did not dampen* future willingness to pay. This means that potential negative anchoring or entitlement effects of subsidies were at best limited in scope. Our results also provide some suggestive evidence that a one-time subsidy for the Olyset *boosted* future willingness to pay. As discussed in the theory section, this could be driven by two effects: (1) a positive learning effect; and (2) an income (via health) effect. Our experimental data does not enable us to estimate the relative importance of these two mechanisms, but we have some suggestive evidence that both mechanisms were at play.

Learning effect: At both follow-ups (after 2 months and after 1 year), households who had purchased the first LLIN were asked: "In your opinion, how does this Olyset net compare to other nets you may have had in the past?" The great majority (88% at the 2-month follow-up and 90% at the 1-year follow-up) said that the Olyset was better.[19] At the 2-month follow-up, the main

[19] At the 2-month follow-up, the rate was 96% among those who had started using the Olyset, and 70% among

(non-exclusive) reasons given for why the Olyset concerned the comfort level (37%), the sturdiness (40%) and the health effectiveness (26%). At the 1-year follow-up, the same share of respondents mentioned comfort and sturdiness, but the share mentioning health effectiveness had risen to 40%. To bring this in the terms of the model, this suggests that households learned about the quality of life impact Q of the bednet relatively quickly (within two months), while they learned about the effectiveness, e, over the course of a year.

Income effect: With respect to the potential income effect, the 1-year follow-up survey data suggests that the incidence of malaria among household heads (either the male or the female) may have been lower among households who received a high LLIN subsidy in Phase 1 (Appendix Table A2).[20] 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 however have data on income to directly test for an income effect.[21]

Before concluding this section, we estimate the effect of the "treatment on the treated": the effect of experimenting with a LLIN in period 1 on period 2 adoption (Table 4). This essentially consists in rescaling the reduced form results presented in Table 3 by the compliance rate. Specifically, we regress adoption in period 2 on whether the household adopted the LLIN in Phase 1, instrumented with either a polynomial in price or a dummy for having received a high subsidy in Phase 1. Thus, columns 2 and 3 of Table 2 correspond to the first stage estimations for columns 1 and 2 of Table 4. These instrumental variable specifications measure the effect "on the treated", that is the effect of having experimented with the first LLIN for those who took up the subsidy. In the terminology of Angrist and Imbens (1996), this effect is valid for the *compliers* – which make about 27% of the population according to the first stage estimate.

The estimated effect is very large in magnitude (essentially a 100% increase in take-up of the second LLIN) but imprecisely estimated. Only the specification which uses the dummy for "high subsidy" as an instrument reaches the 10% significance threshold. Note, however, that the exclusion restriction for the instrument (the Phase 1 subsidy affects willingness to pay for the second LLIN only through the learning and income effects) does not hold in the presence of anchoring effects. Thus our preferred specifications are the reduced form specifications presented in Table 3.

4.3 Spillover Effects

This section tests whether information about the attributes of the Olyset spread through social networks. Here again, we adopt a revealed preferences approach to perform this test: we test whether a given household's LLIN purchase behavior was affected by the adoption behavior of

those who had purchased it but not yet started using it.

[20] None of the coefficient estimates in Table A2 are significant given the small sample size, but they all negative and large compared to the mean, suggesting a decrease in malaria incidence of about 20%. Such an effect is within the range of effects estimated in the medical literature (Lengeler, 2004), although a recent randomized experiment in Orissa, India, suggests that when the coverage rate in the overall population is low, the private returns to ITN use may be much smaller than estimated in medical trials (Tarozzi et al., 2011).

[21] We did not attempt to measure income as income is notoriously difficult to measure among the rural self-employed (many of them farmers), who make the great majority of our sample.

their neighbors.[22]

Measuring Exposure Given the large differences in LLIN take-up across price groups, the random assignment of households to price groups in Phase 1 generates an exogenous source of geographic variation in the density of households that had a chance to experiment with an LLIN. As shown in Figure 1, households randomly assigned to a low price (high subsidy) were much more likely to buy an LLIN in Phase 1 than households assigned a high price. The time needed for households to acquire the LLIN was also much lower when the subsidy was higher. Appendix Figure 1 shows that households that received a voucher for a free LLIN typically redeemed it within a few days. In contrast, those who were assigned a high price were very unlikely to redeem their voucher, and if they did, they took two months to redeem it. All in all, across neighborhoods within a given village, the "exposure" to LLINs varied with the share of households that received a high subsidy level. Since this share was exogenously determined by the random assignment, we can exploit this variation to estimate social effects without running into the reflection problem identified by Manski (1993).

Using GIS coordinates, we compute, for each household in the sample, the number of sampled households that live within a given radius, and the number and share of them who received a voucher for a given subsidy level. In particular, we compute the share of households within a given radius who received the maximum subsidy offered in the area (i.e., the share of households who received a voucher for a free LLIN in the two areas where the subsidy reached 100%; the share of households who received a voucher for an LLIN at 40 Ksh in the area where the lowest price was 40Ksh; etc.). We use three different radii to define the neighborhood: 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 1.28 neighbors within a 250m radius (4.01 neighbors within 500m, 7.77 within 750m) who received the maximum possible subsidy level offered in the area. This represents, at the mean, 24-26% of the study households living within these radii.[23]

Results Figure 3 plots the coefficients of OLS regressions, where the dependent variable is whether a given household purchased the LLIN in Phase 1 and the independent variables are dummies for each exposure group, with exposure being defined as the share (panel A) or the number (panel B) of study households within a 500m radius of the given household who received the

[22] We use neighbors as proxies for social contacts as we did not map out social networks in the areas of studies. To the extent that our measure of social networks is noisy, this will bias our results downwards. Note however that neighbors are a very important part of social networks in rural Western Kenya. Data collected by Dupas et al. (2011) in the same area of study shows that 68% of women in rural households speak to at least four neighbors daily, and 91% speak to at least four neighbors a few times a week.

[23] Panel B of Table A3 tests whether these density measures are correlated with the voucher price. E.g. 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. None of the exposure measures have statistically significant coefficients in the price regressions, and all the coefficient estimates are small (e.g., a household with 100% of sampled neighbors within 250m 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).

maximum subsidy offered in the area in Phase 1. Both specifications show take-up of the Phase 1 LLIN increasing as exposure to the product via neighbors increases (solid line). Figure 3 also presents results for how Phase 2 adoption was affected by neighbors' Phase 1 price (dashed line). These results suggest 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.

To confirm these results and test how sensitive they are to the choice of the radius, Table 5 reports regression estimates.

We consider three outcomes: (1) whether the household bought the first LLIN (redeemed their first voucher); (2) whether the household bought at least one LLIN over the two periods; and (3) whether the household bought both LLINs. The first outcome is available for the full sample, but the other two outcomes are only available for the subset of households in the four areas where Phase 2 was implemented. Therefore we report the results for the first outcome (buying the first LLIN) for both the full sample and the subsample observed in both phases, to enable comparisons across outcomes within a fixed sample.

For each of the three radii (250m, 500m, and 750m), we start by running the following specification (Panel A):

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

where $Y_{hj1}$is the outcome of interest and $P_{hj1}$is the period 1 price faced by household h in area j and υ_j is an area fixed effect. The regressor of interest is $ShareMax_{j1}$, the share of neighbors (within a given radius) who received the maximum subsidy offered in area j in Phase 1. (We impute this share to be zero if there are no other study households in this radius). Since the density measures may be spatially correlated, we present standard errors corrected for spatial dependence in brackets, in addition to presenting the White standard errors in parentheses. We use the spacial dependence correction proposed by Conley (1999).[24]

In Panel B, we run a similar regression with an added control variable, the total number of study households within 500 meters. This is to account for the fact that people living in less densely populated areas (and who have zero neighbors who got the subsidy because they have zero neighbors) may be less likely to adopt new products.[25] Panel C reports results of an analysis similar to Panel B, but with the *number* of neighbors within the radius who received the high subsidy, rather than the share.[26]

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

[25]Since the study sample is not a random sample of the population but instead the subset of households who have children in school, and those might not be randomly located, the local density of households *in our sample* is only an imperfect proxy for the local population density.

[26]In Appendix Table A4, we report results from alternate specifications that include the full distribution of prices around the household, rather than just the share or number with the maximum subsidy level. The results are unchanged in substance. We also ran the Table 5 regressions with household-level controls. The results are also unchanged (table available upon request).

Finally, Panels D and E present IV estimates. The independent variable of interest in Panel D is the share of sampled households within a given radius who are using the LLIN. In Panel E, it is the share of sampled households that redeemed their voucher within a week. To overcome the obvious endogeneity issue, we use the share of sampled households within that radius who received the maximum subsidy level as an instrument. 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 (Panel D) or redeemed their voucher within a week (Panel E), is instrumented by $ShareMax_{j1}$.

For each outcome of interest, the results in Table 5 are overall relatively consistent, quantitatively, across all five panels, all three radius choices, and the two standard error formulas. The estimated social effects depend on which outcome we consider, however.

As could be seen on Figure 3, the results for "Bought first LLIN" suggest strong social effects within the first phase: the higher the proportion of neighbors who received the maximum subsidy, the more likely the household is to have redeemed the voucher and purchased the LLIN. When looking at the results using the 'within 250m 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 14.1 percentage points. This implies that households are just about 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 in Phase 1 was 120 Ksh ($1.85), a relatively large sum for rural households in the areas of study. The point estimate is somewhat larger for the subsample observed in both periods, but the standard errors are large and we cannot reject that the estimated social effect is the same in the full sample and the subsample. The IV estimates in Panels D and E confirm that these spillovers come through subsidized neighbors' early experimentation with the product.

Turning to the second outcome, "Bought at least one LLIN", we find a spillover effect of about the same magnitude as for "Bought first LLIN", but the standard errors are larger and the effects are not quite significant at conventional levels.

More surprising are the the results for the last outcome, "Bought two LLINs", which contrast somewhat with the earlier results. Exposure to highly subsidized neighbors within 250m does not affect the likelihood that a household invested in two LLINs. But exposure to highly subsidized neighbors within 500 or 750 *lowered* the probability that the household invested in two nets. Overall, exposure through neighbors thus increased the likelihood that households bought at least one LLIN, but decreased the likelihood that households bought two. This result is consistent with people reacting to the health spillovers over time: people with more neighbors using an LLIN get convinced to invest in one themselves, but as the malaria transmission rate decreases over the course of the year in areas with higher LLIN coverage, the need to invest in a second LLIN decreases in those areas. Future research is needed to better understand the magnitude of these health spillover effects and how households learn about them.

5 Conclusion

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

This paper used a randomized field experiment to estimate the effect of a one-time, targeted subsidy on the long-run adoption of a new health product that is both more effective and higher quality than its predecessor (the long-lasting antimalarial bednet Olyset). We find that temporary subsidies for a subset of households considerably increase short-run adoption rates among both subsidy recipients and their neighbors, and subsequently increase willingness to pay for bednets through income and learning-by-doing effects that appear to trump any potential anchoring effect.

We contrast our findings with those of two previous randomized studies that found opposite results for two other health technologies. We argue that all three sets of results are consistent with a simple model of technology adoption in which households are initially uncertain about both the health effectiveness of a new technology and how using the technology affects one's quality of life. In such a model, the effect of subsidies on learning and adoption depends on a number of key parameters, including whether households initially overestimate or underestimate the product quality, the presence of spillovers, and how high and quickly observable the private returns to adoption are.

The extent to which the adoption of new products is affected through "free trial" periods and how it diffuses through neighbors or friends is a central question, especially for less developed economies where modern diffusion channels, such as TV commercials, do not reach the great majority of the population. The empirical evidence provided in this paper suggests that, at least for some class of preventative health products, learning by doing and social learning are important channels through which short-term, targeted subsidies can translate into sustained levels of adoption. This provides a rationale for subsidies even for technologies that do not generate positive externalities. The theoretical framework we propose provides some guidance as to when to expect this rationale to apply.

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Model Appendix: Forward-looking Case

If the household is fully forward-looking, then in period 1 the household's problem is to decide whether to purchase the product given that they know how they will behave in period 2, they have a prior about the period 2 price $\tilde{p}_2$, they know the learning probabilities if they experiment with the product in period 1, and they have a prior about the income effect $\tilde{e}_h$. The household has no information, however, about the period 2 value of the idiosyncratic unobservable shock (ϵ_{h2}) beyond its distribution.

Recall our assumption that the intertemporal utility function is log-linear:

$$\begin{aligned} W &= U_1 U_2^{\beta} \\ w &= u_1 + \beta u_2 \end{aligned}$$

The expected indirect intertemporal utility from adopting product j in period 1 is:

$$w_h^j = \alpha \log(y_{h1} - p_1) + \bar{q}_1 + \epsilon_{h1}^j + \beta \mathbb{E}\left(\max\{\alpha \log(y_{h2} - \tilde{p}_2) + \tilde{q}_2 + \epsilon_{h2}^j, \alpha \log(y_{h2}) + \epsilon_{h2}^0\} | a_1 = 1\right)$$

The expected indirect intertemporal utility from not adopting in period 1 is:

$$w_h^0 = \alpha \log(y_{h1}) + \epsilon_{h1}^{j0} + \beta \mathbb{E}\left(\max\{\alpha \log(y_{h2} - \tilde{p}_2) + \tilde{q}_2 + \epsilon_{h2}^j, \alpha \log(y_{h2}) + \epsilon_{h2}^0\} | a_1 = 0\right)$$

The household adopts in period 1 if:

$$\begin{aligned} w_h^j &\geq w_h^0 \\ \Leftrightarrow \epsilon_{h1}^{j0} - \epsilon_{h1}^j &\leq \alpha \log(\frac{y_{h1} - p_1}{y_{h1}}) + \bar{q}_1 + \beta \Delta \end{aligned} \tag{11}$$

where $\Delta = \mathbb{E}\left(\max\{\alpha \log(y_{h2} - \tilde{p}_2) + \tilde{q}_2 + \epsilon_{h2}^j, \alpha \log(y_{h2}) + \epsilon_{h2}^0\} | a_1 = 1\right) -$ $\mathbb{E}\left(\max\{\alpha \log(y_{h2} - \tilde{p}_2) + \tilde{q}_2 + \epsilon_{h2}^j, \alpha \log(y_{h2}) + \epsilon_{h2}^0\} | a_1 = 0\right)$ is the expected *utility gain* from making the period 2 decision with period 1 product experience (rather than with no experience). This option value of experimentation is composed of two additive effects: the expected income effect and the option value of learning about the attributes of the product and therefore making a better-informed repurchase decision in period 2. There is no simple analytical expression for Δ, but what is critical to observe here is that Δ is non-negative: having experimented with the product in period 1 cannot make period 2's utility worse in anyway. It can only increase it, by improving the information set and thereby enabling better-informed decisions; and by expanding the budget set, if the product is effective at improving health and thus available income.

Given that Δ is non-negative, expression (11) makes clear that allowing households to be forward-looking makes the demand function in period 1 *flatter*. This means that the potential for subsidies to increase adoption in period 1 will be reduced: the extensive margin effect of a period 1 subsidy will be dampened. By how much it will be dampened depends on the size of Δ.

Intuitively, if the option value of experimentation is extremely high, many people will experiment even if the period 1 price is high, and a subsidy is less needed to entice people to experiment, and vice versa. We thus now turn to discussing what affects the size of Δ. We discuss its two components in turms, the income component (we call it Δ_1) and the option value of learning component (Δ_2).

The magnitude of $\Delta_1$depends on the anticipated income effect, that is, on the beliefs about the health effectiveness parameter e. People with a higher belief on e will be more likely to adopt in period 1.

The magnitude of the option value of learning (Δ_2) depends on households' beliefs with respect to the period 2 price ($\tilde{p}_2$), but this effect is likely to be non-linear. Indeed, if people expect the price in period 2 to be outside their budget set, then the option value of learning is zero. On the other hand, if people expect the price in period 2 to be free, then the option value of learning is also zero. At intermediate period 2 price beliefs, the option value of learning will be higher.

What's more, both $\Delta_1$and Δ_2 depend on the actions of others, since $\tilde{y}_{h2}$ and $\tilde{q}_{h2}$ are affected through the health spillover and learning spillover effects, respectively. This means that the option value of own experimentation in period 1 is lower, the larger the share of the population that experiments in period 1. Households are therefore playing a game with each other, and this will lead to free-riding. Proving the existence of an equilibrium is outside the scope of this paper, but we can apply the following theoretical results of Bolton and Harris (1999): (1) Given our discrete-time set-up, there is no "encouragement effect" (whereby one household would experiment in order to bring forward the time other households experiment and generate information about the product's attributes), and no symmetric equilibrium in pure strategies; (2) Because of free-riding, the level of experimentation is lower than socially optimum.

Free-riding makes Δ smaller, and thus limits how flat the demand function becomes when we allow households to be forward-looking. Thus, the larger the spillover effects, the closer the period 1 demand function is to the myopic demand function, and the higher the potential extensive margin effect of the subsidy in period 1. This extensive margin effect is multiplied by the intensive margin effect, which is unaffected compared to the myopic case, and of ambiguous sign.

Forward-looking Case with Reference-Dependence Allowing fully forward-looking agents to exhibit reference-dependence is somewhat innocuous since agents who anticipate they will anchor should deliberately choose anchor points that are very high, to make sure they never suffer from "losses". It is conceivable, however, that forward-looking households are naïve about their reference-dependence. That is, they may assume the period 2 price will be equal to the period 1 price, without considering the possibility that their belief about the period 2 price might be wrong and the actual period 2 price might be greater than anticipated; but when period 2 arrives, they exhibit reference-dependence and suffer a utility loss if the period 2 price is greater than the period 1 price.

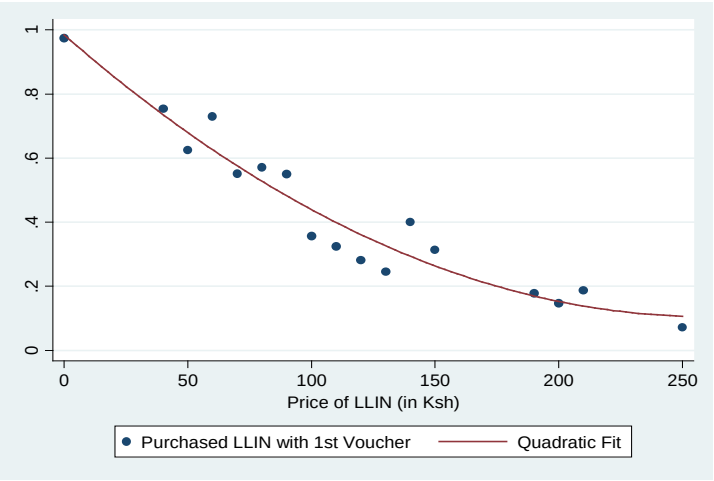
In such a scenario, the period 1 price affects period 2 adoption in two ways with possibly opposite sign:

- Period 1 extensive margin effect: A lower period 1 price (a higher period 1 subsidy) means lower expected period 2 price, and that may increase the option value of experimentation, since the product will still be affordable in period 2 if it turns out to be of high quality and effective. That means the demand function in period 1 may become steeper, and therefore the extensive margin effect of the period 1 subsidy may become larger. As discussed above, this will not be the case if the anticipated period 2 price is 0.

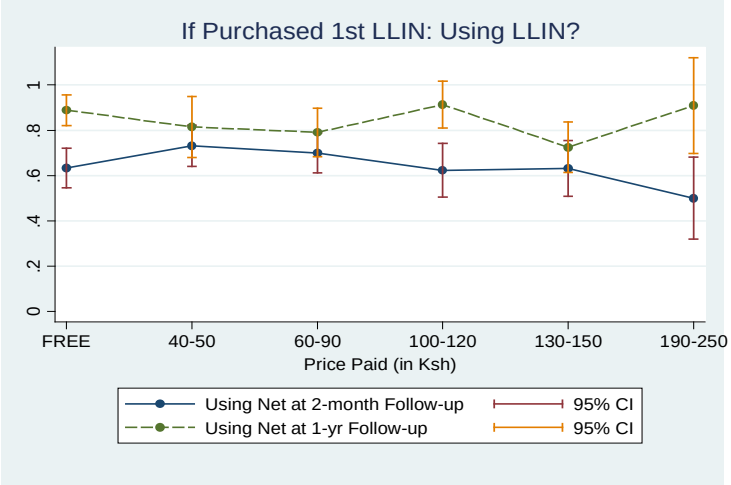
- Direct period 2 anchoring effect: as in the myopic case, a lower period 1 price means that willingness to pay in period 2, holding everything else (beliefs, income) constant, will be lower, because of the perceived "loss" from paying higher than anticipated.

Figure 1

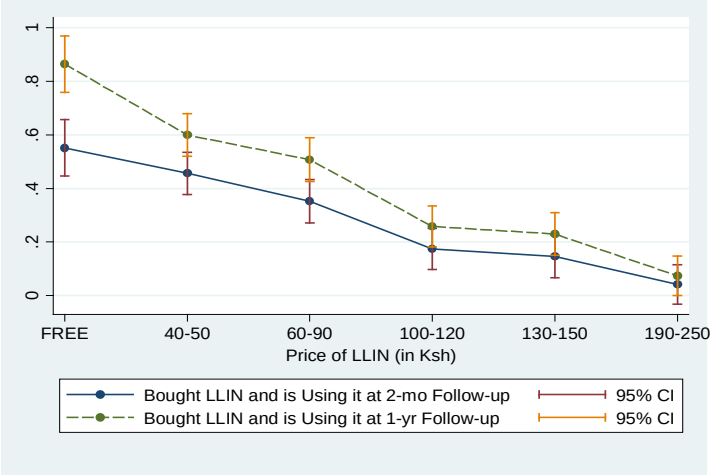
Panel A: Share of study households that purchased the LLIN in Phase 1



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



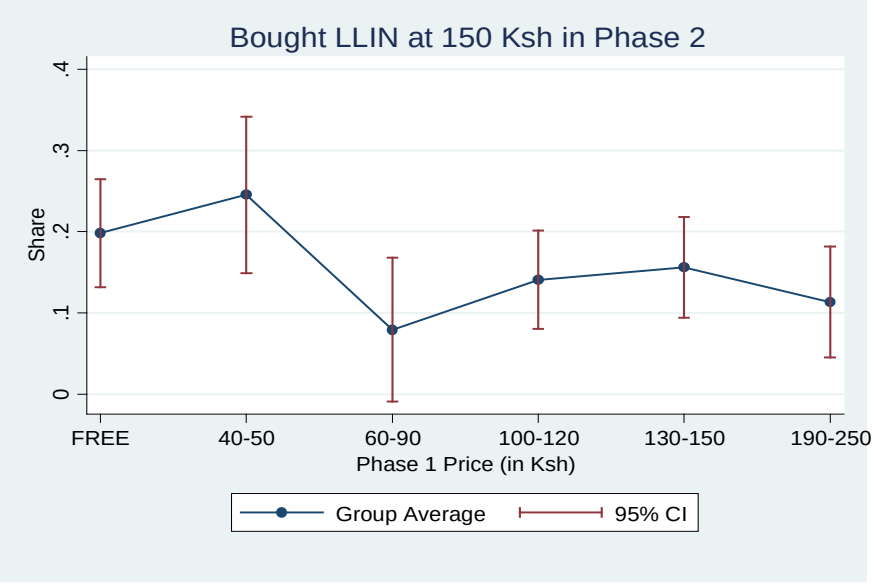
Panel C: Phase 1 Demand Function: Adoption of 1st LLIN, by Phase 1 LLIN price



Notes: Data from 1,120 households (Panels A and C), 479 households (Panel B, solid line), 273 households (Panel B, dashed line). The second follow-up was conducted in only 4 of the 6 study areas. Usage is self-reported (see Table 2 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
Phase 2 Take-up as a function of Phase 1 price

Panel A. Means by Subsidy Group

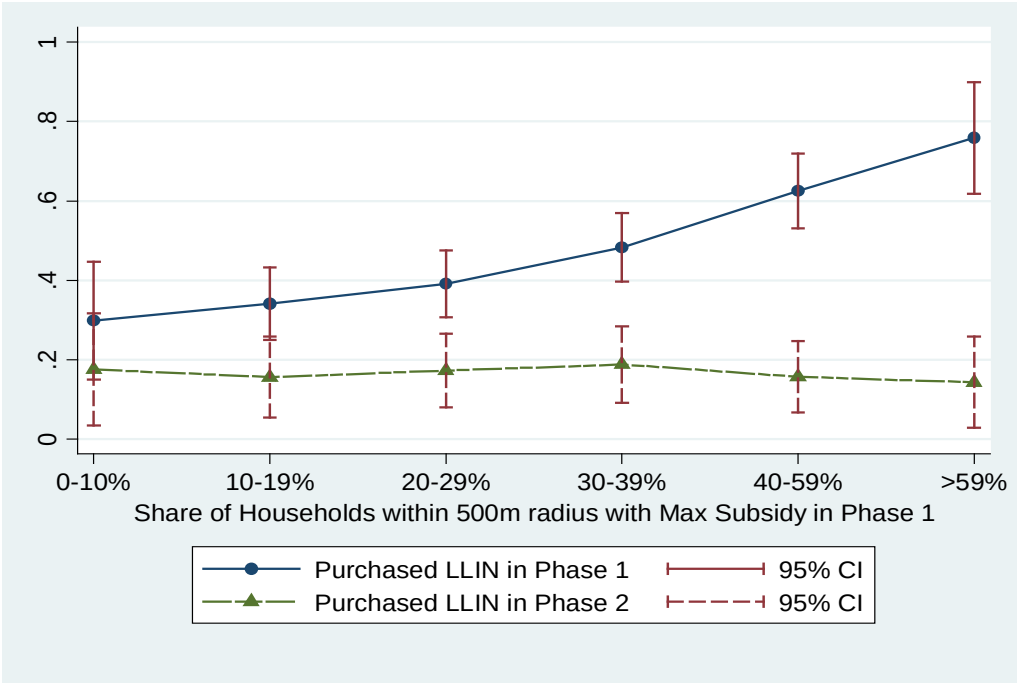


Panel B. Non-Parametric Estimation of the Relationship between Phase 1 price and Phase 2 Adoption

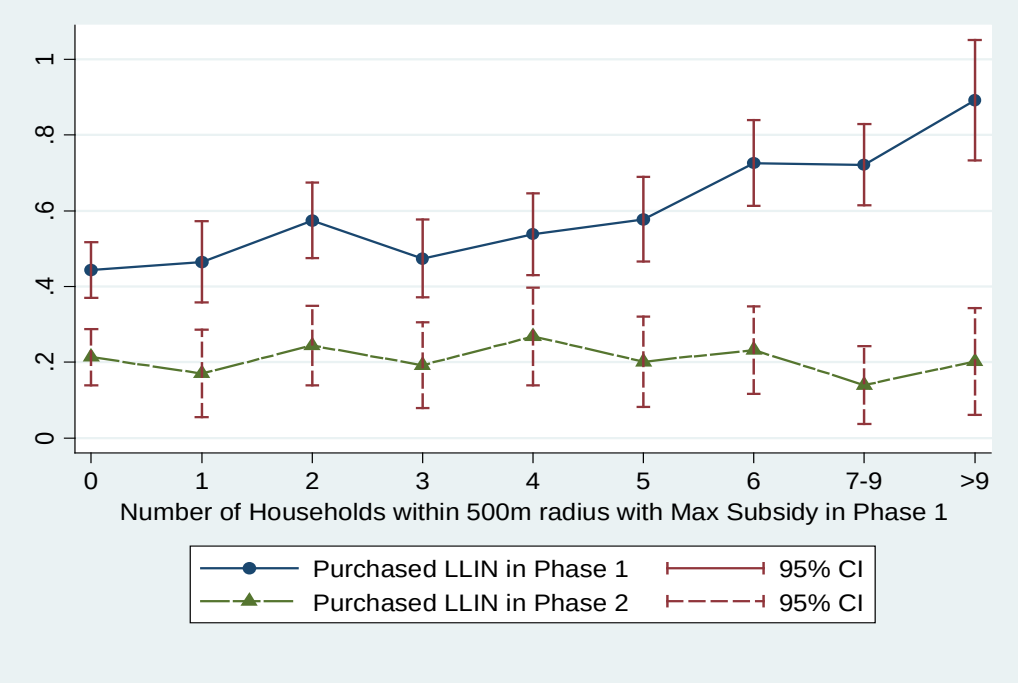
Notes: Both panels based on data from all households (whether or not they redeemed their 1st LLIN voucher) in 4 areas (599 households total). Panel A: OLS regression on dummies for each price groups. Panel B: Kernel-weighted local polynomial smoothing.

Figure 3
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 1094 households for whom accurate GIS coordinates could be collected. Each graph plots the coefficients and confidence intervals obtained through OLS regressions. The dependent variable is a dummy equal to 1 if the household purchased a LLIN in Phase 1 (solid line) or in Phase 2 (dashed line). The independent variables are dummies for each exposure "bin". These bins are based on 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 study 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	13.403	-1.28	0.035	0.032
			(1.375)	(.344)	
Number of children (under 18) currently living in household	5.447	2.852	-0.129	0.016	0.745
			(.291)	(.073)	
Socio-Economic Status					
Female head has completed primary school	0.248	0.432	0.030	-0.006	0.776
			(.044)	(.011)	
Number of household members with an income-generating activity	1.762	1.036	0.063	-0.034	0.063
			(.107)	(.027)	
Household assets index value (in US $)	338.227	324.965	25.991	-5.142	0.682
			(33.069)	(8.265)	
Electricity at home	0.019	0.136	0.010	-0.003	0.671
			(.014)	(.004)	
At least one member of HH has a bank account	0.12	0.325	0.000	0.003	0.603
			(.033)	(.008)	
Bednet Ownership at Baseline					
Number of bednets owned	1.738	1.51	-0.130	0.038	0.575
			(.154)	(.038)	
Share of HH members that slept under a net the previous night	0.408	0.368	-0.023	0.009	0.469
			(.038)	(.009)	
HH owns a circular PermaNet LLIN*	0.327	0.47	-0.036	0.016	0.733
			(.068)	(.023)	
HH ever received a free bednet	0.323	0.468	-0.026	0.003	0.681
			(.048)	(.012)	
Has ever shopped at shop where voucher has to be redeemed	0.623	0.485	0.052	-0.014	0.437
			(.045)	(.011)	
Distance from shop where voucher has to be redeemed (in km)	1.737	1.201	0.041	-0.009	0.929
			(.116)	(.029)	
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 Phase 1 price on Phase 1 adoption*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dep. Var.:	Bought and Used 1st LLIN ("Experimented" with 1st LLIN)											
Specification:	OLS				Logistic (Odds Ratios)				Probit (Marginal Effects)			
Panel A. No controls												
1st LLIN Price in US$	-0.155	-0.317			0.394	0.297			-0.161	-0.230		
	(0.014)***	(0.044)***			(0.036)***	(0.077)***			(0.013)***	(0.042)***		
(1st LLIN Price in US$) squared		0.043				1.090				0.020		
		(0.011)***				(0.080)				(0.012)*		
1st LLIN Price ≤ 50 Ksh (High Subsidy)			0.274				3.507				0.271	
			(0.032)***				(0.561)***				(0.036)***	
1st LLIN Price = 0 (Free)				0.437				7.188				0.437
				(0.053)***				(2.065)***				(0.057)***
1st LLIN Price = 40 or 50 Ksh (High Subsidy but not Free)				0.199				2.598				0.193
				(0.037)***				(0.485)***				(0.039)***
Observations	1120	1120	1120	1120	1120	1120	1120	1120	1120	1120	1120	1120
Panel B. With household-level controls												
1st LLIN Price in US$	-0.156	-0.317			0.388	0.293			-0.161	-0.228		
	(0.014)***	(0.044)***			(0.036)***	(0.077)***			(0.013)***	(0.042)***		
(1st LLIN Price in US$) squared		0.042				1.089				0.020		
		(0.011)***				(0.081)				(0.012)*		
1st LLIN Price ≤ 50 Ksh (High Subsidy)			0.275				3.576				0.271	
			(0.033)***				(0.584)***				(0.036)***	
1st LLIN Price = 0 (Free)				0.438				7.440				0.435
				(0.053)***				(2.164)***				(0.057)***
1st LLIN Price = 40 or 50 Ksh (High Subsidy but not Free)				0.199				2.628				0.192
				(0.038)***				(0.499)***				(0.039)***
Observations	1108	1108	1108	1108	1108	1108	1108	1108	1108	1108	1108	1108
Mean of Dep. Variable in non-"High Subsidy" group	0.265	0.265	0.265	0.265	0.265	0.265	0.265	0.265	0.265	0.265	0.265	0.265

*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. All regressions include enumeration area fixed effects. Price of 1st LLIN varies from 0 to US$3.8. Household-level controls in Panel B include all 14 variables presented in Table 1. Standard errors in parentheses. *Significant at 10%; ** significant at 5%; *** significant at 1%.*

Table 3. *Effect of Phase 1 price on Phase 2 Willingness to Pay*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dep. Var.:	Bought 2nd LLIN											
Specification:	OLS				Logistic (Odds Ratios)				Probit (Marginal Effects)			
Panel A. No controls												
1st LLIN Price in US$	-0.020	-0.038			0.851	0.757			-0.020	-0.038		
	(0.016)	(0.043)			(0.107)	(0.247)			(0.016)	(0.042)		
(1st LLIN Price in US$) squared		0.005				1.033				0.005		
		(0.011)				(0.086)				(0.011)		
1st LLIN Price ≤ 50 Ksh (High Subsidy)			0.072				1.655				0.071	
			(0.039)*				(0.468)*				(0.042)*	
1st LLIN Price = 0 (Free)				0.070				1.686				0.075
				(0.047)				(0.593)				(0.054)
1st LLIN Price = 40 or 50 Ksh (High Subsidy but not Free)				0.075				1.619				0.072
				(0.056)				(0.607)				(0.062)
Observations	599	599	599	599	599	599	599	599	599	599	599	599
Panel B. With household-level controls												
1st LLIN Price in US$	-0.020	-0.043			0.854	0.709			-0.019	-0.042		
	(0.016)	(0.042)			(0.111)	(0.237)			(0.016)	(0.041)		
(1st LLIN Price in US$) squared		0.006				1.053				0.006		
		(0.011)				(0.090)				(0.010)		
1st LLIN Price ≤ 50 Ksh (High Subsidy)			0.074				1.722				0.071	
			(0.039)*				(0.503)*				(0.042)*	
1st LLIN Price = 0 (Free)				0.071				1.754				0.074
				(0.047)				(0.632)				(0.053)
1st LLIN Price = 40 or 50 Ksh (High Subsidy but not Free)				0.079				1.685				0.072
				(0.057)				(0.659)				(0.063)
Observations	590	590	590	590	590	590	590	590	590	590	590	590
Mean of Dep. Variable in non-"High Subsidy" group	0.148	0.148	0.148	0.148	0.148	0.148	0.148	0.148	0.148	0.148	0.148	0.148

*Notes: All regressions inclue enumeration area fixed effects. Price of 1st LLIN varies from 0 to US$3.8. Household level controls in Panel B include all 14 variables presented in Table 1. Standard errors in parentheses. *Significant at 10%; ** significant at 5%; *** significant at 1%.*

Table 4. *IV Estimates: Effect of Phase 1 adoption on Phase 2 Willingness to Pay*

	(1)	(2)	(3)	(4)
Dep. Var.:	Bought 2nd LLIN			
Specification:	OLS		Probit	
Panel A. No controls				
Experimented with 1st LLIN (instrumented with polynomial in price)	0.119		0.498	
	(0.089)		(0.363)	
Experimented with 1st LLIN (instrumented with "High Subsidy" dummy)		0.208		0.804
		(0.115)*		(0.416)*
Observations	599	599	599	599
First Stage F-Stat / Chi-2	47.51	54.96	73.86	44.78
Panel B. With household-level controls				
Experimented with 1st LLIN (instrumented with polynomial in price)	0.119		0.498	
	(0.087)		(0.363)	
Experimented with 1st LLIN (instrumented with "High Subsidy" dummy)		0.209		0.804
		(0.113)*		(0.414)*
Observations	590	590	590	590
First Stage F-Stat / Chi-2	48.44	56.22	76.14	46.47

*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 enumeration area fixed effects. Price of 1st LLIN varies from 0 to US$3.8. Household level controls in Panel B include all 14 variables presented in Table 1. Standard errors in parentheses. *Significant at 10%; ** significant at 5%; *** significant at 1%.*

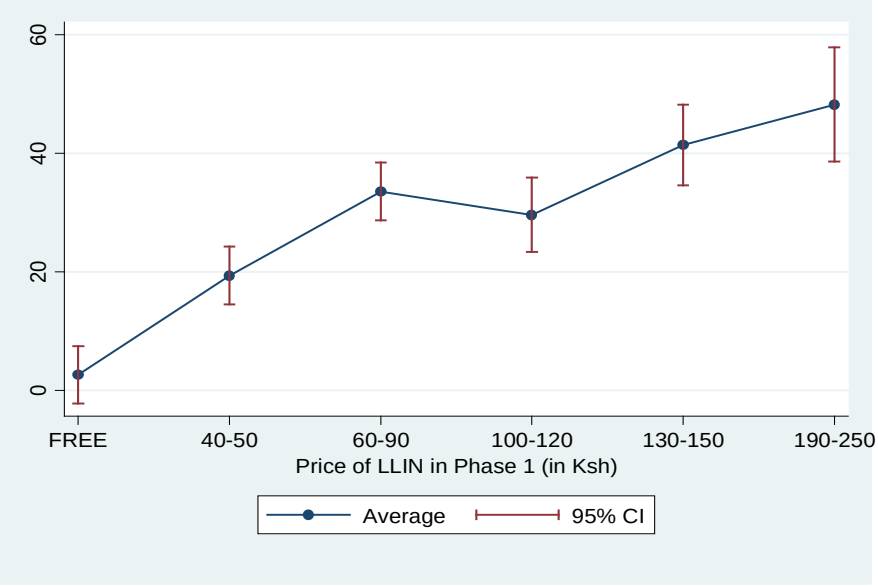
Table 5. *Diffusion Effects*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Radius R=	250 meters				500 meters				750 meters			
Sample	All	Subset observed both periods			All	Subset observed both periods			All	Subset observed both periods		
Dep.Variable:	Bought 1st LLIN	Bought 1st LLIN	Bought at least one LLIN	Bought two LLINs	Bought 1st LLIN	Bought 1st LLIN	Bought at least one LLIN	Bought two LLINs	Bought 1st LLIN	Bought 1st LLIN	Bought at least one LLIN	Bought two LLINs
Panel A. Specification: RELATIVE exposure												
Share of study households with max subsidy within R	0.124	0.163	0.117	-0.032	0.189	0.184	0.136	-0.160	0.243	0.169	0.141	-0.233
	(0.056)**	(0.068)**	(0.072)	(0.048)	(0.083)**	(0.102)*	(0.108)	(0.072)**	(0.104)**	(0.128)	(0.136)	(0.090)***
	[0.055]**	[0.063]**	[0.069]*	[0.054]	[0.082]**	[0.095]*	[0.101]	[0.089]*	[0.108]**	[0.129]	[0.137]	[0.099]**
Panel B. Specification: RELATIVE exposure, controlling for density												
Share of study households with max subsidy within R	0.117	0.160	0.123	-0.026	0.174	0.152	0.113	-0.154	0.217	0.124	0.101	-0.217
	(0.057)**	(0.069)**	(0.073)*	(0.049)	(0.084)**	(0.103)	(0.110)	(0.073)**	(0.105)**	(0.130)	(0.137)	(0.091)**
	[0.056]**	[0.063]**	[0.069]*	[0.055]	[0.082]**	[0.095]	[0.099]	[0.092]*	[0.11]**	[0.133]	[0.136]	[0.098]**
Total # (/10) of study households within R	0.028	0.010	-0.023	-0.026	0.018	0.031	0.023	-0.006	0.013	0.023	0.021	-0.008
	(0.026)	(0.042)	(0.045)	(0.030)	(0.011)*	(0.018)*	(0.019)	(0.013)	(0.007)*	(0.011)**	(0.012)*	(0.008)
	[0.025]	[0.037]	[0.043]	[0.026]	[0.011]	[0.016]*	[0.021]	[0.012]	[0.007]*	[0.011]**	[0.012]*	[0.006]
Panel C. Specification: ABSOLUTE exposure												
# (/10) of study households with max subsidy within R	0.093	0.167	0.190	-0.075	0.061	0.056	0.085	-0.123	0.061	0.064	0.081	-0.069
	(0.136)	(0.168)	(0.177)	(0.118)	(0.075)	(0.085)	(0.091)	(0.060)**	(0.055)	(0.063)	(0.067)	(0.045)
	[0.134]	[0.161]	[0.174]	[0.108]	[0.077]	[0.085]	[0.092]	[0.053]**	[0.065]	[0.066]	[0.069]	[0.034]**
Total # (/10) of study households within R	0.013	-0.014	-0.055	-0.011	0.007	0.023	0.006	0.019	0.000	0.009	0.002	0.006
	(0.041)	(0.057)	(0.061)	(0.040)	(0.020)	(0.027)	(0.029)	(0.019)	(0.015)	(0.019)	(0.021)	(0.014)
	[0.042]	[0.056]	[0.062]	[0.031]	[0.019]	[0.027]	[0.032]	[0.016]	[0.016]	[0.022]	[0.023]	[0.01]
Panel D. IV Estimates (I)												
Share of study households using LLIN (instrumented	0.215	0.277	0.186	-0.035	0.292	0.255	0.175	-0.258	0.447	0.272	0.221	-0.475
with *Share with max subsidy*) within R	(0.112)*	(0.143)*	(0.150)	(0.100)	(0.146)**	(0.187)	(0.198)	(0.132)*	(0.218)**	(0.286)	(0.302)	(0.197)**
Total # (/10) of study households within R	0.009	0.011	0.013	-0.010	0.011	0.016	0.017	-0.003	0.012	0.019	0.018	-0.001
	(0.008)	(0.013)	(0.014)	(0.009)	(0.007)	(0.013)	(0.014)	(0.009)	(0.007)	(0.013)	(0.014)	(0.009)
Panel E. IV Estimates (II)												
Share of study households who bought LLIN within 1	0.210	0.150	0.092	0.007	0.290	0.134	-0.001	-0.051	0.382	0.036	-0.019	-0.391
week (instrumented with *Share with max subsidy*) within R	(0.095)**	(0.122)	(0.129)	(0.086)	(0.119)**	(0.162)	(0.171)	(0.114)	(0.183)**	(0.267)	(0.282)	(0.185)**
Total # (/10) of study households within R	0.009	0.017	0.018	-0.012	0.011	0.020	0.023	-0.010	0.012	0.024	0.023	-0.003
	(0.008)	(0.013)	(0.013)	(0.009)	(0.007)	(0.012)	(0.013)*	(0.009)	(0.007)*	(0.013)*	(0.013)*	(0.009)
Observations	1094	584	584	584	1094	584	584	584	1094	584	584	584
Mean of Dep. Variable	0.458	0.469	0.533	0.094	0.458	0.469	0.533	0.094	0.458	0.469	0.533	0.094

Notes: Each panel-column corresponds to a separate regression. Coefficient estimates obtained using linear regressions with enumeration area fixed effects. All regressions also include a quadratic in own price. The number of observations and mean of the dependent variable, common to all regressions wtihin one column, are presented at the bottom.

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

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



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

Table A1. *Attrition*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var.:	Bought 1st LLIN but Missing in 1st Follow-Up				Attrited before distribution of 2nd LLIN voucher			
Specification:	OLS				OLS			
Panel A. No controls								
1st LLIN Price in US$	0.005	-0.016			0.000	0.043		
	(0.016)	(0.043)			(0.011)	(0.028)		
(1st LLIN Price in US$) squared		0.007				-0.012		
		(0.013)				(0.007)*		
1st LLIN Price ≤ 50 Ksh (High Subsidy)			0.014				-0.033	
			(0.026)				(0.026)	
1st LLIN Price = 0 (Free)				-0.007				-0.026
				(0.040)				(0.031)
1st LLIN Price = 40 or 50 Ksh (High Subsidy but not Free)				0.025				-0.044
				(0.030)				(0.038)
Observations	492	492	492	492	642	642	642	642
	0.087	0.087	0.087	0.087	0.087	0.087	0.087	0.087
Panel B. With household-level controls								
1st LLIN Price in US$	0.002	-0.009			-0.001	0.043		
	(0.016)	(0.043)			(0.011)	(0.028)		
(1st LLIN Price in US$) squared		0.004				-0.012		
		(0.013)				(0.007)*		
1st LLIN Price ≤ 50 Ksh (High Subsidy)			0.016				-0.033	
			(0.026)				(0.026)	
1st LLIN Price = 0 (Free)				-0.008				-0.027
				(0.040)				(0.031)
1st LLIN Price = 40 or 50 Ksh (High Subsidy but not Free)				0.029				-0.042
				(0.030)				(0.039)
Observations	488	488	488	488	633	633	633	633
Mean of Dep. Variable in non-"High Subsidy" group	0.087	0.087	0.087	0.087	0.071	0.071	0.071	0.071

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)
	Dep. Var: Had malaria in the month preceding the 1-yr Follow-up Survey					
	OLS		Logistic		IV	
Panel A. No Controls						
1st LLIN Price ≤ 50 Ksh (High Subsidy)	-0.020	-0.028	0.770	0.693		
	(0.023)	(0.024)	(0.241)	(0.219)		
Share of study households with max subsidy within 500m radius		-0.040		0.553		
		(0.036)		(0.299)		
Total # of study households within 500m radius		-0.003		0.959		-0.003
		(0.003)		(0.032)		(0.003)
Experimented with 1st LLIN (instrumented with "High Subsidy" dummy)					-0.067	-0.086
					(0.076)	(0.078)
Share of study households using LLIN (instrumented with *Share with max subsidy*) within R						-0.040
						(0.075)
Observations	961	937	961	937	961	937
Panel B. With Household-Level Controls						
1st LLIN Price ≤ 50 Ksh (High Subsidy)	-0.030	-0.032	0.656	0.642		
	(0.022)	(0.023)	(0.201)	(0.196)		
Share of study households with max subsidy within 500m radius		-0.042		0.577		
		(0.038)		(0.332)		
Total # of study households within 500m radius		-0.004		0.954		-0.003
		(0.003)		(0.032)		(0.003)
Experimented with 1st LLIN (instrumented with "High Subsidy" dummy)					-0.095	-0.098
					(0.071)	(0.077)
Share of study households using LLIN (instrumented with *Share with max subsidy*) within R						-0.040
						(0.079)
Observations	946	931	946	931	946	931
Mean of Dep. Variable in non-"High Subsidy" group	0.095	0.098	0.095	0.098	0.095	0.098

Notes: Sample restricted to the four areas where the first year follow-up was conducted for both redeemers and non-redeemers of the 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 Panel B include all 14 variables presented in Table 1.
Significant at 10%; ** significant at 5%; * significant at 1%.*

Table A3. *Social Exposure Variables*

Panel A. Summary Statistics

	Mean	Std. Dev	Min	Median	90th ptile	Max
Within 250m radius						
Share with max subsidy	0.24	0.25	0.00	0.20	0.50	1.00
Share using LLIN	0.29	0.28	0.00	0.25	0.67	1.00
# with max subsidy	1.28	1.58	0	1	3	8
# using LLIN	1.56	1.87	0	1	4	10
Total # of sampled households	5.75	5.16	0	5	12	31
Within 500m radius						
Share with max subsidy	0.25	0.18	0.00	0.24	0.46	1.00
Share using LLIN	0.31	0.22	0.00	0.28	0.62	1.00
# with max subsidy	4.01	3.80	0	3	9	17
# using LLIN	5.05	4.47	0	4	11	21
Total # of sampled households	18.13	12.95	0	17	33	63
Within 750m radius						
Share with max subsidy	0.26	0.16	0.00	0.24	0.44	1.00
Share using LLIN	0.32	0.19	0.00	0.27	0.60	1.00
# with max subsidy	7.77	6.35	0.00	7	18	24
# using LLIN	9.63	7.06	0.00	8	21	32
Total # of sampled households	34.14	20.12	0.00	36	60	80

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	0.133					
	(0.121)					
# with max subsidy		0.027				
		(0.030)				
Total # of sampled households	0.002	-0.002				
	(0.006)	(0.008)				
Within 500m radius						
Share with max subsidy			-0.125			
			(0.179)			
# with max subsidy				-0.004		
				(0.015)		
Total # of sampled households			0.001	0.002		
			(0.002)	(0.004)		
Within 750m radius						
Share with max subsidy					-0.190	
					(0.224)	
# with max subsidy						0.003
						(0.010)
Total # of sampled households					0.000	0.000
					(0.002)	(0.003)
Observations	1094	980	1094	1094	1094	1094

Notes: Coefficient estimates obtained using linear regression with area fixed effects.
Significant at 10%; ** significant at 5%; * significant at 1%.*

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

	(1)	(2)	(3)	(4)	(5)	(6)
	Bought 1st LLIN					
Radius R=	250 meters		500 meters		750 meters	
Panel A. No controls						
Share of study households with max subsidy within R	0.134		0.160		0.162	
	(0.060)**		(0.090)*		(0.114)	
Share of study households with price 60-90 within R	0.088		0.001		-0.037	
	(0.074)		(0.103)		(0.132)	
Share of study households with price 100-120 within R	0.072		-0.049		-0.070	
	(0.067)		(0.099)		(0.128)	
Share of study households with price 130-150 within R	-0.039		-0.061		-0.270	
	(0.072)		(0.110)		(0.141)*	
# (/10) of study households with max subsidy within R		0.100		0.058		0.034
		(0.161)		(0.081)		(0.058)
# (/10) of study households with price 60-90 within R		0.093		-0.099		-0.153
		(0.164)		(0.086)		(0.064)**
# (/10) of study households with price 100-120 within R		0.015		-0.018		-0.084
		(0.174)		(0.110)		(0.086)
# (/10) of study households with price 130-150 within R		-0.018		0.003		-0.134
		(0.188)		(0.100)		(0.077)*
Total # (/10) of study households within R	0.020	-0.010	0.020	0.032	0.016	0.074
	(0.027)	(0.109)	(0.011)*	(0.057)	(0.007)**	(0.043)*
Observations	1094	1094	1094	1094	1094	1094
Panel B. With Household-Level Controls						
Share of study households with max subsidy within R	0.129		0.173		0.179	
	(0.061)**		(0.093)*		(0.117)	
Share of study households with price 60-90 within R	0.101		0.004		-0.044	
	(0.074)		(0.103)		(0.133)	
Share of study households with price 100-120 within R	0.072		-0.063		-0.059	
	(0.068)		(0.101)		(0.130)	
Share of study households with price 130-150 within R	-0.027		-0.037		-0.223	
	(0.073)		(0.117)		(0.153)	
# (/10) of study households with max subsidy within R		0.078		0.055		0.043
		(0.162)		(0.082)		(0.059)
# (/10) of study households with price 60-90 within R		0.124		-0.087		-0.131
		(0.165)		(0.087)		(0.065)**
# (/10) of study households with price 100-120 within R		-0.001		-0.041		-0.070
		(0.176)		(0.110)		(0.086)
# (/10) of study households with price 130-150 within R		-0.012		0.011		-0.104
		(0.188)		(0.101)		(0.079)
Total # (/10) of study households within R	0.028	0.001	0.026	0.042	0.020	0.065
	(0.028)	(0.110)	(0.012)**	(0.057)	(0.008)**	(0.044)
Observations	1088	1088	1088	1088	1088	1088

Notes: Sample restricted to 1094 for which accurate GIS coordinates could be collected. 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. Omitted exposure category: share (#) of study households with price >150 within R.