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Price Subsidies, Diagnostic Tests, and Targeting of Malaria Treatment: Evidence from a Randomized Controlled Trial

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## ABSTRACT

Both under- and over-treatment of communicable diseases are public bads. But efforts to decrease one run the risk of increasing the other. Using rich experimental data on household treatment-seeking behavior in Kenya, we study the implications of this tradeoff for subsidizing life-saving antimalarials sold over-the-counter at retail drug outlets. We show that a very high subsidy (such as the one under consideration by the international community) dramatically increases access, but nearly half of subsidized pills go to patients without malaria. We study two ways to better target subsidized drugs: reducing the subsidy level and introducing rapid malaria tests over-the-counter.

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# 1 Introduction

Limiting the spread of infectious diseases has positive spillovers. As such, subsidies for prevention and treatment products are often central to infectious disease programs. Financing such subsidies is obviously subject to a budget constraint, however, and it is important to ensure that subsidy dollars are spent where they have the highest return. For products whose usage has heterogeneous returns, the introduction of a subsidy creates a tradeoff between access and targeting. That is, subsidies for the product are likely to increase demand among both appropriate users, for whom the returns are indeed high, and among inappropriate users, for whom the benefits are marginal. This is the "menu-setting problem" described by Olmstead and Zeckhauser (1999).

This paper studies the menu-setting problem introduced by subsidies for the latest class of antimalarials, artemisinin combination therapies (ACTs). This application is of particular importance because the benefits of ACTs to appropriate users are extremely high: malaria is a leading cause of death for children, and artemisinin-based therapies now constitute the only effective class of antimalarials in Africa, where the malaria parasite has developed resistance to earlier generations of antimalarials, rendering them largely ineffective.[1] At the same time, over-treatment with ACTs is costly: it can delay or preclude proper treatment for the true cause of illness, waste scarce resources for malaria control, and contribute to parasite resistance to antimalarials (Perkins and Bell 2008; White 2004). This makes the menu-setting problem even more pressing: the trade-off is not just between affordability and cost-ineffective consumption at a single point in time, but a trade-off between affordability today and effectiveness in the future.

A natural way of ensuring access for appropriate users while limiting overuse is to distribute subsidized health technologies like ACTs through the public health system, where diagnostic tools and trained medical personnel can target technologies to patients with high returns. Unfortunately, this approach may have a limited impact where the public health system functions poorly or is difficult for patients to access, as is the case in rural areas of many developing countries. Indeed, even though many malaria-endemic African countries have a policy of free distribution of ACTs to malaria patients at public health facilities, in 2008, six years after ACTs were placed on the WHO's essential drugs list, fewer than 15 per-

[1] Chloroquine (CQ) was introduced in Kenya in the late 1930s. Resistance of P. falciparum (the parasite strain responsible for most malaria mortality) to chloroquine was first detected in 1978. By the early 1990s, CQ resistance in the western part of the country was already 70 percent (Shretta et al. 2008). Subsequent innovations in antimalarial medicines have been successively less able to withstand parasite resistance. For example, resistance to Sulfadoxine-pyrimethamine (SP) emerged within five years of its introduction as first-line treatment for uncomplicated falciparum malaria in young children in Western Kenya (Terlouw et al. 2003).

cent of African children with malaria were treated with ACTs (World Health Organization 2009).

Such low coverage rates have spurred the international community to consider heavily subsidizing ACTs through retail-sector drug shops. Yet it is in the retail sector, encompassing a wide array of loosely regulated, informal outlets, where the trade-off between access and targeting is potentially most stark – while these outlets have wide reach, they offer limited possibilities for screening. What's more, retail sector subsidies could have the unintended effect of crowding out treatment seeking at formal health centers, where patients may be diagnosed with diseases other than malaria or receive more comprehensive medical advice. Ultimately, gauging the costs and benefits of retail-sector subsidies requires a deep understanding of treatment-seeking behavior in the context of suspected malaria – how much information do households have on the underlying malaria risk of various illness episodes, and how does that affect their willingness to pay for treatment? How elastic is the demand for formal health care to retail sector prices?

This paper presents the results of a field experiment designed to (1) generate detailed data on treatment-seeking behavior among rural households in a malaria-endemic area of Africa, (2) estimate how willingness to pay for quality antimalarials varies with information on patients' underlying malaria risk, and (3) quantify the trade-off introduced by ACT subsidies between broadening ACT access and targeting ACTs to individuals with malaria.

We generate five main empirical results. First, many households bypass the public system entirely and instead procure medication through retail-sector drug shops. Given this, a heavy retail-sector ACT subsidy comparable to that under consideration by the international community is highly effective at increasing access. While 39 percent of illness episodes with at least one malaria-like symptom are true malaria, only 19 percent of these episodes are treated with an ACT in the absence of a retail sector subsidy. This is despite the fact that ACTs are free in Kenyan public health facilities. When a 92 percent subsidy is introduced in the retail sector, the share treated with an ACT more than doubles, reaching 41 percent.

Second, this large increase in access corresponds to an increase among both appropriate and inappropriate users. Thus, overall targeting is poor: only 56 percent of retail-sector subsidized ACTs go to malaria-positive individuals. Importantly, over-treatment is primarily a concern among teens and adults: only 21 percent of individuals aged 14 and above who take heavily subsidized ACTs actually have malaria, while 79 percent of children who take ACTs actually have malaria. This phenomenon is mainly driven by the fact that children are more prone to malaria infection and, conditional on having at least one malaria-like symptom, much more likely to have malaria.

Third, over the range of heavy subsidies that we consider (92 to 80 percent), the trade-off

between ACT access and targeting is not very stark. When moving from the highest to the lowest subsidy level, access among those with the highest returns to ACTs (children, who are at higher risk if not promptly treated) remains unchanged. Yet targeting vastly improves, with the share of subsidized pills going to malaria positive patients rising from 56 to 75 percent. The fact that reducing the subsidy level screens out mostly malaria-negative adults suggests that (1) households have at least some private information about the probability that an illness episode is actually malaria; and (2) households are willing to pay more to treat higher probability illnesses with ACTs. Though as evidenced by the low take-up at full price, they are willing to pay only *somewhat* more, making price alone an insufficient targeting tool.

In this context, one way to improve private information (and further increase targeting) is to provide diagnostic testing in retail sector drug shops. To assess the benefits of this strategy, our experimental design included random variation in access to subsidized, over-the-counter rapid diagnostic tests (RDTs) for malaria in conjunction with ACTs. Unsubsidized RDTs cost around $1.33 (compared to $6 for an adult dose of unsubsidized ACT). RDTs are a relatively new technology that have only recently begun to be adopted by health centers. Our fourth result is that making RDTs available in the retail sector and subsidizing them heavily (85 percent or more, making them substantially cheaper than a subsidized adult ACT dose) doubles the rate at which illnesses are tested for malaria. Despite this, retail-sector RDTs do not offer an immediate remedy to the over-treatment problem: our fifth result is that over half of the patients testing negative elected to take a subsidized ACT anyway. Compliance with negative test results may increase over time as households learn about the reliability of RDTs but our short-run study was not designed to speak to this learning process.

Taken together, our findings suggest that retail sector subsidies for either drugs or diagnostic tests can have dramatic effects on access, even when these commodities are more heavily subsidized (i.e. free) in the public sector. Namely, subsidizing ACTs and making them available over-the-counter in drug shops substantially increases the rate at which malaria-like illnesses are effectively treated; and subsidizing over-the-counter RDTs substantially increases the rate at which illnesses are diagnosed. But retail sector subsidies also have some drawbacks: we see substantial wastage of ACTs on people who do not have malaria (even in the presence of RDT subsidies) and some evidence of crowd-out of public sector treatment seeking.

In addition to shedding light on how prices and information impact the crucial health care decisions of individuals in developing countries, our results have important implications for malaria control policy. In response to the low rates of ACT access noted earlier, the

Affordable Medicines Facility for malaria (AMFm) initiative, financed by major international aid agencies, was established in 2009 in order to reduce the price and increase the availability of ACTs in retail sector establishments through a 95 percent subsidy to pharmaceutical wholesalers (Arrow et al. 2004). The AMFm program was controversial, and after a pilot in 7 countries between 2010 and 2012, its future remains uncertain. The initiative proved to be quite costly – only two years of drug co-payments for the 7 countries in the AMFm pilot cost over $450 million (The Economist 2012). The main critics of the AMFm argued that there was "*no evidence that it has saved the lives of the most vulnerable or delayed drug resistance. Rather, this global subsidy has incentivised medicine sales without diagnosis and shown no evidence that it has served poor people*" (Oxfam 2012). Our data suggest that at least some of these criticisms are unfounded. Our results clearly show that an AMFm-type subsidy can expand access among the malaria-positive poor (thereby saving lives), and does so without reducing access to formal malaria diagnosis, which appears to be very low in any case.

However, our results also show that the goal of such a subsidy should not necessarily be to make ACTs the cheapest treatment option in the retail sector. A more cost-effective approach would account for individuals' use of private information in treatment seeking and find the price that strikes the right balance between access and targeting. Of course the "right" price will be somewhat context specific, but we provide some evidence that the key features of the malaria treatment seeking environment in Western Kenya that deliver our results on the access-targeting tradeoff are common to other regions of East Africa as well.

Beyond its immediate relevance to the design of subsidies for malaria treatment and diagnosis, which affect millions of households in rural Africa in both the short-run (affordability) and long-run (drug resistance), our paper contributes to the literature in three main ways. First, we contribute to the literature on under-diagnosis and over-treatment, two major drivers of health care costs and a source of concern throughout the world (Das et al. 2008; Welch et al. 2011; Adhvaryu 2011). Second, we contribute to the literature on treatment-seeking behavior in resource-constrained environments, along with the earlier contributions on the impact of user charges for health care (see Griffin (1987) and Gertler and Hammer (1997) for reviews), and, more recently, the detailed studies by Leonard et al. (2002) in Tanzania, Banerjee et al. (2004) in Rajasthan (India), and Leonard (2007, 2009) in Tanzania and Cameroon, respectively. Third, our paper adds to a fast-growing experimental literature on user fees for health products whose appropriate use generates positive externalities. So far this literature has focused on optimal pricing for preventative health products, such as bednets or water purification kits, for which over-use is less of a concern, and for which the objective of the social planner is typically conceptualized as expanding access while limiting

*under-use* among subsidy beneficiaries.[2] In contrast, this paper considers the price-setting problem that arises when over-use generates negative externalities.

The remainder of the paper proceeds as follows: Section 2 provides some background facts on the malaria burden and treatment options in rural Africa, as well as the AMFm subsidy. Section 3 develops a theoretical framework for studying treatment-seeking behavior in this environment, and identifies the key tradeoffs inherent to heavily subsidizing ACTs. Section 4 describes our experimental design and data. We present results on the impact of ACT subsidies in Section 5 and results on the impact of RDT subsidies in Section 6.

# 2 Background

Malaria is estimated to cause 200 million illnesses and to kill over 600,000 people every year – the great majority of them in Africa, and the great majority of them among children under age five (World Health Organization 2011). Children under five are most vulnerable to acquiring and dying from malaria because immunity develops with repeated exposure. How readily these children can access effective antimalarials when they get infected is thus a very important determinant of overall malaria morbidity and mortality. Unfortunately, due in large part to the high cost of ACTs, a large share of children under the age of five are treated not with ACTs, but with older antimalarials to which the parasite has gained resistance (World Health Organization 2009).

To address this issue, many African countries (including Kenya) have a policy of providing ACTs for free to those diagnosed with malaria in public health facilities. Diagnosis at health facilities is typically either symptomatic or based on blood slide microscopy tests for malaria. The accuracy of symptom-based diagnosis can be low, however, and even the accuracy of microscopic diagnosis is quite variable in rural settings.[3] Consequently a substantial share of individuals are given antimalarials even if they test negative (Zurovac et al. 2006; Juma and Zurovac 2011). This, coupled with poorly functioning government procurement processes, contributes to regular stockouts of free ACTs (Kangwana et al. 2009).

Stockouts are only one drawback of seeking care at public health facilities. While ACTs

[2]See Cohen and Dupas (2010), Dupas (2012), Hoffmann (2009), and Tarozzi et al. (2011) on bednets; Ashraf, Berry, and Shapiro (2010) and Kremer et al. (2011) on water purification; and Dupas (2011) for a review.

[3]The quality of microscopic testing varies greatly across lab technicians and with the quality of the equipment, and the rate of false negatives in the field was estimated at 31 percent by a 2002 study in Kenya (Zurovac et al. 2006). In contrast, in populations with high parasite density, properly manufactured RDTs have a rate of false negatives generally under 5 percent in lab settings (World Health Organization 2010) and around 8 percent in the field (de Oliveira et al. 2009). The rate of false positives for RDTs is 3 percent. While RDTs perform better in the field and are also cheaper, they were only introduced in the early 2000's and their use is not yet widespread at public health facilities, especially in rural areas.

are free if prescribed and available, fees are often charged for consultation and/or diagnosis (as is the case in our study area). What's more, distance, high rates of provider absenteeism (Chaudhury et al. 2006), long lines and limited opening hours imply a substantial indirect cost of seeking treatment for suspected malaria in the public sector.

Given the drawbacks of the public sector, it is common for households to treat illnesses with over-the-counter medication purchased at drug shops. For example, a seven-country study found that the retail sector accounted for 40-97 percent of all antimalarial sales (Arnold et al. 2012). Our own study population reflects this broad pattern, with 52 percent of antimalarials procured from a drug shop at baseline (Appendix Table A1).

Most households live a short walk away from a drug shop, and these shops are open reliably and offer a wide variety of medications. Drug shop attendants have widely varying levels of education and credentials, but they are often asked by patients for treatment recommendations (Patouillard et al. 2010; Marsh et al. 2004). Drawbacks of drug shops include the lack of skilled medical staff and diagnostic capability, the risk of receiving lower quality or counterfeit drugs (Bjorkman et al. 2012), and the absence of emergency medicines and equipment to treat severe malaria infections.

Given drug shops' large share of the antimalarials market, a call was made by the international community to reduce the price and increase the availability of ACTs in the retail sector. The answer to this call was the AMFm, which began to subsidize ACTs in seven pilot countries in 2010. Through a factory-gate co-payment (a "global subsidy"), the AMFm aims to reduce the price of ACTs by roughly 95 percent to first line buyers, such as governments, NGOs and private wholesalers (Global Fund to Fight AIDS, TB and Malaria 2010). The final price to consumers in retail outlets is not formally restricted, but the aim is for ACTs to be cheap enough for most rural, poor populations to afford them and to crowd-out purchases of other antimalarials. For example, the Kenyan government set a "recommended retail price" for ACTs purchased under the AMFm of 40 Kenyan Shillings (KSh), which is about \$0.50.[4] The government-selected target prices varied across pilot AMFm countries, spanning a subsidy range from 85 percent in Ghana to 92 percent in Kenya. Our study was conceived and implemented in 2008/2009 – at this time the AMFm was under consideration and target prices were being discussed, but the pilots had not yet started. To maximize policy relevance we therefore designed our study to include a range of targeted subsidy levels (i.e. 88 and 92 percent) as well as a somewhat lower subsidy level (80 percent).

[4]Retail sector ACT price surveys conducted after the pilot subsidy was introduced suggest the retail price indeed fell to a level close to KSh 40 on average (Arnold et al. 2012).

# 3 Theoretical Framework

This section models malaria treatment seeking behavior in the environment described above. The goal of the model is to provide a framework for our empirical analysis while highlighting the access/targeting tradeoff inherent to the approach of subsidizing retail sector ACTs. The tradeoff is embedded in the following two policy outcomes: (1) under-treatment: the share of true malaria episodes that do not get treated with ACTs; and (2) over-treatment: the share of non-malaria episodes that are treated with ACTs.

In our empirical analysis, we identify the impact of different subsidy policies on under- and over-treatment by focusing on two related outcomes: access (the share of illness episodes treated with ACTs) and targeting (the share of ACT-takers who are malaria positive). Specifically, we can map access and targeting to under- and over-treatment as long as we know the share of all illness episodes that are truly malaria.[5] In what follows, we present a theoretical framework to discuss how ACT and RDT subsidies will affect these key outcomes.

## 3.1 Household Decision Making

We consider an environment where, when faced with an illness shock, the household has three possible actions, $a \in \{h, s, n\}$: (1) seek diagnosis at a formal health facility (receiving ACTs if positive): $a = h$; (2) bypass the public health sector and buy ACTs at the drug shop: $a = s$; (3) purchase non-ACT drugs or do nothing: $a = n$. When a household gets an illness shock, the household observes the symptoms of the illness and subjectively assesses the probability $\pi$ that the illness is actually malaria. We assume that households' subjective malaria assessments are accurate, in that a household's self-assessed probability of having malaria is equal to the true probability conditional on characteristics of the illness.[6] The expected value of taking a particular action $a \in \{h, s, n\}$ depends on this probability, and is denoted by $V^a(\pi)$. It can be decomposed as follows:

$$\begin{aligned} V^a(\pi) &= \pi\left[U_P^a(\pi) - p_P^a(\pi)\right] + (1-\pi)\left[U_N^a(\pi) - p_N^a(\pi)\right] \\ &= \pi V_P^a(\pi) + (1-\pi) V_N^a(\pi) \end{aligned}$$

where $U_M^a(\pi)$ is the utility obtained from taking action $a$ when the individual's true malaria status is $M \in \{P, N\}$ (i.e., malaria positive or malaria negative) and $p_M^a$ is the expected price paid for treatment when the individual's true malaria status is $M$. Note that the utilities

[5] Denote overall malaria prevalence as $\Pi$, under-treatment as $UT$, over-treatment as $OT$, access as $A$ and targeting as $T$. Then $UT = 1 - TA/\Pi$ and $OT = A(1-T)/(1-\Pi)$.

[6] It is straightforward to loosen this assumption and allow for biased assessments. All the results below go through as long as actual malaria probability is strictly increasing in subjective malaria probability.

and prices may be a function of the malaria probability $\pi$. For example, if the severity of symptoms is increasing as $\pi$ increases, then individuals may expect to pay more to treat the illness, particularly when it is not actually malaria.

We assume that the value of taking action $a = n$ (doing nothing/taking non-ACT medication at the drug shop) becomes relatively less attractive as $\pi$ increases. That is, we assume that $V^a(\pi) - V^n(\pi)$ increases with $\pi$ for $a \in \{h, s\}$. An individual will seek ACT treatment at the drug shop if

$$V^s(\pi) \geq max\left\{V^h(\pi), V^n(\pi)\right\} \tag{1}$$

Figure 1, panel A, provides a graphical illustration of how a household's treatment decision depends on expected malaria positivity. Without loss of generality, we have renormalized the value functions so that $V^n(\pi) = 0$ for all $\pi$. The figure presents the case where presumptively buying an ACT is preferred at higher malaria probabilities ($\pi \geq \pi_2$), while going to the health center is preferred at intermediate malaria probabilities ($\pi_1 \leq \pi \leq \pi_2$), and taking some other action is preferred when the illness is very unlikely to be malaria ($\pi \leq \pi_1$). This is one plausible scenario, but other configurations are certainly possible (and the results below do not depend on this specific case holding in the data).

## 3.2 Impact of an ACT Subsidy at the Drug Shop

We first consider the impact of a decrease in the price of over-the-counter ACTs at the drug shop in the absence of over-the-counter diagnostic tests. A decrease in the price of ACTs in the retail sector (holding other prices constant) will decrease the cost of purchasing an ACT at the drug shop, whether one truly has malaria or not (i.e., both $p_P^s(\pi)$ and $p_N^s(\pi)$ decrease). This increases the left hand side of inequality (1) while leaving $V^h(\pi)$ and $V^n(\pi)$ unchanged for all values of $\pi$. Given this, purchases of ACTs at the drug shop will increase. This is illustrated graphically in Figure 1, panel B. Access (the fraction of illnesses treated with ACTs) therefore increases, even if all crowd-out is from the health center: in this case malaria negative illnesses previously screened out at the health center will now receive ACTs at the drug shop. Note that this increase in access always comes at the expense of decreased targeting. This is because crowd-out from the health center always worsens targeting, and crowd-out from doing nothing (action $n$) increases ACT taking for illnesses with lower malaria probabilities than those that were treated before the price reduction. The key assumption driving this result is that households are willing to pay more for ACTs when they think they are more likely to have malaria – that is, that $V^s(\pi) - V^n(\pi)$ increases with $\pi$.

When there is heterogeneity in valuations in the population, however, an ACT subsidy need not worsen targeting. For example, suppose that only wealthy households are able to af-

ford ACTs prior to a subsidy. If the subsidy policy crowds in enough high-malaria-probability poor relative to low-malaria-probability rich, then it is possible that overall targeting will improve. This underscores that it is important to pay attention to distributional impacts of the ACT subsidy. In particular, the subsidy would be especially attractive if it increased take-up among high-positivity populations who didn't have access to ACTs before (this is certainly the intent of the AMFm). On the other hand, it is possible that the subsidy would mostly go to populations who would have gotten the ACT regardless of the subsidy policy (at a health center, for example), or to very low-positivity populations.

## 3.3 Impact of Adding an RDT Subsidy at the Drug Shop

Now suppose that at some cost, an individual can receive a diagnosis (take an RDT) for malaria at the drug shop. There are two primary advantages of taking a test: (1) If the test is negative, the individual avoids the need to pay for an antimalarial. This is particularly attractive when the price of the RDT is less than the price of the antimalarial. (2) If the test is negative, the individual will be more likely to select an appropriate medication.[7] Figure 1, Panel C, provides a graphical illustration of the impact of adding an RDT subsidy. The expected utility of first taking an RDT at the drug shop and then taking ACTs if positive is illustrated by the dashed line labeled $V^r(\pi)$. $V^r(\pi)$ crosses $V^s(\pi)$ from above since presumptive treatment becomes relatively more attractive as $\pi$ increases. If the subsidized test is not free, then as shown in the graphical example, not everyone who seeks treatment at the drug shop will take the test – households with $\pi \geq \pi_3$ do not bother to take an RDT and instead presumptively treat with an ACT because they are very certain that they have malaria.

The figure also illustrates that subsidizing RDTs has both an intensive and an extensive margin effect. The intensive margin effect applies to individuals with $\pi_2 \leq \pi \leq \pi_3$. These individuals would have sought care at the drug shop even without an RDT, but now they base their ACT purchase decision on their RDT result. As long as some of these individuals comply with the test result, this will reduce over-treatment while leaving under-treatment unchanged. On the extensive margin, the RDT subsidy draws in a set of illnesses to the drug shop that would have otherwise sought treatment elsewhere (on the figure, these are illnesses with $\pi_1 \leq \pi \leq \pi_2$). As long as all these individuals comply with the test result, under-treatment will decrease (weakly, if all crowd-out is from the health center) while over-treatment will not change.

Thus *in the perfect compliance case* the intensive and extensive margin effects imply

[7] There are other potential advantages to taking an RDT that we discuss in section 6.3.

that over-the-counter RDT subsidies will decrease both under-treatment and over-treatment. However, if not all individuals crowded into the drug shop comply with the RDT test result, the extensive margin effect may increase over-treatment.

There are two key insights to take away from this framework. First, while using an ACT subsidy to decrease under-treatment comes at the expense of increasing over-treatment, the relative magnitude of the two effects is ambiguous. These magnitudes depend on the shapes of the value curves $V^a(\pi)$ for $a \in \{h, s, n\}$, heterogeneity in valuations, and treatment seeking behavior in the absence of the subsidy. Second, coupling a retail sector ACT subsidy with an RDT subsidy could allow for increased access without increasing over-treatment – this, however, will depend on takeup and patients' compliance with the test result. These insights make it clear that evaluating the costs and benefits of ACT and RDT subsidies requires detailed, illness-level data on treatment-seeking behavior, along with variation in prices. In what follows, we describe the field experiment we designed in order to obtain such data and estimate access and targeting (and hence under-treatment and over-treatment) under several possible subsidy policies.

# 4 Study Design, Data, and Empirical Background

## 4.1 Experimental Design

The experiment was conducted in the districts of Busia, Mumias, and Samia in Western Kenya between May and December of 2009. Malaria is endemic in this region with transmission occurring year-round, but with two peaks corresponding to heavy rain in May-July and October-November. Like much of sub-Saharan Africa it is rural and poor, with the majority of household heads working as subsistence farmers.

We selected four drug shops, in four rural market centers and sampled all households in the catchment area (within a 4km radius) of each of these shops.[8] We then visited each household to administer a baseline survey, which was completed by the primary female in the household whenever possible. At the end of the survey two vouchers for ACTs and, when applicable, two vouchers for RDTs were distributed. Surveyors explained that ACTs are the most effective type of antimalarial and, if the household received an RDT voucher, what the RDT was for and how it worked.[9] The vouchers stated the drug shop at which the

[8]Participating drug shops were chosen on the basis of several criteria including distance from drug shops participating in other public health interventions, shop owner qualifications, length of time the shop had been in business, and the number of daily customers.

[9]The ACT used in this study was Coartem (Artemether Lumefantrine), produced by Novartis Pharmaceuticals. The RDT was the ICT Malaria Pf test, produced by ICT Diagnostics. This type of test only

products could be purchased and did not have expiration dates so as to avoid incentivizing households to redeem vouchers in the absence of an illness episode. Of the 2,928 households sampled during the census, 2,789 (95 percent) were reached and consented to the baseline survey (baseline survey non-completion is uncorrelated with treatment status).

The experimental design is illustrated in Figure 2. Households were randomly assigned to one of three core groups, corresponding to the three policy regimes of interest. The "No Subsidy" group received vouchers to purchase unsubsidized ACTs at the market price of KSh 500 (just under $6.25). This treatment arm was meant to capture the no-subsidy status quo that prevailed in Kenya prior to the AMFm pilot, in which over-the-counter ACTs were expensive and RDTs were not available in drug shops.[10] The second group received an ACT subsidy only. This treatment was meant to reflect outcomes under the planned AMFm pilot for Kenya (i.e. without RDTs). The third group received vouchers for both subsidized ACTs and RDTs.

Within the two ACT subsidy groups ("ACT subsidy only" and "ACT+RDT subsidy"), households were randomly assigned to an ACT subsidy level of 92, 88 or 80 percent (corresponding to $0.50, $0.75 and $1.25 for an adult dose, respectively). The 92 percent subsidy level corresponds to the Kenyan government's target retail price of KSh 40 under the AMFm. The lower subsidy amounts reflect prices that could be realized if the subsidy amount were reduced, potentially to fund RDT subsidies. This price range also roughly corresponds to the price range for the cheapest to the most expensive non-ACT antimalarials available in drug shops in our area of study.

Since ACTs are priced by dose, with the appropriate dose determined by age, the four ACT subsidy levels (0, 80, 88 and 92 percent) differed in the price-per-pill to which a household was entitled. Figure A1 in the Appendix illustrates the pricing and dosing regimens in the study. All ACTs and RDTs were provided by trained study officers posted at the drug shop.

The study incorporated two additional layers of randomization. First, a sub-sample of households was randomly selected for a "surprise RDT" offer at the drug shop. Specifically, if these households came to the drug shop to redeem their ACT voucher, but did not redeem an RDT voucher (either because they were not in the RDT treatment group or because they chose not to) they were asked, *after they had paid for the ACT*, whether they would be

detects the *P. falciparum* strain of malaria, which accounts for 98 percent of all malaria infections in Kenya and is by far the most deadly strain of malaria (Kenya Division of Malaria Control 2011).

[10]The rationale behind distributing a voucher for unsubsidized ACTs to the control group was to harmonize the level of "endorsement" of the local drug shop across groups, as well as harmonize the amount of information (on effectiveness and availability) provided about ACTs across groups. The control group is much smaller in size than the other groups because we expected a large (easy to detect) effect of any subsidy, but potentially small (hard to detect) differences between subsidy levels.

willing to take an RDT for free. If the patient (the person for whom the ACT voucher was redeemed) had not come to the shop, a study officer accompanied the client back home in order to perform the test on the patient. The purpose of the surprise RDT was to obtain data on malaria positivity among ACT-takers in the absence of RDT selection effects.[11] Second, households in the ACT+RDT subsidy group were assigned to one of three RDT subsidy levels: a free RDT, an RDT for \$0.19 (corresponding to an 85% subsidy) and an RDT for \$0.19 that was refundable if the test was positive and an ACT was purchased. The purpose of this RDT price variation was to estimate the willingness to pay for RDTs. In practice, we find few substantive differences across the RDT-subsidy levels with respect to take-up and composition (see Appendix Table A7), so we pool them together in our analysis for simplicity.

The randomization of households was done using a computerized random number assignment algorithm and was stratified by drug shop, by the household's distance to the drug shop (in quartiles) and by the presence of children in the household. At the end of the experiment we visited households again to administer an endline survey. At that time, households were informed that the study was ending, and unused vouchers were collected back from households.[12]

## 4.2 Baseline Characteristics of Study Sample

Table 1 presents baseline household characteristics and tests for balance across treatment groups. We interviewed the primary female in the household roughly 90 percent of the time. Our respondents are typically married, with five years of education and four dependents, and around 60 percent are literate. On average, households live 1.7 kilometers from the drug shop for which vouchers were given and 6.6 kilometers from the nearest public health facility. Roughly 40 percent of households had heard of ACTs and less than 15 percent had heard of RDTs at baseline. To test balance across our experimental groups, we regressed each dependent variable in Table 1 on a dummy variable for each of the three ACT subsidy levels and a dummy variable for the RDT subsidy. Columns 2-5 present coefficients and standard errors from these regressions. The sixth column presents F-statistics and p-values for a test of whether the ACT subsidy treatments are jointly equal to zero. There are no

[11] Respondents could request a refund for the ACT they had just purchased if the test result was negative. 93 percent of those offered the surprise RDT consented to be tested (or consented for their sick dependent to be tested).

[12] As compensation, all households were given a tin of cooking fat at endline regardless of whether or not they returned any vouchers to us. Because information that the vouchers were being recalled might have led to presumptive voucher redemption around the time of the endline survey, in the analysis below we ignore all redemptions that took place after the rollout of the endline survey.

significant differences across treatment groups, other than for the number of acres owned and the age distribution in the household. In particular, our control group has slightly older household heads, with, as a consequence, a significantly higher fraction of adults. Since age is highly correlated with malaria positivity, a lack of balance across treatment groups in the age composition of households could confound estimates of treatment assignment on uptake and targeting, even though the magnitude of the age differences is not large. Therefore, unless otherwise noted, we control for the age of the household head in all of our results.

## 4.3 Data

We use three types of data in the analysis that follows. The first is what we liberally call "administrative" data based on voucher redemptions at the drug shop; the second is an endline survey administered to all households in the study; and the third dataset maps reported symptoms and patient characteristics to malaria test results for a universe of illness episodes experienced by our study population.

**Administrative Data: Drug Shop Transactions** The administrative data captures the details of drug shop transactions, including medicines bought, symptoms, patient characteristics, and true malaria status in case an RDT was administered. These data were recorded by trained surveyors posted at each of the four participating drug shops during opening hours, every single day throughout the study period. These data include information on over 1,700 drug shop visits made by study households over a four-month period.

**Endline Survey** The endline survey was administered about four months after the vouchers had been distributed. Only five percent of households surveyed at baseline were not reached at endline, and attrition was balanced across treatment arms. The endline survey asked households to recall all illness episodes that involved fever, chills, headache, sweats, nausea, cough, or diarrhea, that household members experienced in the four months that followed the baseline. Ninety-five percent of households reported at least one illness episode over the study period. For each of these episodes, we collected information about symptoms, where treatment was sought, what type of malaria test (if any) was taken and what medications were purchased. We find no systematic differences in illness reporting at endline between the control and the treatment groups or across treatment groups (Appendix Table A2).[13] In the analysis below, we focus only on the first illness episode reported by

[13]There is evidence that the three ACT subsidy groups have differential reporting in terms of the number of episodes reported (col 1, the p-value for the test of equality between the three groups is 0.005), but the difference is very small in magnitude (less than 5%).

each household, since we want to limit ourselves to illness episodes for which we can be sure households still had study vouchers.

**Symptoms Database** In our data, we only observe actual malaria status for those illness episodes for which (1) care was sought at a participating drug shop and (2) an RDT was administered at the time of the drug shop visit (either because the household redeemed an RDT voucher or because it was sampled for a surprise RDT). However, as the theoretical framework made clear, we need to study how care seeking behavior varies with expected malaria positivity for all illness episodes, irrespective of whether and where treatment was sought. To address this, we constructed a predicted malaria positivity index for all illness episodes, based on a "symptoms database" (N=533) collected for our study population. We collected the symptoms database approximately one year after the study ended during unannounced home visits. At the visit, trained surveyors asked if anyone was feeling ill, and if yes, they collected information about symptoms (using the same instrument as that used in the endline survey) and then tested the patient for malaria with an RDT. We use these data on illness-specific characteristics to impute a malaria probability to the universe of illness episodes enumerated at endline and all illnesses observed at drug shops.

Our predicted malaria positivity measure appears to be a useful proxy for true malaria status: the correlation between predicted positivity and actual RDT test results in our administrative drug shop data is 0.48. The appendix gives additional detail on how we constructed predicted positivity for all illness episodes enumerated at endline.

## 4.4 A Key Stylized Fact: Age and Malaria Risk

One important fact made apparent by the symptoms database is that, conditional on being ill, children have a much higher chance of having malaria than adults. This can be seen in Figure 3, Panel A, which uses local linear regression to plot malaria positivity rates by age (solid line). The malaria rate is 54 percent among those under 5 (who are most at risk of dying if not promptly treated) but just 14 percent among those considered as "adults" from a dosing point of view (14 and older, indicated by vertical gray line). This means that age is a very important (and easily observable) predictor of malaria status.

An age gradient is also observed conditional on seeking (subsidized) ACT treatment at retail outlets (panel B of Figure 3). What's more, at all ages, malaria rates are higher among individuals seeking ACT treatment (Panel B) than among the generally ill (Panel A), suggesting that households do have some private information on their malaria risk, enabling advantageous selection into treatment-seeking.

Importantly, the striking age gradient in malaria positivity is not specific to our study population. The dashed lines in Figure 3 show analogous results for individuals in Uganda, where remarkably similar, albeit slightly less sharp, patterns emerge.[14]

The fact that children are much more likely to be malaria positive than adults (and also much more at risk if they have malaria) has two immediate implications for ACT and RDT subsidies. First, it suggests that retail-sector ACT subsidies could be simply targeted at children. In practice such targeting is difficult since the drug is the same for children and adults – if only child doses were subsidized, a strict enforcement apparatus would be needed to prevent adults from taking multiple subsidized children's doses. Second, RDT subsidies clearly have greater potential to be cost-effective for adults, who are least likely to be malaria positive conditional on suspecting malaria, and require the most expensive dose of ACTs.

# 5 Results: Impacts of a Retail Sector ACT Subsidy

## 5.1 Overall Impacts on ACT Access and Treatment Seeking

We start by estimating the impact of introducing an ACT subsidy in the retail sector on overall access. To this end, we make use of our endline data. Table 2 presents the impact of the three subsidy levels (80, 88 and 92 percent) on the medical treatment of illness episodes, estimated using the following regression:

$$y_{eh} = \delta + ACTsub'_h\alpha + x'_h\gamma + \lambda_{strata} + \varepsilon_{eh} \tag{2}$$

where $y_{eh}$ is the outcome of interest for illness episode $e$ in household $h$, $ACTsub_h$ is a vector of dummy variables identifying the three ACT subsidy treatments, $\lambda_{strata}$ are strata fixed effects, and $x_h$ controls for age of the household head. Since we are first interested in the impact of an ACT subsidy absent an RDT subsidy, we exclude from this analysis households sampled for an RDT subsidy and households selected for a surprise RDT at the drug shop (as these could modify the effect of the ACT subsidy on treatment). The analysis uses the first reported illness episode experienced by the household, to ensure that a voucher could be used for treatment if so desired.[15]

Column 1 of Table 2 shows that all three subsidy levels have a very large and significant impact on ACT access. Compared to the access rate of only 19 percent in the control group

[14]The Uganda data comes from two separate projects run by Jessica Cohen (Central Uganda) and Jessica Cohen, William Dickens and Gunther Fink (Eastern Uganda).

[15]If more than one household member got sick simultaneously, we include all concurrent first episodes, and therefore cluster the standard errors in all illness episode regressions at the household level. Results are similar, though slightly attenuated, if we also include second illness episodes following the baseline survey.

(which is quite low given that 39 percent of illness episodes in our symptoms database were malaria and ACTs are supposed to be freely available at health centers), subsidies of 80 percent or more increase the likelihood that an illness is treated with an ACT by 16 to 23 percentage points (an 85-118 percent increase, significant at the 1 percent level). The vast majority of subsidized ACTs appear to go to patients who otherwise would not have taken the drug: a comparison of columns 2 and 3 show that even though the retail-sector subsidy substantially increases access to ACTs from the drug shop, access to ACTs from the health center remains virtually unchanged.

Columns 4-7 investigate changes in treatment seeking behavior in more detail. We find that all three ACT subsidy levels yield comparable and large increases in treatment seeking at the drug shop of 16-17 percentage points (around 32 percent, column 4). This is driven by a substantial increase in care-seeking: the fraction of households not seeking any care decreases by 9-11 percentage points (around 42 percent, column 6). There is also some crowd-out from the health center, though the estimated 6-11 percentage point reduction is only significant at the highest subsidy level (column 5). Importantly, we see no decrease in the likelihood of getting a malaria test (column 7), suggesting that the illness episodes that would otherwise have led to health center visits would not have received a test-based diagnosis anyway.

## 5.2 Impacts by Underlying Malaria Risk

These results make it clear that drug shops are a very important source of care for our study population (even in the control group nearly half of illnesses sought care at the drug shop) and that retail-sector subsidies effectively expand access to ACTs. However, the theoretical framework highlighted that the impact of the ACT subsidy on the ultimate outcomes of interest, over-treatment and under-treatment, will depend on how treatment seeking behavior varies with underlying malaria positivity. We study this in two ways. First, since age is a primary determinant of malaria positivity, we look at heterogeneity in treatment effects by age group. We do this in Appendix Table A4 and find that the subsidy increases ACT access among both high-positivity children (Panel A, column 1) and low-positivity teens/adults (Panel B, column 1). Illness episodes among children are somewhat more likely to be crowded out of the health center (and significantly so; Panel A, column 5̀), whereas illness episodes among older individuals are more likely to be crowded out of no care (Panel B, column 6). To the extent that some children have other serious illnesses (such as typhoid or pneumonia) rather than malaria, this crowd-out may delay prompt diagnosis and treatment of these other illnesses. On the other hand, we see no effect of the health center crowd-out on access

to malaria testing for children (Panel A, column 7). This is likely due to the policy of the Kenyan Ministry of Health at the time of the study to treat all children under five with signs of malaria with ACTs (regardless of whether or not they received a formal diagnostic test).

Second, we exploit the predicted positivity measure we constructed from reported symptoms to study how treatment effects vary with a continuous measure of malaria risk. Figure 4 plots the following local linear regressions by treatment group:

$$y_{eh} = g\left(predpos_{eh}\right) + \varepsilon_{eh} \tag{3}$$

where $y_{eh}$ is the outcome of interest and $predpos_{eh}$ is our measure of predicted malaria positivity for illness episode $e$ of household $h$. Solid gray vertical lines demarcate overall tertiles of predicted positivity. Like the analysis by age group, the results show that the subsidy increases ACT access for both low and high positivity patients. While crowding into the drug shop occurs across all tertiles of predicted positivity, crowding out from seeking no care is concentrated in the lower tertile of predicted positivity and crowding out from the health center is concentrated in the middle tertile of predicted positivity.

Panel A of Figure 4 also presents suggestive evidence that lower subsidies are more effective: reducing the subsidy from 92 percent to 88 or 80 percent steepens the ACT use-predicted positivity gradient, which implies that lower subsidy levels reallocate ACTs to patients who are more likely to have malaria. The next subsection studies targeting more directly by using our administrative drug shop data, which has information on actual malaria positivity from surprise RDT tests. Note that since we find no impact of the subsidies on ACT access at the health center, our drug shop data should give a relatively complete picture of how changing the retail sector ACT subsidy level impacts targeting.

## 5.3 Targeting of Retail-Sector Subsidized ACTs

To estimate how ACT targeting varies with the ACT subsidy level we limit our sample to the subset of "ACT Subsidy Only" households randomly selected for a surprise RDT test and run the following regression:

$$pos_h = \beta_0 + \beta_1 ACT88_h + \beta_2 ACT80_h + \varepsilon_h \tag{4}$$

where $pos_h$ indicates whether the first patient seeking treatment with an ACT voucher in household $h$ tested positive for malaria and $ACT88_h$ and $ACT80_h$ are dummy variables indicating whether household $h$ was selected for the 88 and 80 percent subsidy levels respectively. Note that since very few households bought ACTs from the drug shop at the

non-subsidized price, we focus on the comparison between the three subsidy levels (92, 88 and 80 percent). Thus the omitted category in these regressions is the AMFm target subsidy level (92 percent).

The results are presented in Panel A of Table 3, column 1. Over-treatment is clearly a large problem at the highest subsidy level – only 56 percent of patients taking ACTs from the drug shop tested malaria positive (see "DV mean" in Panel A). The two lower subsidy levels are associated with much higher malaria positivity rates: drug shop ACT-takers are 18-19 percentage points more likely to be malaria-positive under the 88 and 80 percent subsidies than under the 92 percent subsidy.

## 5.4 ACT Subsidy Level and Targeting: Mechanisms

There are two main ways through which lowering the subsidy level can change the composition of ACT-takers. First, higher prices could select a different set of households into treatment-seeking at the drug shop. We find no evidence for this in our data: Panel B of Table 3 shows that there is no significant reduction in the overall share of households redeeming at least one ACT voucher (column 1); and we find no significant changes in average demographic characteristics of treatment seekers as the ACT subsidy level changes (results not shown).

Second, higher prices could lead to within-household selection, whereby households reserve vouchers for individuals who are more likely to be malaria positive when the ACT price is higher. We find strong evidence that this is the case, with two complementary forces at work: the first force is a reallocation from more expensive ACT doses (adult patients) to less expensive ACT doses (child patients). Panel B of Table 3 illustrates this effect – while voucher use for younger children is unaffected by the subsidy level, voucher use for individuals aged 14 and over declines significantly as the subsidy level is lowered.

The second force is a reallocation, *within* age/dose category, to episodes most likely to be malaria. This is particularly pronounced for adults: Panel A of Table 3 shows that those adults selected out by higher prices are least likely to be malaria positive – though our power is limited by a small sample size, adult ACT takers at lower subsidy levels are roughly twice as likely to test positive for malaria.[16]

The targeting effect, like the access effect shown in Table 2, appears to be non-linear in price: it occurs when the subsidy goes from 92 to 88 percent, with no further improvement in targeting for further subsidy decreases. This non-linearity is likely explained by the fact that the cheapest alternative (non-ACT) antimalarial treatments cost around 35-50 KSh for

[16]We also find that conditional on dose price, ACT takers under the highest subsidy are 11 percentage points less likely to test positive (significant at the 10 percent level).

an adult dose – which falls in-between the ACT prices under the 92 percent (40 KSh) and 88 percent (60 KSh) subsidy levels. Adults with a low perceived chance of malaria thus appear to choose the cheapest antimalarial available. This implies that ACT subsidies that bring the ACT price on par with that of older, less effective antimalarials may fail to optimally exploit individuals' private information on their underlying malaria risk.

Overall, these results suggest that households have information regarding the malaria status of a given illness episode, and are willing to pay more to treat higher probability malaria episodes. As a result, slightly higher prices do not significantly reduce access among those who need ACTs most – namely, children, but dissuade low-positivity adults from purchasing ACTs in the retail sector, and these patients do not simply compensate by acquiring ACTs in the public sector.[17] Information on illness-specific malaria status is not perfect, however. Even at the lowest ACT subsidy level we consider, 25 percent of ACTs purchased at the drug shop go to malaria-negative patients. This suggests a need for improved access to malaria diagnostics. The next section asks whether introducing an RDT subsidy in the retail sector can fill that need.

# 6 Results: Impact of Adding an RDT Subsidy

## 6.1 Provider Choice and Diagnostic Testing

Table 4 presents estimates of the impacts of the RDT subsidy on where treatment is sought and whether a malaria test is taken. As in Table 2, this analysis is based on the endline data. We drop all households in the control (no subsidy) group (since none of them were given an RDT voucher).[18] We estimate the RDT treatment effects separately by ACT subsidy level

[17] One concern is that at higher prices, adults will simply choose to take partial, subtherapeutic doses of ACTs. While we have evidence that suggests this was not a problem in our context (around 96 percent of ACT takers in all treatment arms reported taking the full dose), our surveyors posted at the drug shops throughout the study period were instructed to never allow the sale of a partial dose to a client, or the sale of a child dose to an adult patient. The surveyors also gave detailed instructions on the importance of taking a full dose. Thus there is reason to think that partial dosing may be a bigger problem in equilibrium. Additional research is needed to gauge how common partial dosing is, how it is impacted by ACT price, and how to best prevent it.

[18] We also drop all surprise tested households in the ACT subsidy only group, since the surprise RDT could have affected the final treatment decision reported at endline. We do not exclude households sampled for a surprise test if they were also sampled to receive RDT vouchers. That is because over 80 percent of them elected to redeem their RDT voucher anyway, conditional on visiting the drug shop (where they would otherwise have been surprise tested), and F-tests of the significance of surprise testing selection confirm that the surprise testing had no significant impact on behavior for this group. Our results are largely unchanged, though somewhat less precisely estimated given the drop in sample size, when excluding these households.

with the following regression:

$$y_{eh} = \beta_0 + \beta_1 RDT_h \times ACT92_h + \beta_2 RDT_h \times ACT88_h + \beta_3 RDT_h \times ACT80_h + \beta_4 ACT88_h + \beta_5 ACT80_h + x_h'\gamma + \lambda_{strata} + \varepsilon_{eh} \quad (5)$$

The first three columns of Table 4 suggest no impact of the RDT subsidy on where people seek treatment, irrespective of what the ACT subsidy level is. All the coefficient estimates are insignificant and trivial in magnitude. This suggests that the perceived value of RDTs was too low to substantially change treatment seeking behavior, possibly due to the fact that people had no prior experience with RDTs and may have been very uncertain about the test's accuracy.

However, our results do suggest that individuals saw *some* value to taking the test: the RDT subsidy increased the share of illness episodes tested for malaria by 15 to 25 percentage points (column 4 of Table 4). This corresponds to a doubling in the rate of malaria testing on average across all ACT subsidy levels. This large effect on testing comes from the fact that, conditional on seeking care at the drug shop, households redeemed their RDT voucher at a very high rate (82 percent). Note that rates of test taking were high even when the individual had to pay 15 KSh ($0.19) for the test (Appendix Table A7).

## 6.2 RDTs and Targeting of Retail-Sector Subsidized ACTs

We now move to studying the targeting effects of RDTs in our administrative drug shop data. As highlighted by the theoretical framework, RDT provision can impact targeting via the extensive margin (by selecting individuals with different likelihoods of being malaria positive into treatment-seeking at the drug shop) and the intensive margin (individuals who would have gone to the drug shop anyway are now able to view a test result before deciding to purchase an ACT). Given this, Table 5 presents regression analyses, unpacking selection into treatment seeking (columns 1 and 2) and ACT taking (column 3).

Panel A, column 1, shows that the RDT subsidy has only a modest (borderline significant) impact on the share of households seeking treatment at the drug shop.[19] This confirms our results from the endline data in Table 4. Furthermore, column 2 of Panel A shows that overall, the RDT subsidy has no impact on the share of treatment seekers who are malaria positive. Combined, these results suggest that there is essentially no extensive margin effect of RDTs.

Turning to the intensive margin, column 3 estimates how malaria positivity among pa-

[19] We control for the ACT subsidy level in both Panels A and B. As in Table 3, we limit the sample of households to those selected for a surprise RDT test.

tients who ultimately elect to take the ACT varies with the RDT subsidy. We find that ACT takers are 9 percentage points more likely to be malaria positive in the presence of a retail-sector RDT subsidy (off of a base of 68 percent across all ACT price groups in the no-RDT group). This comes from the fact that nearly 40 percent of those testing negative for malaria decide not to purchase an ACT (i.e., they "complied" with the test result).

Taken together, the results so far suggest that the RDT subsidy was somewhat effective at reducing over-treatment and thus improving targeting. Panel B of Table 5 shows that RDTs appear to have the largest targeting benefits when ACTs are subsidized the most (92 percent). However, this result is largely driven by what appears to be a positive selection into the drug shop (column 2). There is no compelling theoretical explanation for this positive selection, so we consider the positive retail-sector targeting impact of the RDT subsidy observed in Table 5 as a likely upper bound.[20]

## 6.3 Compliance with RDT results

There are two main reasons why RDT subsidies have only moderate impacts on targeting of ACT subsidies in our data. First, a good proportion of illness episodes are among children, who have a very high chance of truly needing an ACT anyway, so there is little room for RDTs to improve targeting among them.

Second, compliance with RDT test results (in terms of ACT treatment-seeking) was partial. We show this in Figure 5. Compliance with negative test results increases (as expected) as the ACT subsidy level decreases, but at best less than half of those testing negative choose to forgo the ACT. While we explicitly advised that patients aged five and under take an ACT regardless of test result (consistent with WHO and Kenyan Ministry of Health guidelines at the time of the study), 49 percent of patients over five still took an ACT when RDT negative.

This cautiousness in complying with test results is not entirely surprising given the fact that the status quo diagnostic technology (microscopy) is often ignored by health practitioners and has a high rate of false negatives (see footnote 3). While RDTs have a much lower rate of false negatives than microscopy (5 percent versus 31 percent, as mentioned earlier), it might take some time for households to learn this.

Another possible explanation for the high ACT purchase rate after a negative RDT result is hoarding – households might have decided to buy the ACT dose to keep it for later (the

[20] One potential concern is that the positivity differences at the 92 percent ACT subsidy level are driven by hoarding behavior (i.e. individuals in the high subsidy-no RDT treatment rushed to purchase ACTs before getting sick to "cash in" on the subsidy). However, the RDT treatment had no significant impact on the time-to-voucher-redemption for all three ACT subsidy levels, which suggests that this is not the case.

next malaria episode). Such hoarding could have been encouraged by the experimental design, if households were afraid the vouchers would expire or that the supply of ACTs at drug shops would dry up. In practice, hoarding did not seem to be common, however, as evidenced by the following facts: (1) only 16 percent of households used both ACT vouchers by the end of the study; (2) we see no relationship between the timing of the first voucher redemption and malaria status at the time of redemption; and (3) just 12 percent of households redeemed a voucher in the first week following voucher distribution – this is identical to the share of households who had a member who was currently sick with malaria in our follow up symptoms database. Nevertheless, to the extent that lack of information and hoarding would disappear in the long run, our results represent a lower bound on RDT compliance (and therefore the targeting benefits of an RDT subsidy).

# 7 Discussion

## 7.1 Cost Effectiveness

So far our discussion of the results has focused on access and targeting, but as discussed in Section 3, the key policy outcomes of interest are under-treatment and over-treatment. Table 6 uses the evidence above, combined with some needed assumptions, to estimate the exent of under- and over-treatment under three regimes of interest: the AMFm "status quo", an 80 percent ACT subsidy with no RDT, and an 80 percent ACT subsidy with an RDT subsidy. Over-treatment decreases monotonically from one regime to the next, reflecting the combined effect of increased targeting and small declines in access. Interestingly, under-treatment also decreases as the ACT subsidy level decreases. This result is the direct consequence of our finding that ACT access does not meaningfully decrease when the subsidy level decreases, but targeting substantially improves.

In our context it is quite clear that the lower subsidy is more cost effective: under- and over-treatment are lower under this regime *and* the subsidy cost is lower. Adding an RDT subsidy to the ACT subsidy does not appear cost-effective in the short-run, however: the 80 percent ACT subsidy with no RDT subsidy performs almost as well in terms of targeting as compared to the same subsidy level plus an RDT subsidy (indeed, we cannot reject that they are identical), but does not incur the additional cost of subsidizing RDTs (around $1 per test).

This does not imply that RDTs do not have the *potential* to be cost-effective. As discussed earlier, there are reasons to think that RDT compliance would improve over time, provided people learn about their accuracy. What's more, an important benefit of RDTs

that is not captured by our calculations is that they may increase the likelihood that a non-malaria illness is treated with appropriate medication promptly. Given that pneumonia, a bacterial illness whose symptoms often overlap with those of malaria, is the largest cause of childhood mortality, this benefit could be substantial, even if individuals who test RDT negative continue to take ACTs as a precaution. The cost-effectiveness of RDT subsidies could also depend critically on the level of malaria infection in a region, with areas of lower endemicity offering potentially more gains to RDTs.

RDT results may also help households learn about the effectiveness of ACTs: if an illness doesn't get better after taking an ACT, the household might not use this as a signal that ACTs are ineffective if the RDT test was negative. This effect could be very important. Adhvaryu (2011) presents evidence from Tanzania suggesting that individuals are more likely to go seek free ACTs at their local health center when the rate of over-treatment with ACTs in their neighborhood in the previous six weeks was lower. This is consistent with a model in which households interpret non-recovery among ACT-takers as a negative signal about the effectiveness of ACTs, rather than revise the diagnosis. Expanding access to accurate diagnosis could greatly reduce this type of incorrect inference.

Our finding that many households are willing to pay for an RDT even if they take an ACT regardless of the result suggests that households do see some of these important benefits to testing.[21] Learning to fully trust the RDT result might require much more exposure than what we capture during our study period, however. Further research is needed to assess the long-run impact of expanding access to rapid diagnostic testing.

## 7.2 External Validity

While our study was carried out in only one region of Kenya, the malaria treatment seeking environment in our study is similar to a wide swath of the heavy malaria-burden regions in sub-Saharan Africa. Appendix Table A8 presents basic statistics from household surveys recently conducted in two regions of Uganda, two regions of Tanzania and the Southern region of Malawi.[22] Like our results in Western Kenya, these surveys reflect heavy reliance on the private/retail sector for malaria treatment, limited use of ACTs to treat malaria episodes and high out of pocket expenditures on (frequently experienced) suspected malaria

[21] Moreover, a geographical analysis of redemption patterns in our data shows that exposure to RDTs via neighbors increased demand for RDTs over the course of the study, suggesting important social learning effects (results available upon request).

[22] The surveys covered rural areas, town centers and some small urban areas, but did not include major cities. The surveys were conducted 1.5-2 years after the baseline survey conducted for this study. The data in columns (2) and (3) are from surveys that took place one month and three months into the AMFm launch in Uganda and Tanzania, respectively, but in both cases a very limited quantity of subsidized ACTs had arrived in country at that time.

episodes. All surveys also reveal limited rates of blood test diagnosis for such episodes. What's more, as mentioned earlier, the distribution of malaria positivity by age that we observe in Kenya is very comparable to that observed in Uganda (see Figure 3).

Another important question is whether the subsidy regimes we created through our experimental voucher system adequately simulated what would happen under a large-scale subsidy such as the AMFm. In particular, one concern is that the longer term effects of a given subsidy scheme may be different from those we observe in the short-run. For example, households' demand for ACTs could change after they have had a chance to experiment with them. If demand for ACTs rises over time, the "right" price to balance access and targeting will also rise over time. Data that we collected from our study households in 2011, a year after the AMFm pilot subsidy was introduced in Kenya with the target level of 92 percent, suggests that over-treatment under the actual AMFm regime was remarkably comparable to that observed in our experiment: Just 45 percent of patients who fell sick in the past 3 days and took ACTs tested positive for malaria. This is relatively close to our estimate of 56 percent targeting under the 92 percent subsidy, and suggests that the demand patterns observed in our voucher experiment can provide useful insights despite the short time horizon and partial equilibrium setting.

# 8 Conclusion

There is a large class of health issues for which both under-treatment and over-treatment generate negative spillovers. Under-treatment is a public bad for any communicable disease, since the number of untreated individuals increases transmission rates. Over-treatment is a public bad whenever the cost of treatment is subsidized. Over-treatment is also a public bad when it leads to improper treatment for the true cause of illness and to drug resistance. For any such health issue, it is critical to find the right balance between, on the one hand, access and affordability when the medicine is truly needed, and on the other hand, disincentive to overuse the medicine.

Malaria is one of the most common (and deadly) illnesses in this class of health issues, killing over 600,000 people each year, partly because of lack of access to effective treatment. At the same time, parasite resistance to treatment has been developing faster and faster with each new generation of antimalarials. Learning how to reduce malaria mortality and morbidity through prompt access to effective treatment, while at the same time limiting resistance to the latest generation of antimalarials, the ACT, is one of the most pressing and important questions facing the global health community today.

This question is currently under intense debate and scrutiny. The AMFm, controversial

from the beginning (among other things, it was never supported by the US government), has received a great deal of criticism for a lack of evidence regarding its impact on under- and over-treatment, especially among poor and vulnerable groups (Oxfam 2012). Furthermore, there is no evidence on whether simple changes to the AMFm (such as reducing the subsidy level or subsidizing diagnostic tests) could improve program performance. Our detailed data on treatment-seeking behavior for over 2,700 households in a malaria-endemic area of Kenya, combined with our experimental design, sheds critical light on all the essential pieces of the puzzle: the price elasticity of demand for effective medication, how demand for ACTs varies by malaria risk level, and how access to proper diagnosis affects the demand for medication and targeting. Our analysis leads to five important findings.

First, the ongoing public sector subsidy for ACTs falls far short of the goal to guarantee access to those most vulnerable to malaria, in part because rural households tend to favor treatment-seeking at the drug shop over public health facilities. Second, the demand for ACTs appears very low at unsubsidized prices, but substantial and inelastic over a range of subsidized prices. Taken together, these first two results suggest that retail-sector subsidies for ACTs are clearly needed to increase ACT access among those that suffer from malaria, but these subsidies may not need to be as large as currently planned by the donor community. Third, over-treatment of malaria is extremely common; therefore large ACT subsidies alone would lead to an important increase in inappropriate use of ACTs. Fourth, over the subsidy range considered by the international community, price is a useful tool for selection: somewhat higher ACT prices reduce ACT taking among adults, who are substantially less likely to be malaria positive, while leaving access among children unchanged. Fifth, demand for rapid diagnostic testing is extremely high when it is readily affordable and available, although compliance with the test results would need to increase for diagnostic testing to substantially improve ACT targeting.

The fact that improved access to diagnostic tests does not solve over-treatment with subsidized ACTs (at least in the short run) is not entirely surprising: households in endemic areas of Kenya, and likely in most of sub-Saharan Africa, face a complicated inference problem. There are three unknowns: the true underlying cause of an illness episode, the relative efficacy of ACTs compared to other treatments (or no treatment) if one truly has malaria, and the accuracy of diagnostic tests. But none of the signals that households receive are very good: since most diseases are self-limiting, a non-malaria episode may appear to benefit from ACT treatment even though it would have resolved equally rapidly without treatment. Likewise, ACTs may appear ineffective when they are used to treat non self-limiting, non-malarious episodes. Adding signals through the provision of highly reliable RDTs in the retail-sector should help households with this inference problem, but only over some time.

RDTs are thus not a silver bullet, at least in the short run. Additional research is needed to understand how best to facilitate learning and enhanced RDT compliance under a bundled subsidy regime.

Many other questions regarding the supply side of the subsidy policy remain unanswered. For example, drug shops, which make a profit from selling antimalarials whether their clients are truly malaria positive or not, might not have any incentive to sell a cheap diagnostic test that will result in fewer drug purchases – their incentives would depend on the relative profit margins associated with antimalarials and RDTs and underlying malaria endemicity (Cohen and Dickens 2012). The problem of RDT provision is thus an incentive problem similar to that of "informed experts" who sell both their diagnostic of a problem and the solution to the problem, such as surgeons or auto repair shops (Wolinsky 1993). Future research on optimal provider incentives and other supply side issues is therefore needed to support further innovations in malaria subsidy policy.

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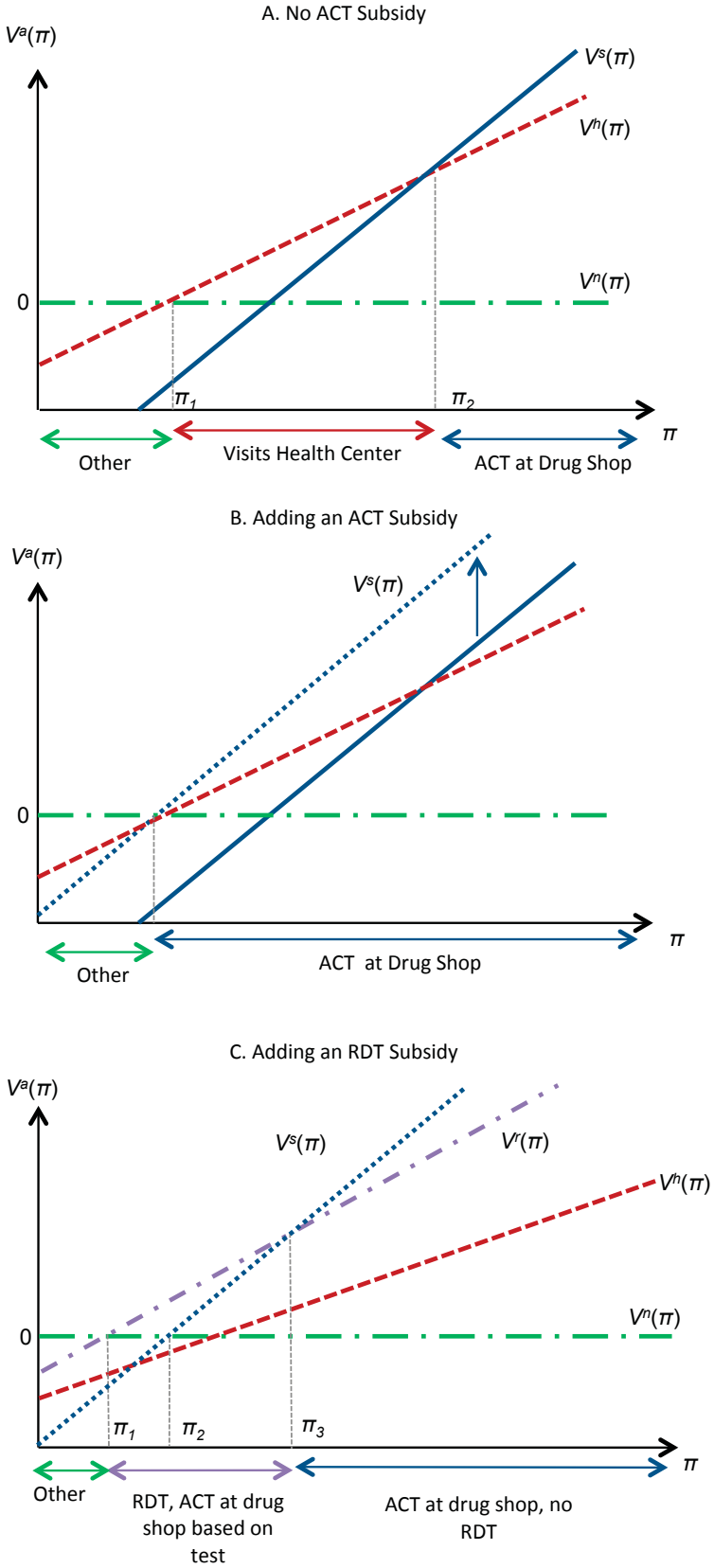
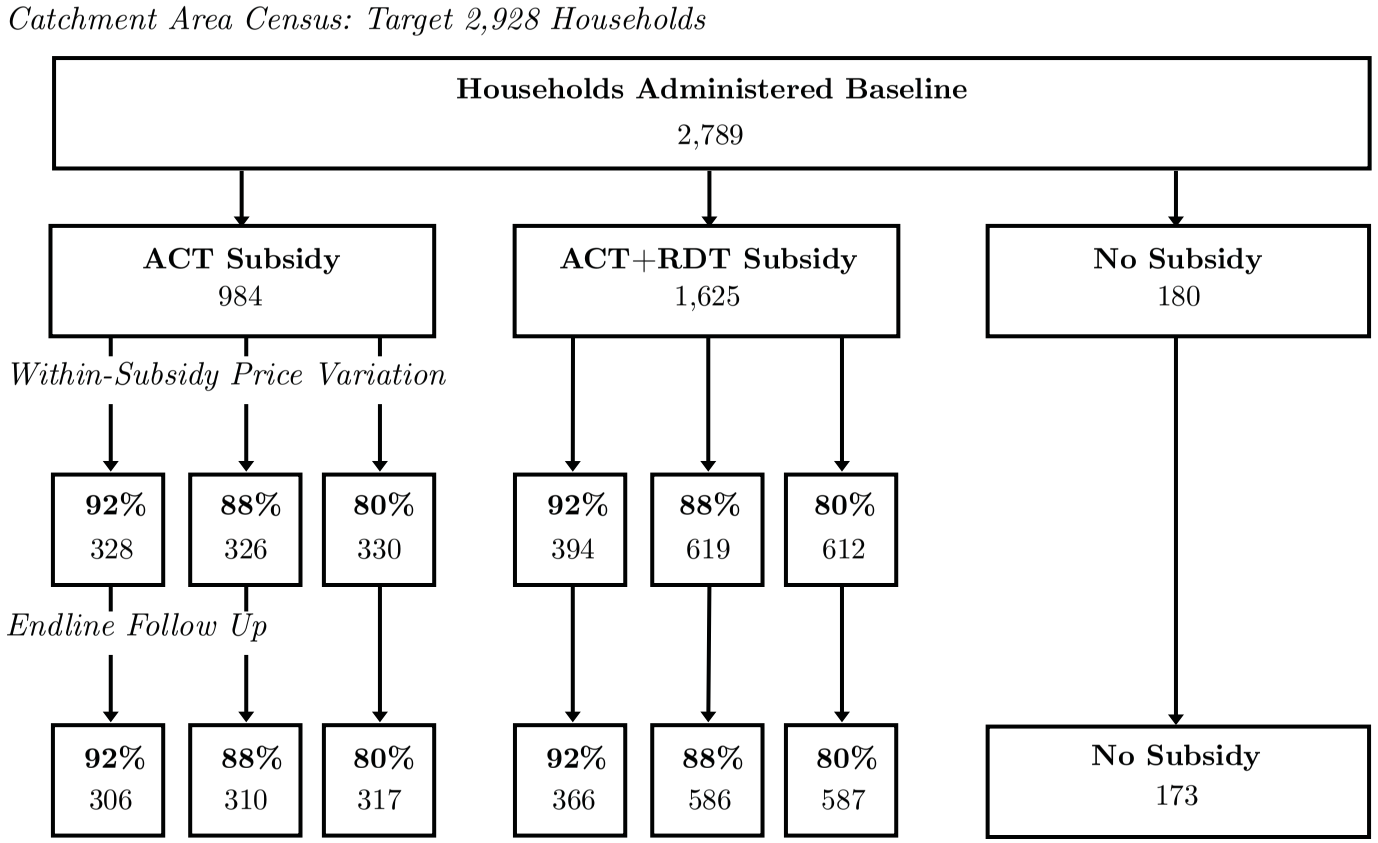
Figure 1. Theoretical Impact of ACT and RDT Subsidies



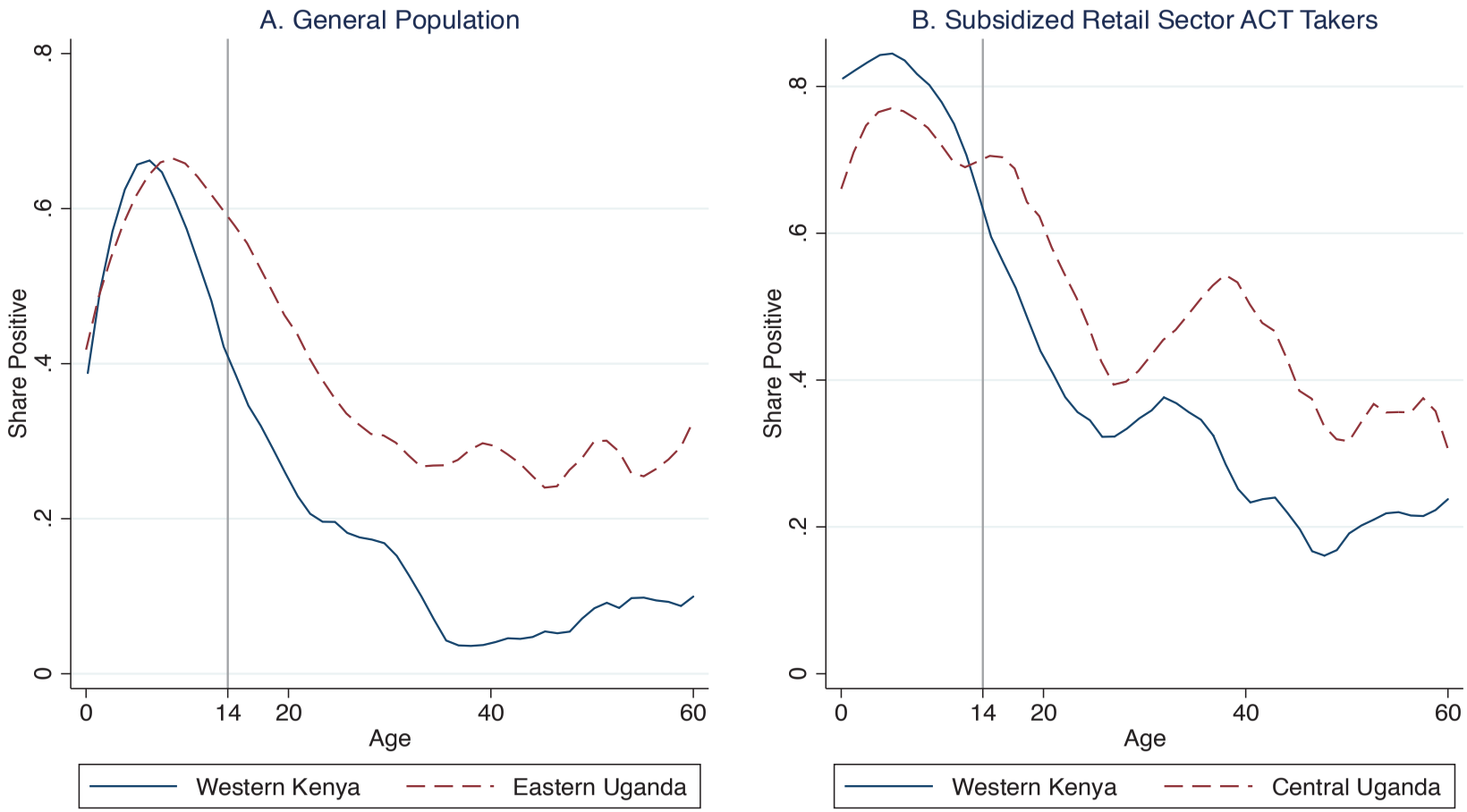

Notes: $\pi$ is the (perceived and actual) probability that the illness episode is malaria. $V^s$ is the value of purchasing an ACT at the drug shop; $V^h$ is the value of visiting a health center and receiving free ACT if positive; $V^n$ is the value of doing neither of the two options above. The value functions are normalized so that $V^n(\pi)=0$ for all $\pi$. Panel C: $V^r$ is the value of getting an RDT and purchasing an ACT at drug shop if RDT is positive.

Figure 2. Experimental Design and Attrition: Number of Households per Study Arm



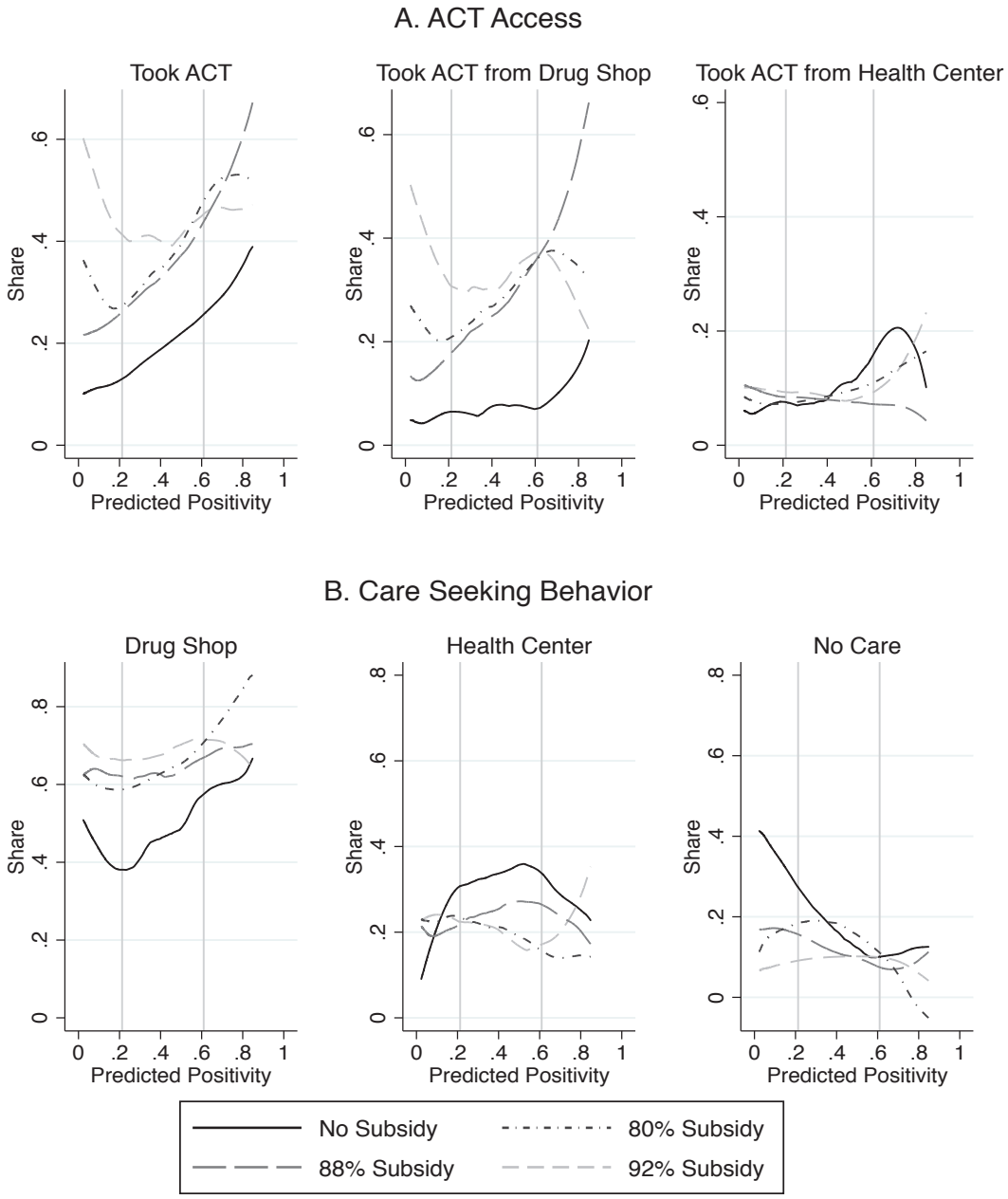

Notes: 49 percent of ACT subsidy only households and 80 percent of ACT+RDT subsidy households were selected for surprise RDT testing at the drug shop. Within each ACT subsidy level, those in the ACT+RDT subsidy group were also randomized into three RDT subsidy levels. Since we find no differences across RDT subsdidy levels we lump them together for simplicity. Details for the impact of the different RDT subsidies are provided in Appendix Table A7.

Figure 3. Malaria Positivity Among General Population and Subsidized ACT Takers - Kenya and Uganda



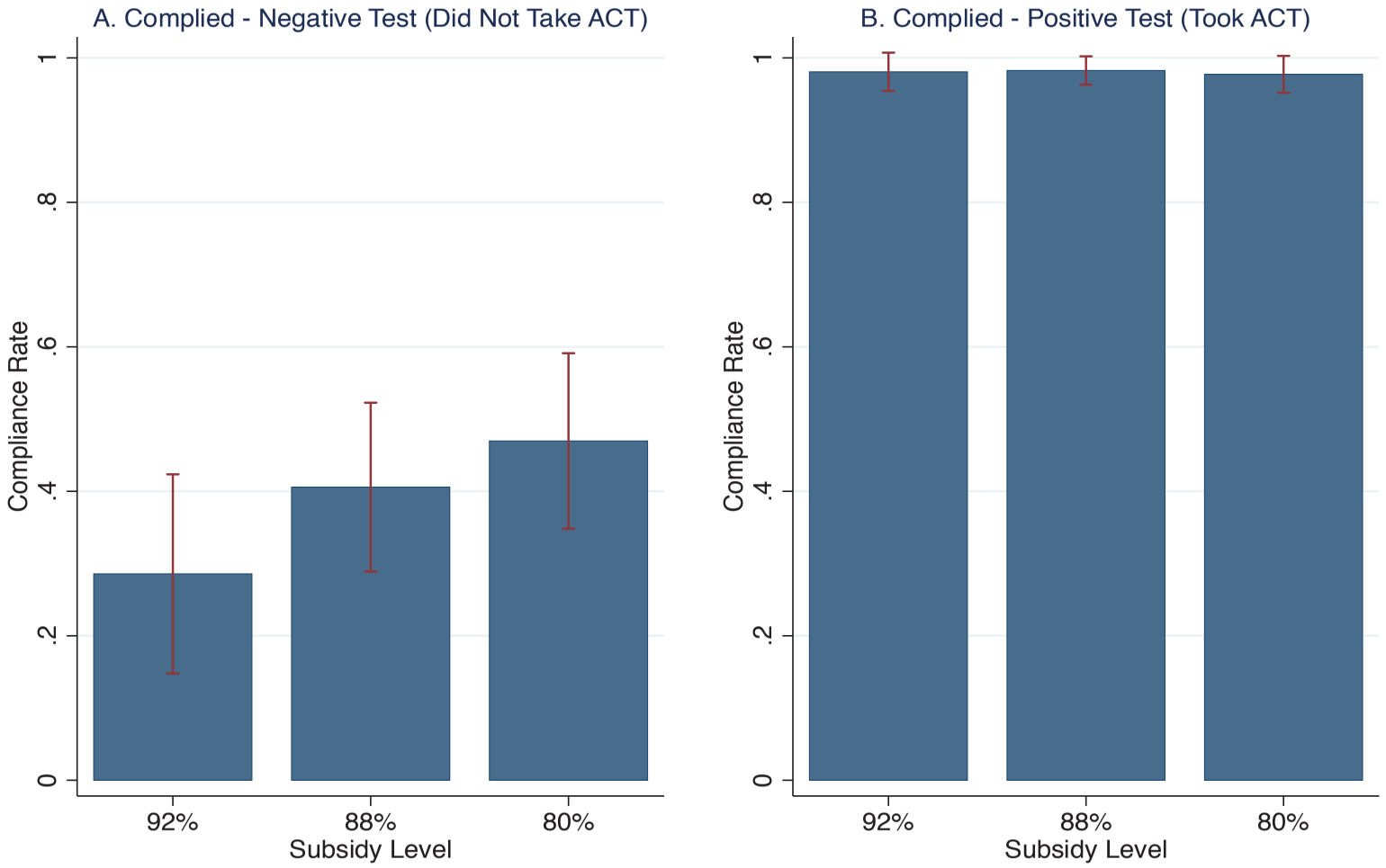

Notes: Local linear regression results. Age is topcoded at 60 for legibility. Data sources: Panel A Kenya data is from symptoms database. Positivity data obtained from RDT tests administered to currently sick individuals who fell sick within three days of visit. See Appendix Table A8, note b for Uganda souce. Positivity data is from all individuals in household available to take RDT test, regardless of illness status. Panel B Kenya data is from administrative drug shop data, all ACT subsidy levels, no RDT subsidy. Positivity data is from surprise RDT tests of ACT takers. See Appendix Table A8, note c for Uganda source.

Figure 4. Impact of Retail Sector ACT Subsidy on Provider Choice and ACT Access




Notes: Data source: endline survey. Excludes households randomly selected for surprise RDT testing at the drug shop. Local linear regressions estimates. Results for the observations with the upper- and lower-most 2.5 percent of predicted positivity are omitted to avoid illustrating imprecisely estimated tails. Gray vertical lines demarcate tertiles. Tertiles calculated using all first illness episodes for both treatment groups and the control group. Confidence intervals are omitted from the figure for legibility, but Appendix Table A5 presents regressions that test whether treatment effects are significantly different from zero by tertile.

Figure 5. Compliance with Malaria Test Results




Notes: Data source: administrative drug shop data. Sample in Panel A limited to patients who redeemed an RDT voucher and received a negative test result. Sample in Panel B limited to patients who redeemed an RDT voucher and received a positive test result.

Table 1. Baseline Summary Statistics

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | | Regression Coefficients and Standard Errors | | | | Joint Test: | |
| | Control Group Mean | 92% ACT Subsidy (T1) | 88% ACT Subsidy (T2) | 80% ACT Subsidy (T3) | RDT Subsidy (T4) | ACT Subsidies = 0 | N |
| *Characteristics of Interviewed Household Head* | | | | | | | |
| Female | 0.867 | 0.017 | 0.029 | 0.040 | 0.010 | 1.22 | 2789 |
| | [0.341] | (0.029) | (0.028) | (0.028) | (0.012) | {0.299} | |
| Age (years) | 41.7 | -1.98 | -3.22** | -2.44* | 0.185 | 2.09* | 2646 |
| | [17.3] | (1.46) | (1.44) | (1.45) | (0.626) | {0.099} | |
| Education (years) | 5.10 | 0.141 | 0.381 | 0.151 | 0.169 | 0.868 | 2774 |
| | [4.00] | (0.343) | (0.341) | (0.342) | (0.161) | {0.457} | |
| Literate | 0.575 | 0.047 | 0.050 | 0.027 | 0.000 | 0.802 | 2782 |
| | [0.496] | (0.042) | (0.042) | (0.042) | (0.020) | {0.493} | |
| Married | 0.783 | -0.015 | 0.004 | 0.006 | -0.015 | 0.422 | 2784 |
| | [0.413] | (0.035) | (0.035) | (0.034) | (0.016) | {0.737} | |
| Subsistence Farmer | 0.589 | 0.052 | 0.039 | 0.059 | -0.005 | 0.814 | 2787 |
| | [0.493] | (0.042) | (0.042) | (0.042) | (0.019) | {0.486} | |
| Number Dependents | 4.12 | -0.263 | -0.096 | -0.077 | 0.021 | 1.07 | 2663 |
| | [2.78] | (0.223) | (0.221) | (0.222) | (0.098) | {0.360} | |
| *Household Characteristics* | | | | | | | |
| Number members | 5.48 | -0.354 | -0.233 | -0.197 | 0.024 | 1.18 | 2789 |
| | [2.77] | (0.217) | (0.214) | (0.215) | (0.092) | {0.316} | |
| Fraction Adults (Ages 14+) | 0.623 | -0.035* | -0.048*** | -0.024 | 0.002 | 2.90** | 2337 |
| | [0.235] | (0.020) | (0.019) | (0.020) | (0.009) | {0.034} | |
| Acres Land | 2.72 | -0.660** | -0.601* | -0.571* | 0.197* | 1.35 | 2250 |
| | [3.69] | (0.330) | (0.327) | (0.324) | (0.117) | {0.258} | |
| Distance from drug shop (km) | 1.68 | 0.012 | 0.012 | 0.002 | 0.010 | 0.334 | 2788 |
| | [0.917] | (0.023) | (0.022) | (0.022) | (0.011) | {0.801} | |
| Distance from closest clinic (km) | 6.57 | -0.018 | -0.036 | -0.043 | 0.044* | 0.308 | 2785 |
| | [2.47] | (0.060) | (0.059) | (0.059) | (0.027) | {0.820} | |
| *Baseline Malaria Knowledge and Health Practices* | | | | | | | |
| Number bednets | 1.77 | -0.031 | -0.060 | 0.028 | 0.005 | 0.635 | 2784 |
| | [1.43] | (0.120) | (0.121) | (0.120) | (0.057) | {0.593} | |
| Share HH members slept under n | 0.561 | 0.023 | 0.006 | 0.030 | -0.012 | 0.690 | 2661 |
| | [0.397] | (0.034) | (0.034) | (0.034) | (0.017) | {0.558} | |
| Heard of ACTs | 0.399 | 0.016 | 0.017 | 0.030 | 0.001 | 0.243 | 2771 |
| | [0.491] | (0.042) | (0.041) | (0.042) | (0.020) | {0.866} | |
| Heard of RDTs | 0.128 | 0.039 | 0.020 | 0.021 | -0.011 | 0.790 | 2786 |
| | [0.335] | (0.030) | (0.029) | (0.029) | (0.014) | {0.499} | |
| Treats water regularly | 0.408 | -0.036 | -0.018 | 0.004 | 0.023 | 1.000 | 2779 |
| | [0.493] | (0.041) | (0.041) | (0.041) | (0.019) | {0.392} | |
| Number of presumed malaria | 1.20 | 0.015 | -0.008 | -0.029 | 0.033 | 0.169 | 2789 |
| episode last month | [1.22] | (0.102) | (0.103) | (0.103) | (0.050) | {0.918} | |
| *Cost per Episode (Among Those Seeking Care)* | | | | | | | |
| Total Cost (US $) | 1.63 | 0.140 | -0.040 | -0.217 | 0.131 | 0.964 | 1319 |
| | [1.86] | (0.293) | (0.250) | (0.238) | (0.174) | {0.409} | |

Notes: Data source: Baseline survey. The first column shows average values of characteristics for the control group. Columns 2-5 show regression coefficients and standard errors on indicated treatment groups (the omitted category is the control group). All regressions include a full set of strata dummies. Column 6 shows F-statistics and p-values from a test of whether the three ACT subsidy coefficients are jointly equal to zero. Standard deviations are in brackets, standard errors are in parentheses, and p-values are in braces. All tests are based on heteroskedasticity robust standard errors. ***, **, and * indicate significance at the 1, 5, and 10 percent levels respectively. The exchange rate at the time of the study was around 78 Ksh to US$1.

Table 2. Impact of ACT Subsidy on Treatment Seeking and ACT Access

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | Took ACT | Took ACT from Drug Shop | Took ACT from Health Center | Visited Drug Shop | Visited Health Center | Sought No Care | Took Malaria Test |
| ACT Subsidy = 92% | 0.225*** | 0.249*** | -0.024 | 0.159*** | -0.055 | -0.110*** | -0.003 |
| | (0.053) | (0.046) | (0.037) | (0.058) | (0.053) | (0.042) | (0.047) |
| ACT Subsidy = 88% | 0.161*** | 0.217*** | -0.056 | 0.167*** | -0.070 | -0.097** | -0.017 |
| | (0.050) | (0.043) | (0.037) | (0.058) | (0.052) | (0.042) | (0.046) |
| ACT Subsidy = 80% | 0.178*** | 0.206*** | -0.035 | 0.173*** | -0.106** | -0.085* | 0.029 |
| | (0.048) | (0.042) | (0.035) | (0.054) | (0.047) | (0.045) | (0.045) |
| P-value: 92%=88%=80%=0 | 0.000*** | 0.000*** | 0.498 | 0.004*** | 0.164 | 0.048** | 0.804 |
| DV Mean (Control Group) | 0.190 | 0.071 | 0.119 | 0.488 | 0.286 | 0.226 | 0.190 |
| N | 631 | 631 | 631 | 631 | 631 | 631 | 631 |

Notes: Data source: endline survey. Sample excludes all households selected for a surprise or subsidized RDT. The unit of observation is the first illness episode with at least one malaria-like symptom that the household experienced following the baseline. A few households have multiple observations if multiple household members were ill simultaneously. Robust standard errors clustered at the household level in parentheses. All regressions control for household head age and a full set of strata dummies. ***, **, and * indicate significance at the 1, 5, and 10 percent levels respectively.

Table 3. Impact of Retail-Sector ACT Subsidy on Targeting of Subsidized ACTs at Drug Shops

| | (1) | (2) | (3) |
|---|---|---|---|
| | A. Dependent Variable: Surprise RDT reveals that subsidized ACT-taker is malaria-positive | | |
| | All | Patients Under Age 14 | Patients Aged 14 and Older |
| ACT Subsidy = 88% | 0.187** | 0.060 | 0.256* |
| | (0.081) | (0.082) | (0.148) |
| ACT Subsidy = 80% | 0.182** | 0.066 | 0.170 |
| | (0.084) | (0.083) | (0.160) |
| P-value: 88%= 80% = 0 | 0.038** | 0.687 | 0.192 |
| DV Mean (ACT 92%, no RDT) | 0.563 | 0.791 | 0.214 |
| N | 190 | 132 | 58 |
| | B. Dependent Variable: Household redeemed... | | |
| | ...at least one ACT Voucher | ...their first ACT Voucher for a Patient Under Age 14 | ...their first ACT Voucher for a Patient Aged 14 and older |
| ACT Subsidy = 88% | -0.022 | 0.035 | -0.057** |
| | (0.038) | (0.035) | (0.027) |
| ACT Subsidy = 80% | -0.050 | 0.031 | -0.080*** |
| | (0.038) | (0.034) | (0.026) |
| P-value: 88%= 80% = 0 | 0.411 | 0.540 | 0.007*** |
| DV Mean (ACT 92%, no RDT) | 0.439 | 0.268 | 0.171 |
| N | 984 | 984 | 984 |

Notes: Data source: administrative drug shop data. Heteroskedasticity robust standard errors in parentheses. The omitted category is the 92% ACT subsidy group. Sample in Panel A limited to households who were selected for a surprise RDT test and purchased an ACT on their first visit to the drug shop. Regressions in Panel B control for a full set of strata dummy variables and age of the household head. Regressions in Panel A omit strata and age controls so as not to absorb selection effects, which these regressions aim at identifying. ***, **, and * indicate significance at the 1, 5, and 10 percent levels respectively.

Table 4. Impact of RDT Subsidy on Treatment Seeking and ACT Access by ACT Price

| | (1) Visited Drug Shop | (2) Visited Health Center | (3) Sought No Care | (4) Took Malaria Test | (5) Took RDT Test | (6) Took Microscopy Test | (7) Took ACT |
|---|---|---|---|---|---|---|---|
| RDT Subsidy × 92% ACT Sub | -0.005 | -0.018 | 0.029 | 0.244*** | 0.263*** | -0.019 | 0.002 |
| | (0.048) | (0.042) | (0.032) | (0.044) | (0.034) | (0.034) | (0.050) |
| RDT Subsidy × 88% ACT Sub | 0.026 | -0.045 | 0.007 | 0.229*** | 0.229*** | 0.000 | 0.042 |
| | (0.046) | (0.041) | (0.030) | (0.038) | (0.030) | (0.032) | (0.044) |
| RDT Subsidy × 80% ACT Sub | -0.012 | 0.023 | -0.003 | 0.146*** | 0.166*** | -0.021 | 0.016 |
| | (0.043) | (0.035) | (0.033) | (0.039) | (0.029) | (0.030) | (0.041) |
| P-value: 92%=88%=80%=0 | 0.938 | 0.612 | 0.832 | 0.000*** | 0.000*** | 0.851 | 0.787 |
| DV Mean (ACT 92%, No RDT) | 0.667 | 0.222 | 0.104 | 0.194 | 0.069 | 0.125 | 0.444 |
| N | 1993 | 1993 | 1993 | 1993 | 1993 | 1993 | 1993 |

Notes: Data source: endline survey. Sample excludes all households who were selected for a surprise RDT but not an RDT subsidy. The unit of observation is the first illness episode that the household experienced following the baseline. A few households have multiple observations if multiple household members were ill simultaneously. Robust standard errors clustered at the household level in parentheses. All regressions control for ACT price dummies, household head age, and a full set of strata dummies. ***, **, and * indicate significance at the 1, 5, and 10 percent levels respectively.

Table 5. Impact of RDT Subsidy on ACT Targeting

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | | Dependent Variable: Surprise RDT reveals that patient is malaria-positive | | |
| | Dependent Variable: Sought Treatment at drug shop | Sample: patients who visited drug shop | Sample: patients who bought subsidized ACT at drug shop | *Proportion that redeemed RDT voucher, conditional on seeking treatment at drug shop* |
| *A. Across all ACT Subsidy Levels* | | | | |
| RDT Subsidy | 0.033* | 0.018 | 0.092*** | *0.809* |
| | (0.019) | (0.038) | (0.037) | |
| *B. By ACT Subsidy Level* | | | | |
| RDT Subsidy × 92% ACT Sub | 0.028 | 0.142** | 0.182*** | *0.781* |
| | (0.036) | (0.068) | (0.068) | |
| RDT Subsidy × 88% ACT Sub | 0.054 | -0.038 | 0.040 | *0.828* |
| | (0.033) | (0.061) | (0.060) | |
| RDT Subsidy × 80% ACT Sub | 0.017 | -0.056 | 0.050 | *0.808* |
| | (0.032) | (0.066) | (0.065) | |
| DV Mean (ACT 92%, No RDT) | 0.442 | 0.556 | 0.563 | -- |
| N | 2609 | 870 | 790 | *723* |

Notes: Data source: administrative drug shop data. Heteroskedasticity robust standard errors in parentheses. All regressions control for ACT price dummies. Regressions in column 1 also include strata and age controls. Columns 2 and 3 omit these controls so as not to absorb selection effects, which these regressions aim at identifying. ***, **, and * indicate significance at the 1, 5, and 10 percent levels respectively.

Table 6. Estimated Impacts of Various Subsidy Schemes on Under- and Over-Treatment

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | No Subsidy | ACT 92% Subsidy | ACT 80% Subsidy | ACT 80% + RDT Subsidy |
| *Experimental Estimates of Access and Drug Shop Targeting* | | | | |
| Total Share Taking ACT | 0.190 | 0.415 | 0.368 | 0.377 |
| Share Taking ACT at Drug Shop | 0.071 | 0.320 | 0.277 | 0.291 |
| Share Taking ACT at Health Center | 0.119 | 0.095 | 0.091 | 0.086 |
| Targeting at Drug Shop | 0.745 | 0.563 | 0.745 | 0.806 |
| *Assumptions for Estimates of Under- and Over-Treatment* | | | | |
| Share of Illness Episodes That are Malaria [a] | 0.386 | 0.386 | 0.386 | 0.386 |
| Targeting at Health Center (Medium) [b] | 0.75 | 0.75 | 0.75 | 0.75 |
| Targeting at Health Center (High) | 0.85 | 0.85 | 0.85 | 0.85 |
| Targeting at Health Center (Low) | 0.65 | 0.65 | 0.65 | 0.65 |
| *Under- and Over-Treatment: Preferred Estimates (assuming Medium Targeting at Health Center)* | | | | |
| Overall Targeting | 0.748 | 0.606 | 0.746 | 0.793 |
| Over Treatment | 0.078 | 0.266 | 0.152 | 0.127 |
| Under Treatment | 0.631 | 0.348 | 0.288 | 0.225 |
| *Under- and Over-Treatment: Alternative Estimates (assuming High Targeting at Health Center)* | | | | |
| Overall Targeting | 0.811 | 0.629 | 0.771 | 0.816 |
| Over Treatment | 0.059 | 0.251 | 0.137 | 0.113 |
| Under Treatment | 0.601 | 0.324 | 0.264 | 0.203 |
| *Under- and Over-Treatment: Alternative Estimates (assuming Low Targeting at Health Center)* | | | | |
| Overall Targeting | 0.686 | 0.583 | 0.722 | 0.770 |
| Over Treatment | 0.097 | 0.282 | 0.167 | 0.141 |
| Under Treatment | 0.662 | 0.373 | 0.312 | 0.247 |

Notes: Source: Authors' computations. Targeting (T) is the share of ACTs taken for illness episodes that are malaria. Overtreatment (OT) is the share of non-malaria episodes treated with an ACT. Undertreatment (UT) is the share of malaria episodes not treated with an ACT. See section 3 for the formulas relating T, OT and UT to the estimated parameters.

[a] The assumption on the share of illness episodes that are malaria ($\Pi$) is based on the rate observed in the symptoms database collected through unannounced household visits during which rapid diagnostic tests for malaria were administered. See section 4.3 for details.

[b] We consider three possible levels of targeting at health centers since there is no clear evidence from the literature on this parameter.

# Appendix: Predicting Malaria Positivity

We impute malaria probabilities to endline illness episodes based on the following probit model, fit to our symptoms database:

$$\Pr\left(pos_{eh} = 1 \mid x_{eh}, over14_{eh}\right) = \Phi\left(\beta_0 + x'_{eh}\delta + over14_{eh}\lambda + \left(x \times over14\right)'_{eh}\gamma\right)$$

where $pos_{eh}$ is a dummy variable equal to 1 if illness episode $e$ experienced by household $h$ in our symptoms database tested RDT positive for malaria, $x_{eh}$ is a vector of illness characteristics including patient age and age squared, as well as symptom dummies (cough, chills, headache, diarrhea, runny nose, vomiting, body pain, malaise/fatigue, and poor appetite), and $over14_{eh}$ is a dummy variable indicating that the patient is aged 14 or older (i.e. requires an "adult" dose; see Figure A1). We also interact all the symptom dummies with this indicator, to allow for a different relationship between malaria positivity and symptoms among younger and older patients.[23]

The results of this regression are presented in Appendix Table A3. Our estimates are consistent with clinical indicators of malaria (CDC 2011) – chills, headaches, and body pain are positively correlated with malaria positivity, while runny nose is negatively correlated with malaria positivity. Table A3 also reveals that age correlates very strongly with malaria positivity. Although the interaction terms make the trend somewhat difficult to infer, sick children (aged 13 and under) are substantially more likely to actually have malaria as compared to sick adults (the relevant fractions testing positive are 14 percent for adults and 58 percent for children). While striking, these results are not unexpected – young children are substantially more vulnerable to malaria, as they do not benefit from the acquired immunity that develops with repeated exposure to the parasite. Appendix Figure A2 presents the distribution of predicted positivity among first illness episodes in our endline survey.

[23]We do not include the most commonly cited symptom of malaria, fever, in order to avoid endline reporting bias. In Kiswahili (the interview language for our respondents), the word for "fever" – "*homa*" – is commonly used to refer to malaria. A concern is that if the subsidy regimes we study affected the likelihood that people get a formal diagnosis, this would make the reporting of *homa* (hence fever) endogenous. The pseudo $R^2$ on the probit declines from 0.2308 to 0.2216 when excluding fever and its interaction with the age variables. In practice, our results are very similar when including fever in predicting malaria positivity (though including fever does appear to introduce some reporting bias).

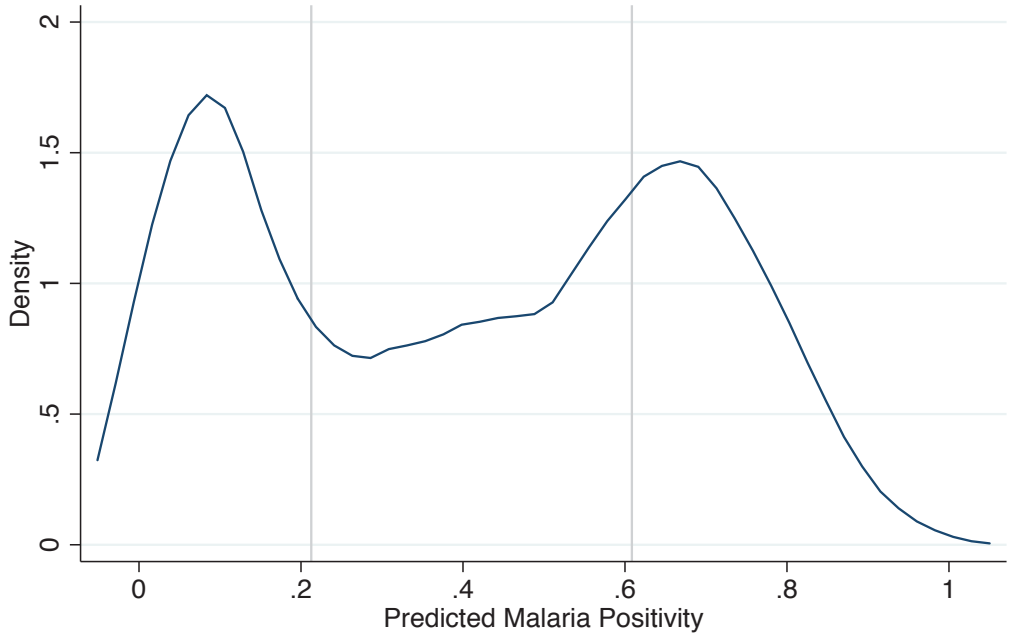
Appendix Figure A1. ACT Price and Dosing Guide

| | ***Recommended Dose and Corresponding Dose Cost for:*** | | | |
|---|---|---|---|---|
| | **Adult (14+)** | **Ages 9-13** | **Ages 4-8** | **Ages 3m-3y** |
| Dose / Price Per Pill | 4 pills, twice a day for three days | 3 pills, twice a day for three days | 2 pills, twice a day for three days | 1 pill, twice a day for three days |
| Ksh 20.83 (Control) | Ksh 500 | Ksh 375 | Ksh 250 | Ksh 125 |
| Ksh 4.16 (80% Subsidy) | Ksh 100 | Ksh 75 | Ksh 50 | Ksh 25 |
| Ksh 2.50 (88% Subsidy) | Ksh 60 | Ksh 45 | Ksh 30 | Ksh 15 |
| Ksh 1.66 (92% Subsidy) | Ksh 40 | Ksh 30 | Ksh 20 | Ksh 10 |

Notes: The exchange rate at the time of the study was around 78 Ksh to US$1. The tables reads as follows. Column 1: The unsubsidized ACT cost is Ksh 500 ($6.25) for an adult dose (age 14+). 80%, 88% and 92% subsidies correspond to 100 Ksh ($1.25), 60 Ksh ($0.75) and 40Ksh ($0.50) for an adult dose, respectively.

Ideal dosing is based on weight but manufacturers and the Kenyan Ministry of Health provide age guidelines as well, as it is not always feasible to weigh malaria patients. This study used the age guidelines from the Kenya Ministry of Health.

Appendix Figure A2. Distribution of Predicted Positivity Among Endline Illness Episodes




Notes: Data source: endline survey. Sample limited to first illness episodes. Graph shows kernel density estimate. Gray vertical lines demarcate tertiles of predicted positivity.

Appendix Table A1. Baseline Treatment Seeking Behavior

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | | By Patient's Age | | |
| | All | Patient 13 or Younger | Patient 14 or Older | P-value Under 14=14+ |
| *Household Level Malaria and Diagnostic Incidence* | | | | |
| Number of Presumed Malaria Episodes Last Month | 1.22 | 0.752 | 0.434 | -- |
| At Least One Presumed Malaria Episode Last Month | 0.685 | 0.490 | 0.332 | -- |
| HH Member Took RDT Test in Last Month (if Reported Malaria) | 0.040 | -- | -- | -- |
| HH Member Took Microscopy Test in Last Month (if Reported Malaria) | 0.251 | -- | -- | -- |
| *Treatment Seeking for All Presumed Malaria Episodes* | | | | |
| Did not Seek Care | 0.182 | 0.153 | 0.218 | 0.000*** |
| Went to Health Center | 0.413 | 0.445 | 0.374 | 0.000*** |
| Went to Drug Shop | 0.369 | 0.365 | 0.376 | 0.552 |
| *Medication for All Presumed Malaria Episodes* | | | | |
| No Antimalarial Taken | 0.221 | 0.194 | 0.255 | 0.000*** |
| Took ACT | 0.213 | 0.237 | 0.182 | 0.000*** |
| Took Sulfadoxine-Pyrimethamine (SP) | 0.100 | 0.079 | 0.140 | 0.000*** |
| Took Amodiaquine (AQ) | 0.181 | 0.209 | 0.141 | 0.000*** |
| Took Other Antimalarial | 0.072 | 0.086 | 0.052 | 0.000*** |
| Forgot Name of Antimalarial Taken | 0.217 | 0.198 | 0.233 | 0.027** |
| *Source of Antimalarials (Among Antimalarial Takers)* | | | | |
| Health Center | 0.444 | 0.459 | 0.428 | 0.149 |
| Drug Shop | 0.523 | 0.515 | 0.537 | 0.322 |
| Another Source | 0.033 | 0.026 | 0.035 | 0.158 |
| *Cost per Episode (Among Antimalarial Takers)* | | | | |
| Total Cost ($US) | 1.68 | 1.49 | 2.06 | 0.001*** |

Notes: Data source: baseline survey. Standard errors clustered at household level for episode-level statistics. ***, **, and * indicate significance at the 1, 5, and 10 percent levels respectively.

Appendix Table A2. Reporting Bias With Endline Illness Episodes

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Reported Any Illness Episode | Number Episodes Reported | Predicted Malaria Positivity - First Episode | Days Ago - First Episode | Patient Age - First Episode |
| ACT 92% | 0.015 | 0.024 | 0.013 | 1.73 | -1.71 |
| | (0.020) | (0.157) | (0.025) | (3.86) | (1.65) |
| ACT 88% | 0.002 | -0.063 | 0.028 | 4.72 | -2.92* |
| | (0.021) | (0.155) | (0.025) | (3.75) | (1.61) |
| ACT 80% | -0.020 | -0.168 | 0.010 | 3.19 | -1.69 |
| | (0.021) | (0.155) | (0.025) | (3.78) | (1.62) |
| RDT Subsidy | 0.006 | -0.025 | 0.004 | -1.27 | 0.906 |
| | (0.010) | (0.078) | (0.012) | (1.87) | (0.777) |
| Surprise RDT Test | 0.001 | 0.089 | -0.021* | 5.09*** | 0.988 |
| | (0.010) | (0.079) | (0.012) | (1.95) | (0.797) |
| P-value (92=88=80) | 0.005*** | 0.101 | 0.315 | 0.388 | 0.221 |
| DV Mean (No Subsidies) | 0.950 | 3.05 | 0.411 | 64.7 | 19.1 |
| N | 2621 | 2621 | 2473 | 2438 | 2473 |

Notes: Data source: endline survey. Table presents the results of regressing listed outcomes on listed treatment dummies. Robust standard errors (clustered at the household level when relevant) in parentheses. All regressions include full set of strata dummies and a control for household head age. ***, **, and * indicate significance at the 1, 5, and 10 percent levels respectively.

Appendix Table A3. Predicting Malaria Positivity - Probit Marginal Effects

| | (1) Coefficient | (2) Standard Error |
|---|---|---|
| Cough | -0.001 | (0.061) |
| Chills | 0.132 | (0.097) |
| Headache | 0.125* | (0.072) |
| Diarrhea | 0.247*** | (0.084) |
| Runny Nose | -0.119** | (0.060) |
| Vomiting | 0.063 | (0.072) |
| Body Pain | 0.197* | (0.111) |
| Malaise | -0.052 | (0.149) |
| Poor Appetite | 0.131 | (0.104) |
| Age 14 or Above | 0.398* | (0.239) |
| Age | 0.106*** | (0.032) |
| Age Squared | -0.008*** | (0.003) |
| (Age 14 or Above)×Cough | -0.096 | (0.126) |
| (Age 14 or Above)×Chills | -0.235** | (0.113) |
| (Age 14 or Above)×Headache | -0.070 | (0.126) |
| (Age 14 or Above)×Diarrhea | -0.221* | (0.131) |
| (Age 14 or Above)×Runny Nose | 0.222 | (0.147) |
| (Age 14 or Above)×Vomiting | 0.089 | (0.155) |
| (Age 14 or Above)×Body Pain | -0.106 | (0.133) |
| (Age 14 or Above)×Malaise | -0.075 | (0.171) |
| (Age 14 or Above)×Poor Appetite | 0.005 | (0.260) |
| (Age 14 or Above)×Age | -0.138*** | (0.034) |
| (Age 14 or Above)×Age Squared | 0.009*** | (0.003) |
| DV Mean / N | 0.428 | 533 |

Notes: Data source: Symptoms database (see text section 4.3 for details). Standard errors in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels respectively. We do not include the most commonly cited symptom of malaria, fever, in order to avoid endline reporting bias. In Kiswahili, the word for "fever" (homa) is commonly used to refer to "malaria". A concern is that if the subsidy regimes we study affected the likelihood that people get a formal diagnosis, this would make the reporting of homa endogenous. The pseudo R2 on the probit declines from 0.2191 to 0.2103 when excluding fever and its interaction with the age variables. In practice, our results are very similar when including fever in prediciting malaria positivity (though including fever does appear to introduce some reporting bias).

Appendix Table A4. Impact of ACT Subsidy on Treatment Seeking and ACT Access, by Age Group

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | Took ACT | Took ACT from Drug Shop | Took ACT from Health Center | Visited Drug Shop | Visited Health Center | Sought No Care | Took Malaria Test |
| *A. First Illness Episodes Where Patient is Under Age 14* | | | | | | | |
| ACT Subsidy = 92% | 0.185*** | 0.239*** | -0.054 | 0.107 | -0.080 | -0.026 | -0.006 |
| | (0.078) | (0.069) | (0.057) | (0.081) | (0.072) | (0.051) | (0.067) |
| ACT Subsidy = 88% | 0.207*** | 0.297*** | -0.091 | 0.075 | -0.056 | -0.022 | 0.013 |
| | (0.076) | (0.069) | (0.056) | (0.080) | (0.074) | (0.047) | (0.068) |
| ACT Subsidy = 80% | 0.184*** | 0.238*** | -0.067 | 0.132* | -0.175*** | 0.019 | 0.022 |
| | (0.077) | (0.068) | (0.057) | (0.076) | (0.066) | (0.055) | (0.066) |
| P-value: 92%=88%=80%=0 | 0.019** | 0.000*** | 0.444 | 0.353 | 0.050** | 0.800 | 0.974 |
| DV Mean (Control Group) | 0.280 | 0.110 | 0.171 | 0.573 | 0.317 | 0.110 | 0.195 |
| N | 331 | 331 | 331 | 331 | 331 | 331 | 331 |
| *B. First Illness Episodes Where Patient is Aged 14 or Older* | | | | | | | |
| ACT Subsidy = 92% | 0.301*** | 0.291*** | 0.010 | 0.219*** | -0.051 | -0.186*** | 0.024 |
| | (0.076) | (0.069) | (0.049) | (0.091) | (0.082) | (0.069) | (0.076) |
| ACT Subsidy = 88% | 0.087 | 0.115** | -0.028 | 0.290*** | -0.123* | -0.167*** | -0.055 |
| | (0.067) | (0.054) | (0.044) | (0.084) | (0.073) | (0.071) | (0.067) |
| ACT Subsidy = 80% | 0.208*** | 0.220*** | -0.011 | 0.161** | -0.062 | -0.112 | 0.078 |
| | (0.062) | (0.056) | (0.042) | (0.081) | (0.073) | (0.073) | (0.068) |
| P-value: 92%=88%=80%=0 | 0.000*** | 0.000*** | 0.893 | 0.005*** | 0.418 | 0.040** | 0.344 |
| DV Mean (Control Group) | 0.100 | 0.038 | 0.063 | 0.412 | 0.262 | 0.325 | 0.188 |
| N | 275 | 275 | 275 | 275 | 275 | 275 | 275 |

Notes: See Table 2 notes. Table does not include 25 observations with missing age information.

Appendix Table A5. Impact of ACT Subsidy on Treatment Channel and ACT Access by Predicted Malaria Positivity Tertile

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Took ACT | Took ACT from Drug Shop | Took ACT from Health Center | Visited Drug Shop | Visited Health Center | Sought No Care |
| 92% ACT Subsidy | | | | | | |
| × Tertile 1 | 0.306*** | 0.265*** | 0.041 | 0.147 | 0.127 | -0.274*** |
| | (0.103) | (0.088) | (0.065) | (0.111) | (0.103) | (0.084) |
| × Tertile 2 | 0.205* | 0.208* | -0.002 | 0.252* | -0.236** | -0.036 |
| | (0.118) | (0.108) | (0.074) | (0.130) | (0.114) | (0.093) |
| × Tertile 3 | 0.161 | 0.249*** | -0.089 | 0.119 | -0.116 | -0.002 |
| | (0.112) | (0.103) | (0.078) | (0.109) | (0.096) | (0.062) |
| 88% ACT Subsidy | | | | | | |
| × Tertile 1 | 0.103 | 0.081 | 0.022 | 0.180* | 0.002 | -0.180* |
| | (0.099) | (0.081) | (0.065) | (0.108) | (0.094) | (0.099) |
| × Tertile 2 | 0.156 | 0.198* | -0.041 | 0.188 | -0.166 | -0.021 |
| | (0.107) | (0.103) | (0.076) | (0.124) | (0.116) | (0.088) |
| × Tertile 3 | 0.179* | 0.316*** | -0.139* | 0.139 | -0.090 | -0.051 |
| | (0.102) | (0.095) | (0.073) | (0.101) | (0.098) | (0.063) |
| 80% ACT Subsidy | | | | | | |
| × Tertile 1 | 0.231*** | 0.195** | 0.034 | 0.115 | 0.041 | -0.174* |
| | (0.096) | (0.086) | (0.058) | (0.103) | (0.088) | (0.092) |
| × Tertile 2 | 0.156 | 0.189** | -0.031 | 0.209* | -0.255*** | 0.047 |
| | (0.110) | (0.096) | (0.071) | (0.117) | (0.103) | (0.095) |
| × Tertile 3 | 0.162 | 0.226** | -0.088 | 0.188* | -0.173* | -0.060 |
| | (0.112) | (0.100) | (0.085) | (0.105) | (0.092) | (0.065) |
| DV Mean (No Subsidy) | 0.206 | 0.063 | 0.131 | 0.510 | 0.278 | 0.210 |
| N | 606 | 606 | 606 | 606 | 606 | 606 |

Notes: Data source: endline survey. Sample excludes all households selected for a surprise or subsidized RDT. The unit of observation is the first illness episode that the household experienced following the baseline. A few households have multiple observations if multiple household members were ill simultaneously. Since the tertile dummies are generated regressors, we use bootstrapped standard errors (clustered at the household level) for these specifications. We bootstrap by generating 500 replicant datasets where households are sampled with replacement from the core sample. For each replicant sample, we recalculate predicted malaria positivity and positivity tertiles. All regressions control for household head age and a full set of strata dummies. Tertile cutoffs are illustrated in Figure 4. ***, **, and * indicate significance at the 1, 5, and 10 percent levels respectively.

Appendix Table A6. Impact of RDT Subsidy on Treatment Seeking and ACT Access by ACT Price

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | Sought Care at Drug Shop | Sought Care at Health Center | Sought No Care | Took Malaria Test | Took RDT Test | Took Microscopy Test | Took ACT |
| *A. First Illness Episodes Where Patient is Under Age 14* | | | | | | | |
| RDT Subsidy × 92% ACT Sub | 0.013 | -0.017 | 0.002 | 0.306*** | 0.303*** | 0.003 | 0.069 |
| | (0.063) | (0.056) | (0.042) | (0.058) | (0.047) | (0.042) | (0.069) |
| RDT Subsidy × 88% ACT Sub | 0.080 | -0.086 | -0.008 | 0.252*** | 0.274*** | -0.022 | 0.052 |
| | (0.062) | (0.056) | (0.034) | (0.056) | (0.044) | (0.045) | (0.061) |
| RDT Subsidy × 80% ACT Sub | 0.006 | 0.041 | -0.033 | 0.230*** | 0.236*** | -0.006 | 0.032 |
| | (0.058) | (0.047) | (0.041) | (0.055) | (0.044) | (0.039) | (0.061) |
| P-value: 92%=88%=80%=0 | 0.631 | 0.366 | 0.872 | 0.000*** | 0.000*** | 0.965 | 0.576 |
| DV Mean (ACT 92%, No RDT) | 0.692 | 0.218 | 0.090 | 0.179 | 0.077 | 0.103 | 0.449 |
| N | 1086 | 1086 | 1086 | 1086 | 1086 | 1086 | 1086 |
| *B. First Illness Episodes Where Patient is Aged 14 or Older* | | | | | | | |
| RDT Subsidy × 92% ACT Sub | -0.041 | -0.003 | 0.062 | 0.159** | 0.206*** | -0.047 | -0.122 |
| | (0.076) | (0.064) | (0.051) | (0.072) | (0.052) | (0.058) | (0.075) |
| RDT Subsidy × 88% ACT Sub | -0.075 | 0.016 | 0.051 | 0.206*** | 0.166*** | 0.040 | 0.025 |
| | (0.070) | (0.059) | (0.054) | (0.056) | (0.041) | (0.045) | (0.063) |
| RDT Subsidy × 80% ACT Sub | -0.009 | 0.024 | -0.014 | 0.040 | 0.076* | -0.036 | -0.010 |
| | (0.065) | (0.056) | (0.051) | (0.060) | (0.039) | (0.052) | (0.060) |
| P-value: 92%=88%=80%=0 | 0.690 | 0.968 | 0.502 | 0.000*** | 0.000*** | 0.592 | 0.424 |
| DV Mean (ACT 92%, No RDT) | 0.683 | 0.200 | 0.100 | 0.217 | 0.067 | 0.150 | 0.467 |
| N | 794 | 794 | 794 | 794 | 794 | 794 | 794 |

Notes: See Table 4 notes. Table does not include 103 observations with missing age information.

Appendix Table A7. RDT Take-Up by RDT Price

| | Took RDT | Sought Treatment at Drug Shop | Took RDT \| Sought Treatment |
|---|---|---|---|
| Free RDT | 0.354*** | 0.016 | 0.812*** |
| | (0.023) | (0.029) | (0.028) |
| RDT sold at Ksh 15, bundled refund[a] | 0.362*** | 0.055* | 0.767*** |
| | (0.023) | (0.029) | (0.030) |
| RDT sold at Ksh 15, no refund | 0.342*** | 0.020 | 0.780*** |
| | (0.019) | (0.025) | (0.025) |
| P-value (equality of RDT treatments) | {0.787} | {0.419} | {0.462} |
| Any RDT | 0.351*** | 0.029 | 0.784*** |
| | (0.013) | (0.021) | (0.018) |
| DV Mean (No RDT) | 0.005 | 0.415 | 0.012 |
| N | 2609 | 2609 | 1131 |

Notes: Data source: administrative drug shop data. Sample restricted to households selected for subsidized ACTs. Heteroskedasticity robust standard errors in parentheses, p-values in braces. All regressions include controls for ACT price treatment, surprise RDT selection, and a full set of strata dummies. ***, **, and * indicate significance at the 1, 5, and 10 percent levels respectively.

[a] Households in the "bundled refund" group received a refund for the RDT cost in the form of a Ksh 15 rebate on the ACT price if the RDT test was positive.

Appendix Table A8. External Validity Comparisons

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Central Uganda[a] | Eastern Uganda[b] | Western and Southeastern Tanzania[c] | Southern Malawi[d] |
| | November-December 2010 | May-June 2011 | March 2011 | January-March 2011 |
| *Malaria Burden (reported/perceived)* | | | | |
| HH Had at least one (Presumed) Malaria Episode (Past Month) | 0.590 | 0.354 | 0.273 | 0.410 |
| *Treatment Seeking for Malaria* | | | | |
| Public Sector | 0.250 | 0.333 | 0.417 | 0.760 |
| Private Sector* | 0.660 | 0.426 | 0.392 | 0.120 |
| No Treatment Sought | 0.090 | 0.221 | 0.187 | 0.120 |
| *Malaria Diagnosis (Any Blood Malaria Test)* | | | | |
| Last Month | 0.150 | 0.225 | | |
| Last Suspected Episode | | | 0.360 | |
| *Medication Taken* | | | | |
| Took ACT (Suspected Malaria) | 0.330 | 0.376 | 0.496 | |
| Antimalarial Cost | 1.690 | 1.355 | 1.366 | |

*Includes private clinics and retail sector

[a]Survey conducted in Luwero district. Malaria positivity figures are among purchasers of subsidized ACTs sold over-the-counter in local drug shops, with price ranging from $0.10 - $0.40 by age group/dosing level. Funding: Department for International Development, Clinton Health Access Initiative and Bill and Melinda Gates Foundation. Author: Jessica Cohen

[b]Survey conducted in Budaka, Bukedea, Kibuku, Kumi, Ngora and Pallisa districts. Malaria positivity figures are among household members from a random sample of the population. Funding: Clinton Health Access Initiative and Bill and Melinda Gates Foundation. Authors: Jessica Cohen, William Dickens, Gunther Fink

[c]Survey conducted in Mtwara and Rukwa regions. Funding: Clinton Health Access Initiative and Bill and Melinda Gates Foundation. Authors: Jean Arkedis, Jessica Cohen, Julius Massaga, Prashant Yadav

[d]Survey conducted in Machinga and Balaka districts. Funding: Bill and Melinda Gates Foundation. Authors: Pascaline Dupas, Dean Karlan, Jonathan Robinson