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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 those for whom the health returns are high and among those for whom the private health benefits are marginal (and the social returns possibly negative). The problem of how to set prices in the context of this type of moral hazard has been dubbed the "menu-setting problem" 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 setting is of particular importance because the benefits of ACTs to those suffering from a malaria infection are extremely high: malaria is a leading cause of death for children and the cause of numerous lost work hours for adults. Moreover, 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] In a context without accurate diagnosis, many patients may therefore choose presumptive treatment with ACTs, even when the risk of actually having malaria is low. At the same time, over-treatment of malaria negative patients 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 (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 to ensure access for appropriate users while limiting over-treatment 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

[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 (Terlouw et al. 2003).

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 percent of African children with malaria were treated with ACTs (World Health Organization 2009). Such low coverage rates 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.

Previous work has studied this access-targeting trade-off for preventative health products, such as bednets and water purification kits.[2] There is also a well-developed theoretical literature on how non-price mechanisms (e.g. ordeals) can be used to effectively target a range of subsidized goods and social programs to high-return beneficiaries, while limiting use by low-return parties (Akerlof 1978; Nichols and Zeckhauser 1982; Besley and Coate 1992; Alatas et al. 2013). The problem we are considering here is different from these two sets of previous studies, however, in that the incentives of beneficiaries and policy makers should in principle coincide (individuals who are 100 percent certain that they do not have malaria should not want an antimalarial). It is lack of information on the part of beneficiaries that creates a targeting problem. This is what motivates our focus on both pricing, which can impact targeting by leveraging pre-existing information available to beneficiaries, and information provision through over-the-counter rapid malaria testing, which should work to align the preferences of the beneficiary and the policy maker.

Specifically, we designed a field experiment to gauge the extent of the access-targeting tradeoff for ACT subsidies, and to test whether subsidizing malaria rapid diagnostic tests (RDTs) sold over-the-counter alongside ACTs can break this tradeoff. Our experiment, conducted with over 2,700 households in Western Kenya, introduced random variation in access to heavily subsidized ACTs and RDTs sold through local drug shops and monitored the impact on treatment seeking behavior and medication taking.

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 initially considered by various African countries, including Kenya) is highly effective at increasing access. When a 92 percent ACT subsidy is introduced in drug shops, the share of illnesses treated with an ACT more than doubles, from 19 to 41 percent.

[2]See Cohen and Dupas (2010), Dupas (2014), Hoffmann (2009), and Tarozzi et al. (2014) on bednets; Ashraf et al. (2010) and Kremer et al. (2011) on water purification; and Dupas (2011) for a review.

Second, this large increase in access is among both appropriate and inappropriate users. As a result, overall targeting is poor: only 56 percent of retail-sector subsidized ACTs go to malaria-positive individuals. Over-treatment is primarily a concern among teens and adults: only 21 percent of subsidized ACT takers aged 14 and older actually have malaria, while 79 percent of children taking subsidized ACTs actually have malaria. A key contributor to this result is acquired immunity to malaria: as people age, the likelihood that a given symptom (fever, aches, etc.) is caused by malaria decreases steeply.

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 greater risk if not promptly treated) remains unchanged. Yet targeting vastly improves, with the share of subsidized ACTs going to malaria positive patients rising from 56 to 75 percent. We find evidence of two distinct mechanisms behind this pattern. The first mechanism is largely mechanical: at the 92 percent subsidy level adult doses of ACTs are equal in cost to the cheapest antimalarials available in the retail sector. We observe a sharp drop in adult dose purchases when the subsidy level declines from 92 to 88 percent (which makes ACTs more expensive than the cheapest alternative antimalarials), with a more muted decline in demand for adult doses when the subsidy is lowered to 80 percent. We interpret these patterns to mean that adults, when uncertain about the true cause of their illness, tend to choose the lowest cost antimalarial first. In contrast, demand for ACT doses for children is essentially identical across the three subsidy levels that we study. The second (weaker) mechanism is a reallocation within a dose-price group: at lower subsidy levels ACT takers are somewhat more likely to truly have malaria. Again, this shift is driven by changes in the adult dose category. This 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.

Fourth, making RDTs available in the retail sector and subsidizing them heavily (85 percent or more) 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 elect to take a subsidized ACT anyway. We caveat, however, that our study can only speak to short-term effects of RDTs – 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.

These results, in addition to shedding light on how prices and information impact the crucial health care decisions of individuals in developing countries, are of direct relevance to the design of subsidies for malaria treatment and diagnosis. Such subsidies, with their potential to affect millions of households in rural Africa in both the short-run (affordability) and long-run (drug resistance), are at the center of an ongoing debate in the international community. Indeed, 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 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). Due in part to these criticisms, in late 2012 the AMFm board decided to roll the AMFm into the core Global Fund grant facility (this new policy will take effect in 2014). This means that while countries will continue to have the option to use malaria control resources to subsidize retail sector ACTs, these funds come at the opportunity cost of fewer resources for other malaria control initiatives such as insecticide treated bednets and indoor residual spraying.

Our results offer important insight for countries deciding how to allocate malaria control resources. Our results clearly show that an AMFm-type subsidy considerably expands access among the malaria-positive poor (thus potentially saving lives), and does so without meaningfully crowding out public sector care. While we find high levels of overtreatment at the original AMFm subsidy level, we also find evidence that modestly reducing the subsidy level can preserve the benefits of the AMFm while reducing overtreatment and the overall cost of the subsidy. Of course the "right" subsidy level 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.

Besides adding to the pricing and targeting literature cited above, our paper contributes to the economics literature in several 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 2014). 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.

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 in Sections 5 and 6 and discuss implications and external validity in Section 7.

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 2013). 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 are free if prescribed and available, fees are often charged for consultation and/or diagnosis

[3]The quality of microscopic testing varies greatly across lab technicians and with the quality of the equipment. Overall, 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.

(as is the case in our study area). What's more, distance, 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 two targeted subsidy levels (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 tradeoff between appropriate/inappropriate use inherent to retail-sector ACT subsidies. 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. We focus on these two outcomes because they have very clear implications for social welfare: all else equal decreases in under- and over-treatment are both associated with higher welfare.

Unfortunately in our empirical analysis, we cannot directly observe under-treatment and over-treatment rates. This is because it was not logistically feasible to collect real-time data on the universe of illness episodes and their true malaria-status. Instead we identify the impact of different subsidy policies on under- and over-treatment by focusing on two related outcomes that could be measured: access (the share of potential-malaria illness episodes, *whether truly malaria or not*, treated with ACTs) and targeting (the share of ACT-takers who are truly 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. Denote this share (the overall malaria prevalence) as Π, under-treatment as UT, over-treatment as OT, access as A and targeting as T. Then $UT = 1 - TA/\Pi$ and $OT = A\,(1-T)\,/\,(1-\Pi)$. 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 π 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.[5] The expected value of taking a particular action $a \in \{h, s, n\}$ depends on this probability, and is

[5]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.

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 and prices may be a function of the malaria probability π. For example, if the severity of symptoms is increasing as π 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 π increases. That is, we assume that $V^a(\pi) - V^n(\pi)$ increases with π 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 normalized the value functions so that $V^n(\pi) = 0$ for all π.[6] 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 π. 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

[6]The normalization we use is $norm[V^a(\pi)] = V^a(\pi) - V^n(\pi)$. Since individual choices depend on differences between the values of different options, this normalization does not affect any of our conclusions.

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 π.

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 afford 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 π 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

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

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 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.[8] Second, bundling 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

[8]Note that if people internalized the externality that overtreatment creates on drug effectiveness, this would both steepen and shift down the value function V^s. This would reduce the impact of a retail-sector subsidy on overtreatment but as long as households have imperfect information on their true malaria status, the potential trade-off between access and overtreatment would remain.

and October-November. Like much of sub-Saharan Africa, the region 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.[9] 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.[10] The vouchers stated the drug shop at which the 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). As expected given the sampling frame, 82 percent of the households interviewed at baseline reported that they had patronized our drug shop partner at least once in the past, and 72 percent reported that this was the drug shop that they usually used.

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.[11] 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.

[9]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. The fact that we excluded from the sample areas too close to health facilities means that our sample is farther away from health facilities than the average household in the area. The average distance of our study sample to health facilities appears very similar to rural areas in Kenya overall, however, possibly because our area of study has a denser network of health facilities than the rest of the country.

[10]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 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).

[11]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.

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.

Note that ACTs are priced by dose, with the appropriate dose determined by age. The ACT vouchers could be redeemed for a dose specific to the age of the patient, thus the total cost of the dose would be determined by not only the subsidy group but also the age-specific dose. 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. 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 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.[12]

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 percent 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. Appendix Table A4 demonstrates that the likelihood of redeeming an RDT voucher and the likelihood of visiting a study drug shop do not differ meaningfully across RDT treatment arms, so in the analysis that follows we pool them together into an "any RDT voucher" group for simplicity.[13]

[12]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).

[13]There are several possible reasons why we find no substantive differences across RDT treatment arms. First, we find limited price sensitivity for malaria treatment (ACTs) in the analysis below – and the range of prices for ACTs was higher than for RDTs – so a lack of price sensitivity for RDTs is perhaps not surprising. Second, there is an important value to the convenience of receiving a diagnosis at a local shop (relative to

In total, our experiment created a total of 11 treatment cells and one control cell – consequently, even if none of our treatments had a significant impact on outcomes, we would expect one cell to be statistically different from the control in any given regression specification. In order to reduce multiple-hypothesis testing concerns, we pool cells together in our analysis whenever empirically justified. As a robustness check, Appendix Table A7 aggregates all our main results that are statistically significant at the 10 percent level or better. We then present the original p-values as well as the analogous sharpened q-values (Benjamini et al. 2006), which control the false discovery rate (FDR). As described by Anderson (2008), the FDR is the expected proportion of rejections that are Type I errors. Thus, if one sets a q-value threshold of 0.05, in expectation 5 percent of all rejections at that level would be Type I errors. Our results are quite robust to this adjustment – all coefficients significant at the 5 percent level or better have q-values of 0.10 or less. We do note, however, that none of our marginally significant results have q-values below 0.10. Thus, we interpret marginally significant results with caution throughout the text.

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.[14]

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

traveling to the public health center, queuing and paying a fee for a diagnosis) that may overwhelm the (low) price of the RDT in our study.

[14] 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.

errors from these regressions. The sixth column presents F-statistics and p-values for a test of whether all the subsidy treatments are jointly equal to zero. There are no significant nor meaningful 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).[15]Throughout our analysis, we focus only on the first illness episode reported by each household, since we want to limit our attention 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. Appendix A gives additional detail on how we constructed predicted positivity for all illness episodes enumerated at endline.

4.4 Empirical Background: Age and Malaria Risk

An important empirical background fact 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 in the symptoms database. 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 the vertical gray line). This means that age is a very important (and easily observable) predictor of malaria status.

Importantly, the striking age gradient in malaria positivity is not specific to our study population. The strong relationship between age and malaria positivity is well known in

[15]There is evidence that the three ACT subsidy groups have differential reporting in terms of the incidence of illness (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 percent).

the malaria literature. Smith, Guerra, Snow, and Hay (2007) use nearly 150 studies to estimate an algorithm predicting parasite prevalence based on age and conclude that the relationship between prevalence and age is "predictable across the observed range of malaria endemicity".[16]

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 (conditional on patient age the difference between these two groups is highly statistically significant, with $p < 0.001$).

5 Results: Impacts of a Retail Sector ACT Subsidy

Our ultimate aim in this section is to study how retail-sector ACT subsidies impact both under-treatment (UT, the share of true malaria illnesses that are not treated with ACTs) and over-treatment (OT, the share of non-malaria illnesses that are treated with ACTs). Since collecting the data to directly measure these outcomes was not logistically feasible, we estimate impacts on UT and OT indirectly. To do so we focus on two related outcomes: access (A, the share of *all* illness episodes treated with an ACT) and targeting (T, the share of ACT takers who are actually malaria positive), which we estimate in subsections 5.1 and 5.2 respectively. Then in subsection 5.3 we discuss underlying mechanisms that could be driving our results. After discussing impacts of the RDT subsidy on A and T in Section 6, we plug our estimates into the formulas for UT and OT in Section 7, to assess the cost effectiveness of alternative subsidy regimes.

5.1 Overall Impacts on ACT Access

We study impacts on ACT access (as well as other measures of treatment seeking behavior) by presenting results from the following regression:

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

[16]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.

where y_{eh} is the outcome of interest for illness episode e in household h, $ACTsub_h$ is a vector of dummy variables for each of the ACT subsidy treatments, λ_{strata} are strata fixed effects, and x_h controls for age of the household head. Table 2, Panel A presents a specification where we pool all three ACT subsidies and compare outcomes to the control group, while Panel B presents a specification where we separately estimate the impact of the three different subsidy levels. In both cases, the omitted category is the "no ACT subsidy" (control) group. We limit our attention to first illness episodes experienced by households during the study period, as all households should have had access to the ACT vouchers at this time.[17] 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. (Appendix B summarizes which subsample is used for which analysis.)

Column 1 of Table 2 reports results on overall ACT access. The first thing to note is the low rate of ACT access in the control group: only 19 percent of illnesses in the control group were treated with ACTs (DV Mean, Column 1, Table 2), despite the fact that 39 percent of illness episodes in our symptoms database were malaria and ACTs are supposed to be freely available at health centers. Put another way, even if *all* ACTs taken by the control group went to malaria-positive individuals, over half of malaria episodes would not be treated with ACTs. The second thing to note is that all three subsidy levels lead to a large and significant increase in ACT access. 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).

Moreover, 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. Column 4 shows 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). This is driven by both crowd-out of care-seeking at the health center (a 7.9 percentage point reduction that is marginally significant in the pooled subsidy analysis) and a substantial increase in the likelihood of seeking any care at all: in the presence of ACT subsidies at drug shops, the fraction of households

[17] 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 very similar if we also include second illness episodes following the baseline survey (see supplementary web appendix M). Note that one disadvantage of limiting our attention to the first illness episode is that we under-weight households that have many illness episodes. If we weight our results by the total number of illness episodes experienced by a household, estimated impacts of the ACT subsidy on access increase. In this sense, our main results can be interpreted as a conservative lower bound.

not seeking any care decreases by 9-11 percentage points (around 42 percent, column 6). 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. We do, however, observe that the ACT subsidies substantially reduce the share of illness episodes treated with antibiotics (column 8). Unfortunately, our experimental protocol does not allow us to assess whether this change should be viewed positively (e.g. a reduction in overtreatment with antibiotics) or negatively (an increase in undertreatment). Finally, we also find some marginally significant evidence that the ACT subsidy crowded out other antimalarials and antipyretics (fever reducers) – what we call "sub-standard malaria treatment" in column 9.

While Table 2 makes it very clear that retail-sector subsidies substantially increase ACT access, we do not find very many differences *within* the subsidy levels. Indeed, we cannot reject that the three subsidy levels have equal impacts in any of our specifications in Panel B of Table 2. This can be seen visually in Panel A of Figure 4, which graphs the share of first illness episodes treated with any ACT, as well as the share of episodes treated with an ACT voucher across the subsidy levels.

One concern with the analysis so far is that it relies entirely on self-reports from the endline survey, during which we asked respondents to recall all their illness episodes in the previous four months. In order to cross-check the quality of households' endline reports, Panel B of Figure 4 compares endline household reports of voucher redemption to our administrative records of voucher redemption. The two data sources paint very similar pictures of voucher demand, which increases our confidence in the quality of our endline household data. Interestingly, the implied price elasticity of demand in the overall voucher redemption data is larger than the one among first illness episodes only (Panel A of Figure 4).

Overall, our results suggest that AMFM-type subsidies for ACTs substantially increase treatment with ACTs. To understand to what extent these changes in access should be viewed as helpful or harmful, we need to explore the malaria status of the ACT takers crowded in by lower prices. We address this in the next subsection by studying how the subsidy level changes targeting, the share of ACT-takers who are malaria positive.

5.2 Overall Impacts on ACT Targeting

We have two options for measuring targeting of ACTs. The first option is to use our drug shop data, where we can observe the *actual malaria status* of people who came to redeem vouchers in the "ACT Subsidy Only" group (see Figure 2) and who were surprise-tested with RDTs. This drug shop data should give a relatively complete picture of ACT targeting within

the 80-92 percent subsidy range since (as demonstrated in the previous section) most ACTs taken by households in the ACT subsidy arms were purchased with our vouchers. We can then take the drug shop data and combine it with assumptions about targeting outside the drug shop to arrive at overall targeting estimates across subsidy levels. Alternatively, we can use our endline data to study how the *predicted malaria positivity* of ACT takers (regardless of treatment channel) changes with the retail-sector ACT subsidy level. While this analysis has the advantage of including all illnesses treated with ACTs, it has an important drawback in that estimated impacts could be biased if people select into treatment seeking based on unobservable (to the econometrician) signals about their malaria status. Given this, we prioritize our administrative data results and present results using predicted positivity as a robustness check.

We begin with our administrative results. We limit our sample to the subset of "ACT Subsidy Only" households randomly selected for a surprise RDT test and run the following regression:[18]

$$pos_h = \beta_0 + \beta_1 ACT88_h + \beta_2 ACT80_h + \varepsilon_h \tag{3}$$

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. The omitted category in these regressions is the AMFm target subsidy level (92 percent). We limit our attention to first voucher redemptions because the free surprise RDT test could change households' subsequent treatment-seeking behavior. The downside of this approach is that we effectively underweight households who redeem multiple vouchers. Our results, however, are unchanged if we weight observations by the total number of ACT voucher redemptions in the household.

The results are presented in Table 3, column 1. Mistargeting is a large problem at the highest subsidy level – only 56 percent of patients taking ACTs obtained with a 92-percent subsidy voucher 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. Column 2 of Table 3 replicates the analysis in column 1, but uses predicted positivity as an outcome instead of actual positivity. The results are very similar, though the coefficients are smaller in magnitude, which is not surprising given

[18]Since almost no one purchases unsubsidized ACTs from drug shops, our administrative data does not provide us with an estimate of targeting in the retail sector under the "no subsidy" regime. To estimate UT and OT under that regime in section 7.1, we will conservatively assume that targeting of retail-sector ACTs in that regime is 100 percent.

that predicted positivity is an imperfect proxy for actual malaria status.

Column 3 uses our endline data to explore overall ACT targeting. Here we limit our sample to endline first illness episodes experienced by households who were *not* selected for a surprise RDT test (since the test result could influence the final treatment decision reported at endline). Consistent with the drug shop redemption data, these results indicate that higher prices increase positivity among ACT-takers overall, though estimates are not uniformly significantly different from zero, possibly due to the noisiness of our predicted positivity measure. We take the positive point estimates as corroborative evidence, and note that since 73-75 percent of all ACT-takers in the three subsidy groups report acquiring the ACTs with a study voucher (and 80 percent report acquiring ACTs from the retail sector), the (unbiased) targeting results using actual positivity at the drug shop shown in column 1 can reasonably be considered as indicative of impacts on overall targeting.

The magnitude of our drug shop targeting estimates are strikingly large given the relatively limited changes in demand we observe over the 80-92 percent subsidy range. The next subsection analyzes this apparent puzzle in greater detail.

5.3 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: the share of households using at least one ACT voucher remains virtually unchanged across the 80-92 percent subsidy range; in addition, 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 restrict 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 and most empirically relevant force is a reallocation from more expensive ACT doses (adult patients) to less expensive ACT doses (child patients). If ACT demand is more elastic at higher price points, this could happen mechanically, even if households are not willing to pay more to treat higher malaria-probability illnesses. Panel A of Table 4 presents evidence of this channel: here, we see that the ACT subsidy level has no impact on the likelihood that a household redeems its first voucher for a child under the age of 14 (column 1). In contrast, column 2 shows that lower subsidy levels substantially reduce the rate of redemptions for "adults" over the age of 14, who face the highest dose price and are also least likely to be malaria positive. These patterns substantially change the composition

of first voucher redemptions: at the 92 percent subsidy level, 39 percent of first voucher redemptions were for an adult aged 14 or older. Moving to the 80 percent subsidy level reduces this share by 16 percentage points, or 41 percent.[19]

The second force is a reallocation *within* age/dose category, to episodes most likely to be malaria – this force would only be present if households were willing to pay more to treat higher-probability malaria episodes. Panel B of Table 4 presents suggestive evidence that this may be happening, especially among adults: the results in column 2 suggest 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.

The targeting effect, like the access effect shown in Table 2 and Figure 4 Panel A, appears to be non-linear in price: there is a substantial change when the subsidy goes from 92 to 88 percent, with little-to-no change as the subsidy further declines to 80 percent. This non-linearity is likely explained by the fact that the cheapest alternative (non-ACT) antimalarial treatments cost around 35-50 KSh for an adult dose – which falls in-between the ACT prices under the 92 percent (40 KSh) and 88 percent (60 KSh) subsidy levels (as shown in Appendix Figure A3). 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 key features of households' information and treatment seeking behavior.

Overall, these results suggest that slightly higher ACT prices (compared to the original AMFm target price) do not significantly reduce access among those who need ACTs most (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.[20] Overtreatment remains an important issue, 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.

[19] Appendix Figure A4 replicates Figure 4 by age group, to graphically show the notably different patterns of demand for adults and children.

[20] One concern is that at higher prices, adults could 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.

6 Results: Impact of Adding an RDT Subsidy

6.1 Provider Choice and Diagnostic Testing

Table 5 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).[21] We estimate a pooled RDT treatment effect (Panel A), as well as the RDT treatment effects separately by ACT subsidy level (Panel B) with the following regression:

$$\begin{aligned} y_{eh} &= \beta_0 + \beta_1 RDT_h \times ACT92_h + \beta_2 RDT_h \times ACT88_h + \beta_3 RDT_h \times ACT80_h + \\ &\quad \beta_4 ACT88_h + \beta_5 ACT80_h + x_h'\gamma + \lambda_{strata} + \varepsilon_{eh} \end{aligned} \tag{4}$$

The first three columns of Table 5 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 26 percentage points (column 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 (approximately 80 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 A4). Columns 7-9 shows that despite this large increase in diagnostic testing, we observe no overall change in use of ACTs, other antimalarial treatments, or antibiotics. These results preview our finding that RDTs do not have meaningful impacts on ACT targeting – we discuss this (and potential mechanisms) in detail in the next two subsections.

[21] 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 on behavior for this group. Our results are largely unchanged, though less precisely estimated given the drop in sample size, when excluding these households.

6.2 RDTs and Targeting of Retail-Sector Subsidized ACTs

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 6 uses the administrative data to unpack selection into treatment seeking (columns 1 and 2) and ACT taking (column 3). For these specifications we limit our attention to surprise-tested households offered subsidized ACT vouchers. We then study how RDT provision impacts voucher use and malaria positivity conditional on the ACT subsidy level.

We show results pooling all ACT subsidy levels in Panel A, and separately by subsidy levels in Panel B. Focusing first on the pooled results, Panel A, column 1, shows that the RDT subsidy has no significant impact on the share of households redeeming at least one ACT or RDT voucher at the drug shop. This confirms our results from the endline data in Table 5. 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. The analysis by ACT subsidy level in Panel B suggests some positive selection into treatment-seeking under the highest subsidy (92 percent), however.

Turning to the intensive margin, column 3 estimates how malaria positivity among patients who ultimately elect to take the ACT varies with the RDT subsidy. In the pooled specification, we find that ACT takers are 8 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). Panel B of Table 6 shows that RDTs appear to have the largest targeting benefits when ACTs are subsidized the most (92 percent). However, this result is mostly driven by the surprising positive selection into the drug shop mentioned above. 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 6 as a likely upper bound.[22] The limited targeting impact of RDTs is confirmed by an

[22]One 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. A more troubling possibility would be if the 92-percent-ACT-subsidy-only group were unusually malaria negative, simply due to chance. This latter possibility would lead us to overestimate the targeting impact of RDTs at the 92 percent ACT subsidy level and lead us to overestimate the targeting impact of higher ACT prices discussed earlier. For example, assume that the 12.7 percentage point increase in positivity among treatment seekers associated with the RDT treatment at the highest subsidy level is illusory and that the

analysis based on predicted malaria positivity, presented in Appendix Table A5.

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, over half 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 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 in the field (see footnote 3). While RDTs have a much lower rate of false negatives than microscopy (5 percent versus 31 percent), 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 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).

estimate is entirely due to the 92-percent-ACT-subsidy-only group testing "too negative". Then this would imply that the lower ACT subsidy level actually increased positivity by 5.5 percentage points, rather than 18.2 percentage points estimated in Table 3, column 1.

7 Discussion: Mapping to Under- and Over-Treatment

7.1 Cost Effectiveness

So far our discussion of the results has focused on access and targeting, but as detailed in Section 3, the key policy outcomes of interest are under-treatment and over-treatment. Table 7 uses the evidence above, combined with some needed assumptions, to estimate the extent of under- and over-treatment under five regimes of interest: the no-subsidy regime, the AMFm "status quo" (a 92 percent ACT subsidy with no RDT), an 88 and 80 percent ACT subsidy with no RDT, and an 80 percent ACT subsidy with an RDT subsidy. Over-treatment decreases 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.[23]

In our context it is quite clear that the 80 percent subsidy is more cost effective than the AMFm status quo: 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

[23]We interpret this result with caution, however: as Figure 4 illustrates, our access estimates based on first illness episodes (Panel A) are not very precisely estimated, and the results using administrative voucher redemptions (Panel B), suggest that demand may not be quite as flat as the first-illness episode point estimates imply.

doesn't get better after taking an ACT, the household might not use this as a signal that ACTs are ineffective if the RDT was negative. This effect could be very important. Adhvaryu (2014) 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 a signal about actual malaria status. 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 test result suggests that households do see some of these important benefits to testing.[24] 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 Welfare

Drawing conclusions about welfare requires information on three inputs:

(1) How many malaria-positive people are treated with ACTs? Any reasonable social welfare function should be strictly increasing in this quantity. This is equal to $(1-UT)\times\Pi$, where Π is the malaria prevalence among all illness episodes.

(2) How many malaria-negative people are taking ACTs? This is equal to $OT\times(1-\Pi)$. Any reasonable social welfare function should be weakly decreasing in this quantity. Whether the function is strictly decreasing would depend on assumptions about: a) disease resistance and b) alternative causes of illness and what other medications patients are taking (e.g. whether ACT subsidies exacerbate undertreatment with antibiotics for pneumonia).

(3) How much does the subsidy cost? Any reasonable social welfare function should be strictly decreasing in this quantity.

As shown in Table 7, we find that the 80 percent subsidy strictly dominates the 92 percent subsidy in terms of (1), (2), and (3). In terms of comparing the "no subsidy" to the 80 percent subsidy: we find that at the 80 percent subsidy (1) is higher, which increases welfare, but (2) and (3) are also higher, and both of these decrease welfare. In comparing these two regimes, we thus need to take a stand on the relative welfare gains and losses from changes in (1) compared to (2) and (3).

Calculating a specific numerical assessment of the welfare difference would require strong

[24]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).

assumptions as well as data that is not available (e.g. the likelihood of resistance emerging or the share of ACT takers that have bacterial pneumonia). However, under most reasonable sets of assumptions, the welfare gains of increasing (1) would outweigh the welfare costs of increasing (2) and (3). There are two main factors behind this reasoning. First, the welfare effect of (2) (overtreatment) is likely second order compared to the effect of (1) (under-treatment) in terms of malaria mortality risk. Second, case-management with ACTs is recognized by the WHO as the most cost-effective intervention (in terms of $ per DALY averted) among malaria control interventions, and is among the most cost-effective of all interventions available to improve health in sub-Saharan Africa (World Bank, 2006). Thus, we do not expect the cost of capital to be so high that the increase in (3) (the subsidy cost) outweighs the mortality benefits of reducing under-treatment.

That said, if overtreatment with antimalarials delays appropriate treatment for other high-risk illnesses (e.g. antibiotics for pneumonia), the effect of overtreatment could approach first-order. Unfortunately, the existing literature does not provide much guidance on how serious these non-malarial illnesses might be. To our knowledge the only recent study of the etiology of non-malarial illness in sub-Sahara African children is D'Acremont et al. (2014). This study finds that the majority (70.5 percent) of febrile illnesses are viral in nature, while 22 percent are bacterial. Only a minority of the bacterial infections were caused by pneumonia. Another study from 2002 in Indonesia finds a similar rate of bacterial infection (Punjabi et al. 2012). Overall, most non-malarial illnesses are thought to be viral and self-limiting with a substantial minority that are bacterial and need prompt treatment. Of course, the causes of non-malarial illnesses will vary over time, geography and age. In our experimental data we find some evidence that ACT subsidies reduce use of antibiotics, but our protocol does not allow us to assess whether this change should be viewed positively (e.g. a reduction in overtreatment with antibiotics) or negatively (an increase in undertreatment with antibiotics).

7.3 Heterogeneity

As discussed in the theoretical section, the distributional impact of the subsidies is particularly important. In supplementary web appendix H we re-run the main analysis allowing for heterogeneity in effects by (1) SES (proxied by literacy status of the interviewed head of household), and (2) baseline malaria knowledge (whether the head knows that mosquitoes, and no other factor, transmit malaria). In general, we do not have enough power to detect differences between these groups. However, we do find insignificant, but suggestive evidence that ACT subsidies disproportionately benefit illiterate-headed households, who in

the absence of any subsidy have very low rates of ACT coverage despite the free distribution at health centers. We also find suggestive evidence that an AMFm-type ACT subsidy increases overtreatment more among literate-headed households, and those households have lower rates of compliance with RDTs.

7.4 External Validity

While our experiment 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 A6 presents basic statistics from household surveys recently conducted in two regions of Uganda, two regions of Tanzania and the Southern region of Malawi.[25] 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 episodes. All surveys also reveal limited rates of blood test diagnosis for such episodes.

Another important question is whether the subsidy regimes we created through our experimental voucher system adequately simulated what would happen if the subsidy were implemented at scale. 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 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.

[25]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.

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 criticized from the start 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 (even after households have been informed about the superiority of ACTs, as in our study), but substantial and relatively 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 rural, poor populations suffering from malaria, but these subsidies may not need to be as large as initially 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, price is a useful tool for selection: somewhat higher ACT prices reduce ACT taking among adults, who are much 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 enhance 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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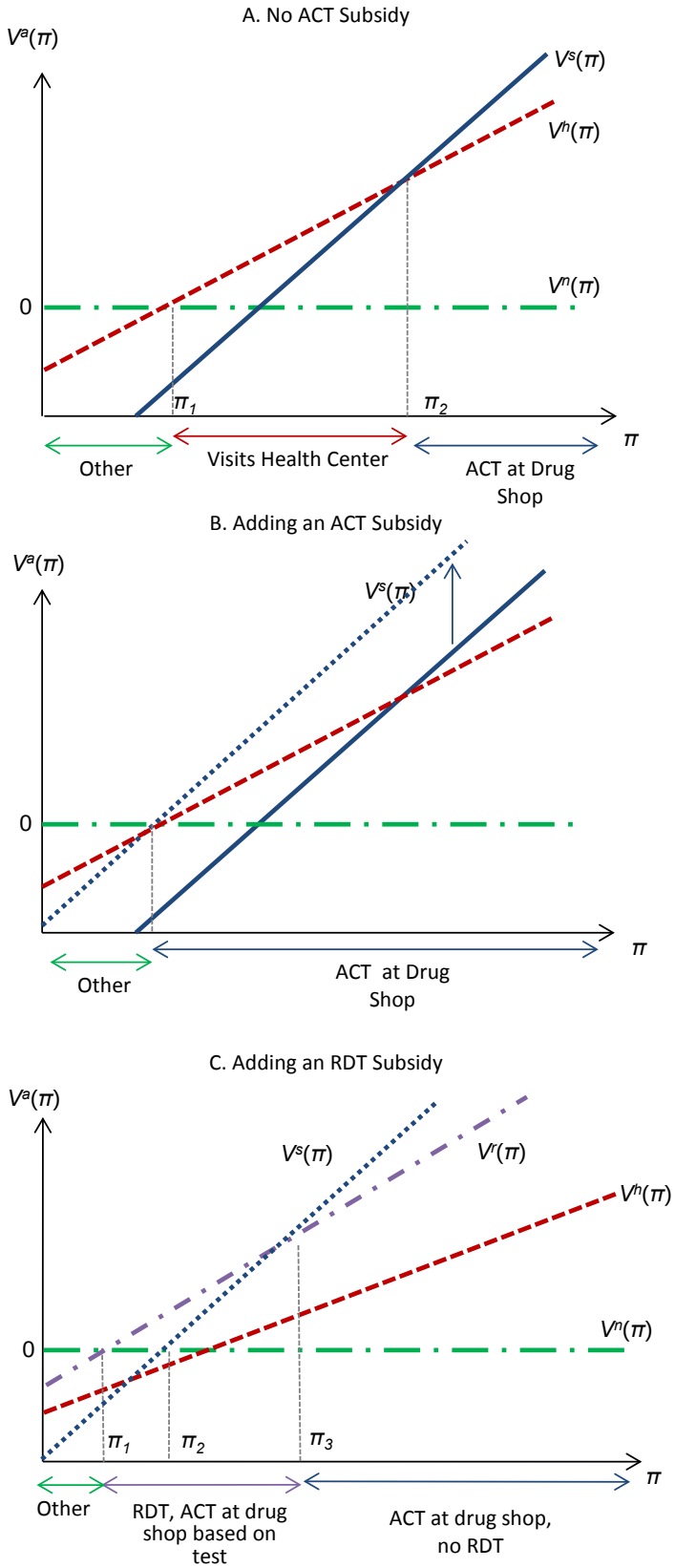
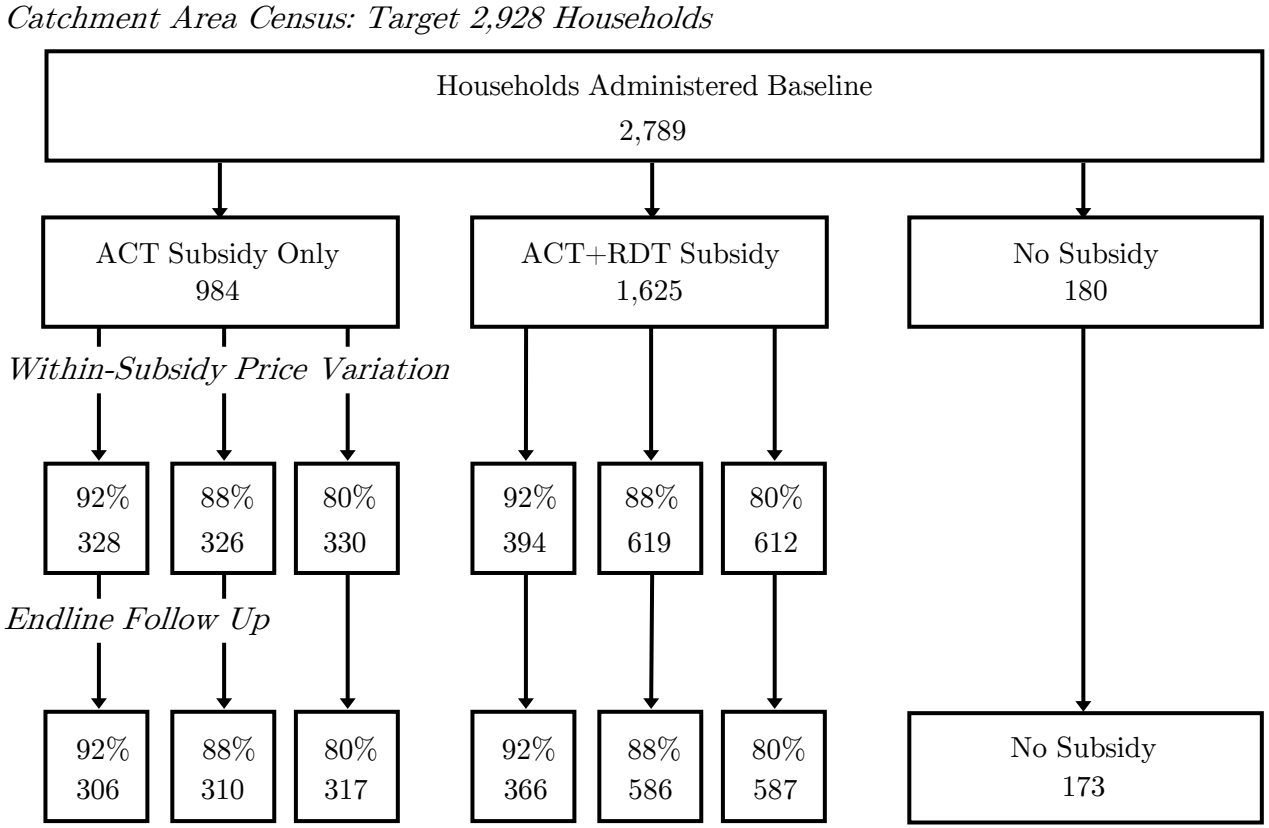
Figure 1. Theoretical Impact of ACT and RDT Subsidies



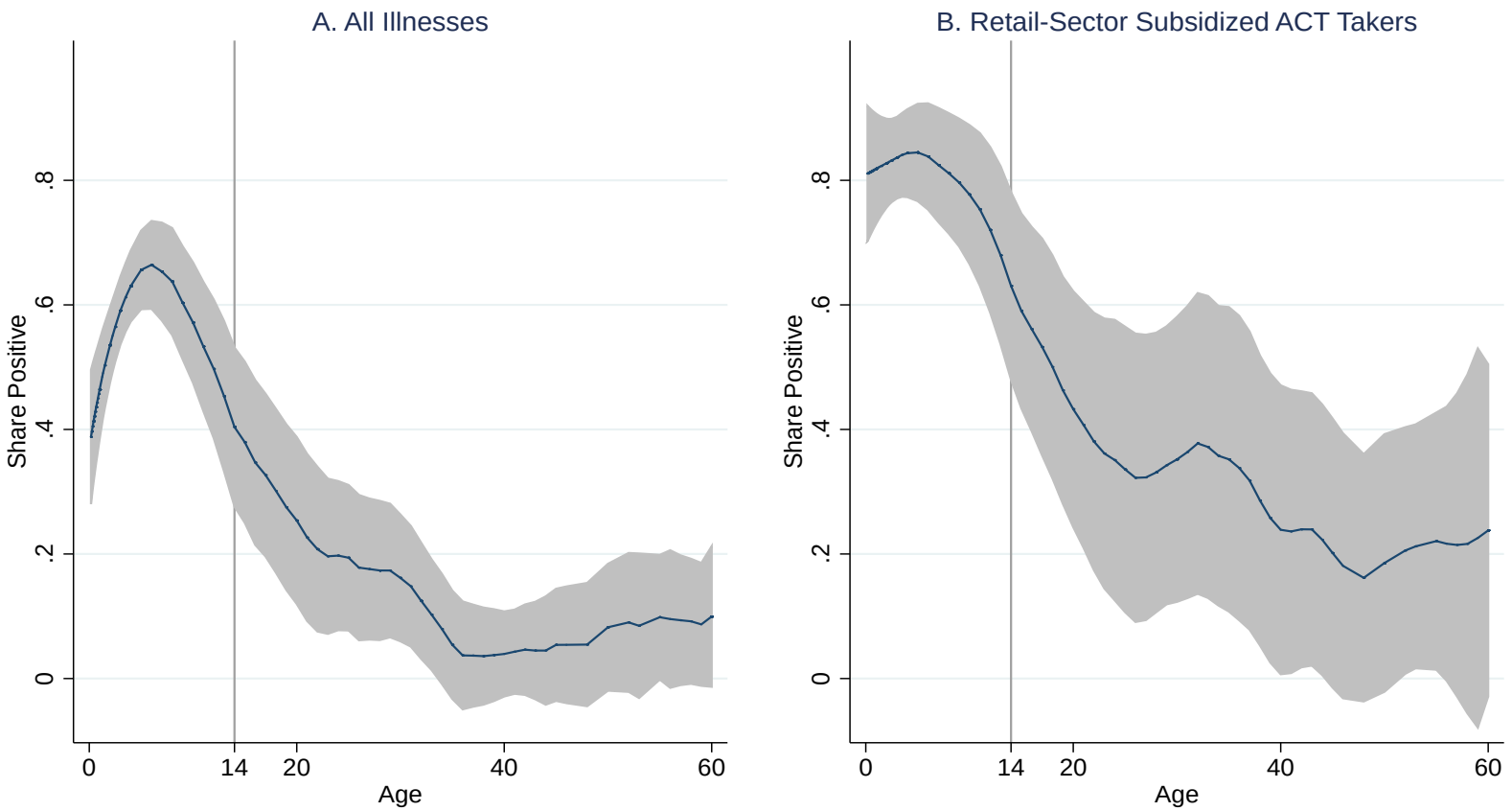
Notes: π is the (perceived and actual) probability that the illness episode is malaria. V^s is the value of purchasing an ACT at the drug shop; V^h is the value of visiting a health center and receiving free ACT if positive; V^n is the value of doing neither of the two options above. The value functions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



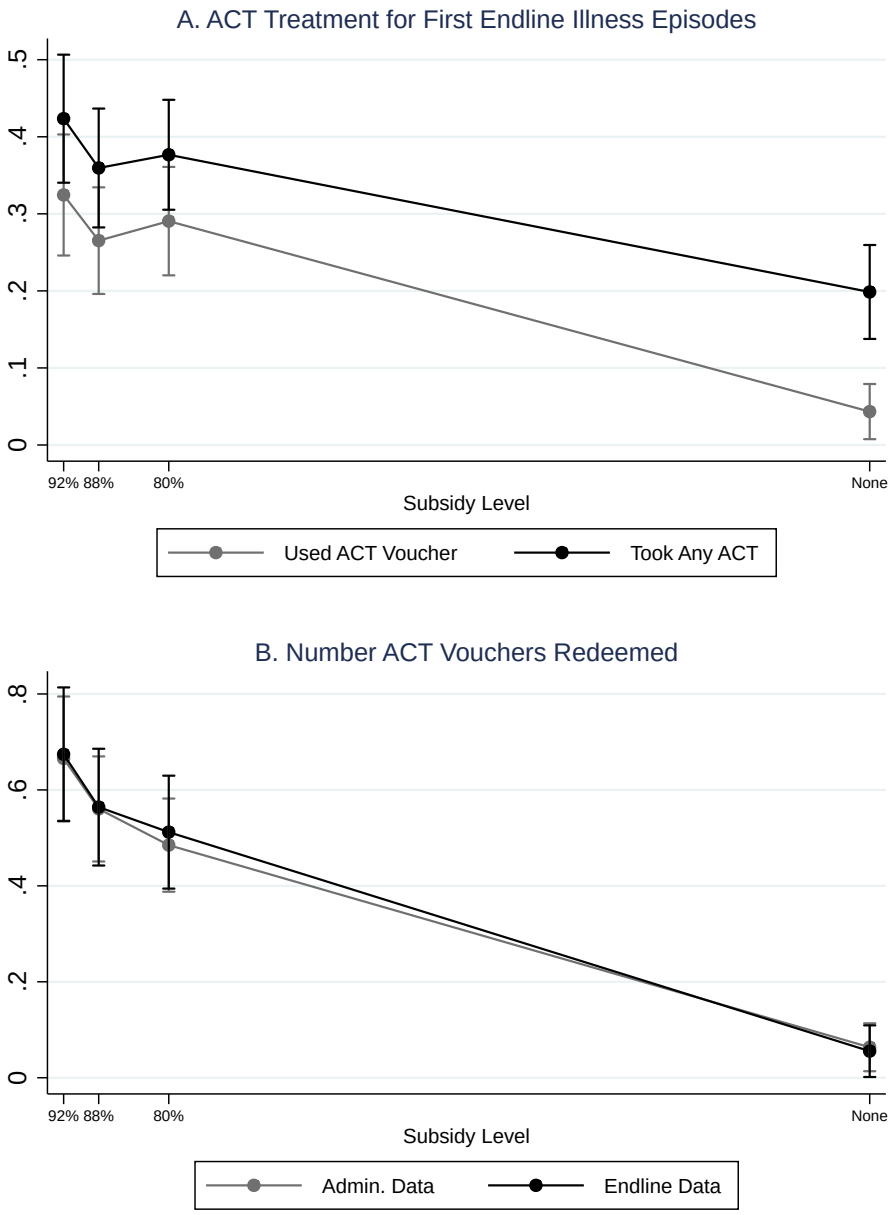
Notes: At the end of the baseline survey each household received two ACT vouchers and, if sampled for the RDT subsidy, two RDT vouchers. 49 percent of ACT subsidy only households and 80 percent of ACT+RDT subsidy households were selected for surprise RDT testing at the drug shop. Within each ACT subsidy 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 group them together for simplicity. Details for the impact of the different RDT subsidies are provided in Appendix Table A6.

Figure 3. Malaria Positivity Among All Illnesses and Subsidized ACT Takers



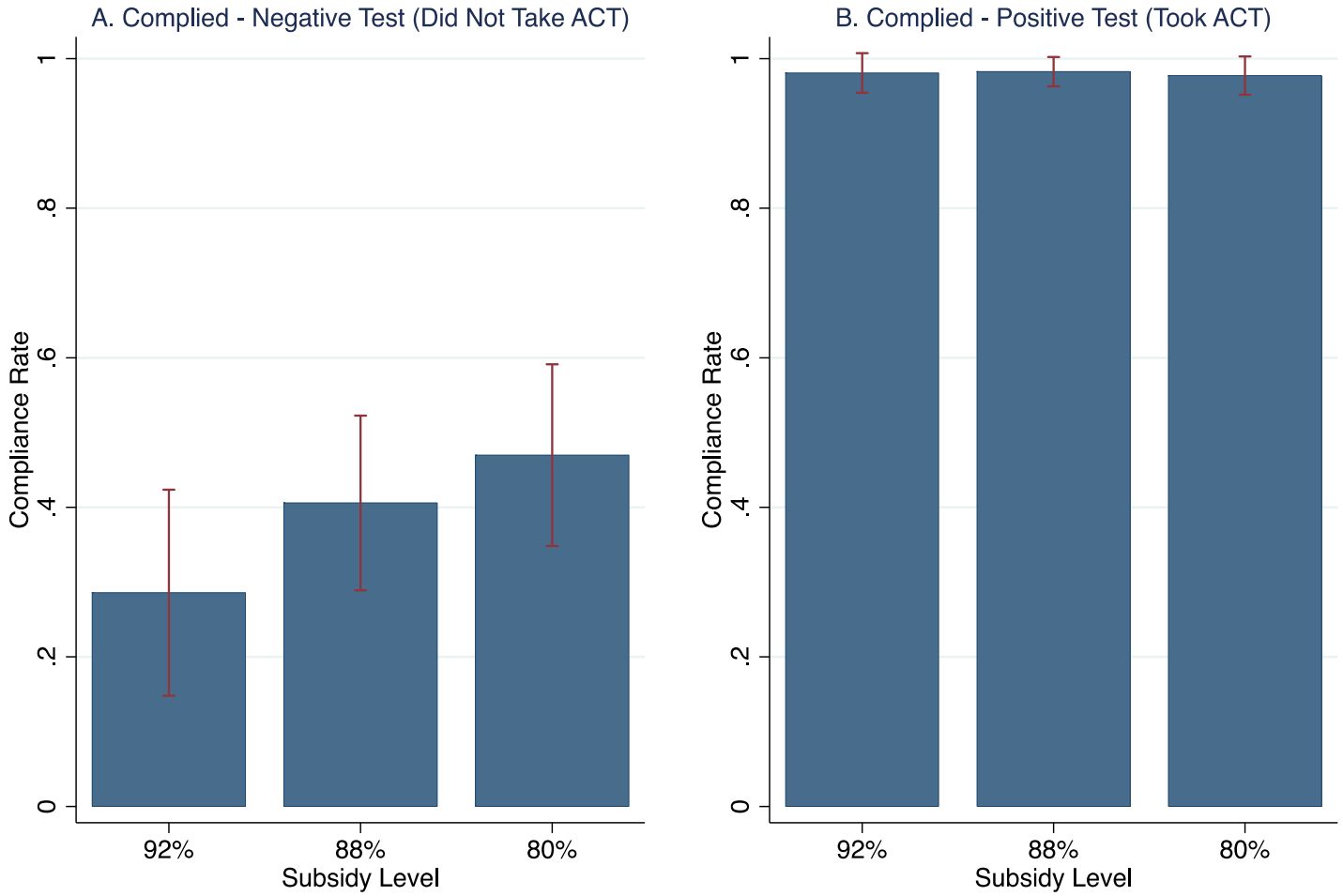
Notes: Local linear regression results. Shaded gray area gives 95 percent confidence intervals. Age is topcoded at 60 for legibility. Panel A data is from the symptoms database. Positivity data is obtained from RDT tests administered to currently sick individuals who fell sick within three days of visit. Panel B data is from administrative drug shop data, all ACT subsidy levels, no RDT subsidy. Positivity data is from surprise RDT tests of ACT takers.

Figure 4. ACT Demand by Subsidy Level



Notes: Both Panels exclude households randomly selected to receive a surprise RDT test and households randomly selected to receive RDT vouchers. There is a total sample of N=631 in Panel A and N=677 (administrative data) and N=609 (endline data) in Panel B. All regressions include strata fixed effects as well as controls for the age of the household head. Whiskers give 95 percent confidence intervals based on robust standard errors (clustered at the household level in endline data).

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. The sample sizes are N= 40, 69, and 66 for the 92, 88 and 80 percent subsidy levels in Panel A. In Panel B the relevant sample sizes are N= 104, 171, and 131.

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)	All Subsidies = 0	N
Characteristics of Interviewed Household Head							
Female	0.867	0.017	0.029	0.040	0.010	1.25	2789
	[0.341]	(0.029)	(0.028)	(0.028)	(0.012)	{0.287}	
Age (years)	41.7	-1.98	-3.22**	-2.44*	0.185	1.61	2646
	[17.3]	(1.46)	(1.44)	(1.45)	(0.626)	{0.170}	
Education (years)	5.10	0.141	0.381	0.151	0.169	1.17	2774
	[4.00]	(0.343)	(0.341)	(0.342)	(0.161)	{0.323}	
Literate	0.575	0.047	0.050	0.027	0.000	0.621	2782
	[0.496]	(0.042)	(0.042)	(0.042)	(0.020)	{0.647}	
Married	0.783	-0.015	0.004	0.006	-0.015	0.514	2784
	[0.413]	(0.035)	(0.035)	(0.034)	(0.016)	{0.725}	
Subsistence Farmer	0.589	0.052	0.039	0.059	-0.005	0.612	2787
	[0.493]	(0.042)	(0.042)	(0.042)	(0.019)	{0.654}	
Number Dependents	4.12	-0.263	-0.096	-0.077	0.021	0.809	2663
	[2.78]	(0.223)	(0.221)	(0.222)	(0.098)	{0.519}	
Household Characteristics							
Number members	5.48	-0.354	-0.233	-0.197	0.024	0.885	2789
	[2.77]	(0.217)	(0.214)	(0.215)	(0.092)	{0.472}	
Fraction Adults (Ages 14+)	0.623	-0.035*	-0.048***	-0.024	0.002	2.23*	2337
	[0.235]	(0.020)	(0.019)	(0.020)	(0.009)	{0.063}	
Acres Land	2.72	-0.660**	-0.601*	-0.571*	0.197*	1.63	2250
	[3.69]	(0.330)	(0.327)	(0.324)	(0.117)	{0.164}	
Distance from drug shop (km)	1.68	0.012	0.012	0.002	0.010	0.523	2788
	[0.917]	(0.023)	(0.022)	(0.022)	(0.011)	{0.719}	
Distance from closest clinic (km)	6.57	-0.018	-0.036	-0.043	0.044*	0.796	2785
	[2.47]	(0.060)	(0.059)	(0.059)	(0.027)	{0.528}	
Baseline Malaria Knowledge and Health Practices							
Number bednets	1.77	-0.031	-0.060	0.028	0.005	0.476	2784
	[1.43]	(0.120)	(0.121)	(0.120)	(0.057)	{0.753}	
Share HH members slept under ne	0.561	0.023	0.006	0.030	-0.012	0.612	2661
	[0.397]	(0.034)	(0.034)	(0.034)	(0.017)	{0.654}	
Only Mosquitoes Transmit Malari	0.517	0.045	0.011	0.024	-0.020	0.842	2789
	[0.501]	(0.042)	(0.042)	(0.042)	(0.020)	{0.499}	
Heard of ACTs	0.399	0.016	0.017	0.030	0.001	0.197	2771
	[0.491]	(0.042)	(0.041)	(0.042)	(0.020)	{0.940}	
ACT is Preferred Antimalarial	0.207	-0.023	-0.029	-0.049	-0.002	0.978	2771
	[0.406]	(0.034)	(0.034)	(0.033)	(0.015)	{0.418}	
Heard of RDTs	0.128	0.039	0.020	0.021	-0.011	0.682	2786
	[0.335]	(0.030)	(0.029)	(0.029)	(0.014)	{0.604}	
Treats water regularly	0.408	-0.036	-0.018	0.004	0.023	1.13	2779
	[0.493]	(0.041)	(0.041)	(0.041)	(0.019)	{0.339}	
Number of presumed malaria	1.20	0.015	-0.008	-0.029	0.033	0.200	2789
episode last month	[1.22]	(0.102)	(0.103)	(0.103)	(0.050)	{0.939}	
Cost per Episode (Among Those Seeking Care)							
Total Cost (US $)	1.63	0.140	-0.040	-0.217	0.131	0.725	1319
	[1.86]	(0.293)	(0.250)	(0.238)	(0.174)	{0.575}	
Sample Size in Treatment	180	328	326	330	1625		

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)	(8)	(9)
	Took ACT	ACT from Drug Shop	ACT from Health Center	Visited Drug Shop	Visited Health Center	Sought No Care	Took Malaria Test	Took Antibiotic	Sub-Standard Malaria Treatment
Panel A. Pooled Impact									
Any ACT Subsidy	0.187***	0.222***	-0.038	0.167***	-0.079*	-0.096***	-0.014	-0.072**	-0.077*
	(0.038)	(0.031)	(0.030)	(0.046)	(0.042)	(0.036)	(0.038)	(0.034)	(0.045)
Panel B. Impact by Subsidy Level									
ACT Subsidy = 92%	0.225***	0.249***	-0.024	0.159***	-0.055	-0.110***	-0.031	-0.046	-0.084
	(0.053)	(0.046)	(0.037)	(0.058)	(0.053)	(0.042)	(0.048)	(0.043)	(0.058)
ACT Subsidy = 88%	0.161***	0.217***	-0.056	0.167***	-0.070	-0.097**	-0.042	-0.062	-0.109*
	(0.050)	(0.043)	(0.037)	(0.058)	(0.052)	(0.042)	(0.047)	(0.040)	(0.057)
ACT Subsidy = 80%	0.178***	0.206***	-0.035	0.173***	-0.106**	-0.085*	0.023	-0.100***	-0.045
	(0.048)	(0.042)	(0.035)	(0.054)	(0.047)	(0.045)	(0.046)	(0.038)	(0.055)
P-value: 92%=88%=80%=0	0.000***	0.000***	0.498	0.004***	0.164	0.048**	0.533	0.066	0.238
P-value: 92%=88%=80%	0.531	0.723	0.660	0.968	0.535	0.846	0.362	0.304	0.539
DV Mean (Control Group)	0.190	0.071	0.119	0.488	0.286	0.226	0.214	0.185	0.536
N	631	631	631	631	631	631	631	631	631

Notes: Data source: endline survey. 'Substandard' malaria treatment includes non-ACT antimalarials and antipyretics. 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 ACT Targeting

	Dependent Variable:		
	Actual Malaria Status	Predicted Positivity	Predicted Positivity
	(1)	(2)	(3)
ACT Subsidy = 88%	0.187**	0.112***	0.111**
	(0.081)	(0.042)	(0.053)
ACT Subsidy = 80%	0.182**	0.107**	0.040
	(0.084)	(0.043)	(0.052)
P-value: 88%= 80% = 0	0.038**	0.012**	0.104
P-value: 88%= 80%	0.955	0.906	0.179
DV Mean (ACT 92%, no RDT)	0.563	0.424	0.422
N	190	189	178
Data Source	Admin.	Admin.	Endline

Notes: The omitted category is the 92% ACT subsidy group. Sample in columns 1 and 2 include all first ACT voucher redemption among households selected for a surprise RDT and no RDT voucher (in column 2, 1 observation has a missing value for predicted malaria positivity). Sample in column 3 includes all endline first illness episodes treated with ACTs among households not selected for a surprise RDT and not selected for an RDT voucher. Robust standard errors (clustered at the household level in the endline data) are in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels respectively.

Table 4. Mechanisms Behind ACT Targeting Effects

Panel A. Does the ACT Subsidy Level Reallocate ACTs Across Dosage Groups?

	(1)	(2)
	Used First Voucher for Patient Under 14	Used First Voucher for Patient 14 or Older
ACT Subsidy = 88%	0.035	-0.057**
	(0.035)	(0.027)
ACT Subsidy = 80%	0.031	-0.080***
	(0.034)	(0.026)
P-value: 88%= 80% = 0	0.540	0.007***
DV Mean (ACT 92%, no RDT)	0.268	0.171
N	984	984
Subsample	All HH	All HH

Panel B. Does the ACT Subsidy Level Reallocate ACTs Within Dosage Groups?

	Surprise RDT Result: Patient Under 14	Surprise RDT Result: Patient 14+
ACT Subsidy = 88%	0.060	0.256*
	(0.082)	(0.148)
ACT Subsidy = 80%	0.066	0.170
	(0.083)	(0.160)
P-value: 88%= 80% = 0	0.687	0.192
DV Mean (ACT 92%, no RDT)	0.791	0.214
N	132	58
Additional Controls	None	None

Notes: The omitted category is the 92% subsidy group. Panel A includes all households not sampled for an RDT, regardless of surprise RDT status. Panel B limits sample to households who were selected for a surprise RDT test and redeemed at least one ACT voucher. Dose group controls include dummy variables for three of the 4 ACT dose groups (based on patient age). Heteroskedasticity robust standard errors in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels respectively.

Table 5. 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	(8) Took Antibiotic	(9) Substandard Malaria Treatment
Panel A. Across all ACT Subsidy Levels									
RDT Subsidy	0.004	-0.013	0.010	0.216***	0.215***	-0.014	0.018	0.020	-0.012
	(0.026)	(0.022)	(0.018)	(0.023)	(0.017)	(0.018)	(0.026)	(0.017)	(0.027)
DV Mean (No RDT)	0.657	0.212	0.123	0.207	0.076	0.125	0.389	0.110	0.456
Panel B. By ACT Subsidy Level									
RDT Subsidy × 92% ACT Sub	-0.005	-0.018	0.029	0.258***	0.263***	-0.019	0.002	0.004	-0.040
	(0.048)	(0.042)	(0.032)	(0.044)	(0.034)	(0.034)	(0.050)	(0.033)	(0.050)
RDT Subsidy × 88% ACT Sub	0.026	-0.045	0.007	0.252***	0.229***	0.000	0.042	-0.016	0.013
	(0.046)	(0.041)	(0.030)	(0.039)	(0.030)	(0.032)	(0.044)	(0.030)	(0.046)
RDT Subsidy × 80% ACT Sub	-0.012	0.023	-0.003	0.152***	0.166***	-0.021	0.016	0.070**	-0.012
	(0.043)	(0.035)	(0.033)	(0.040)	(0.029)	(0.030)	(0.041)	(0.028)	(0.044)
88% ACT Subsidy	-0.006	-0.002	0.014	-0.013	0.004	-0.016	-0.067	-0.011	-0.032
	(0.058)	(0.052)	(0.038)	(0.048)	(0.032)	(0.041)	(0.058)	(0.038)	(0.059)
80% ACT Subsidy	0.009	-0.041	0.020	0.050	0.028	0.007	-0.058	-0.047	0.030
	(0.055)	(0.047)	(0.040)	(0.049)	(0.032)	(0.040)	(0.056)	(0.035)	(0.057)
P-value: RDT Terms Jointly = 0	0.938	0.612	0.832	0.000***	0.000***	0.851	0.787	0.079*	0.850
DV Mean (ACT 92%, No RDT)	0.667	0.222	0.104	0.194	0.069	0.125	0.444	0.125	0.444
N	1993	1993	1993	1993	1993	1993	1993	1993	1993

Notes: Data source: endline survey. 'Substandard' malaria treatment includes non-ACT antimalarials and antipyretics. 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 6. Impact of RDT Subsidy on ACT Targeting

	(1)	(2)	(3)	(4)
	Dependent Variable: Household Sought Treatment at drug shop	Dependent Variable: Surprise RDT reveals that patient is malaria-positive		*Proportion that redeemed RDT voucher, conditional on seeking treatment at drug shop*
		Sample: patients who visited drug shop	Sample: patients who bought subsidized ACT at drug shop	
Panel A. Across all ACT Subsidy Levels				
RDT Subsidy	0.025	0.009	0.081**	*0.818*
	(0.026)	(0.039)	(0.039)	
Panel B. By ACT Subsidy Level				
RDT Subsidy × 92% ACT Sub	0.028	0.127*	0.163**	*0.792*
	(0.045)	(0.070)	(0.070)	
RDT Subsidy × 88% ACT Sub	0.052	-0.058	0.018	*0.837*
	(0.044)	(0.063)	(0.062)	
RDT Subsidy × 80% ACT Sub	-0.010	-0.047	0.061	*0.818*
	(0.047)	(0.068)	(0.067)	
DV Mean (ACT 92%, No RDT)	0.429	0.556	0.563	-
N	1776	755	687	*573*

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

	(1) No Subsidy	(2) ACT 92% Subsidy	(3) ACT 88% Subsidy	(4) ACT 80% Subsidy	(5) ACT 80% + RDT Subsidy
Experimental Estimates of Access and Drug Shop Targeting					
Total Share Taking ACT	0.190	0.415	0.351	0.369	0.385
Share Taking ACT at Drug Shop	0.071	0.320	0.288	0.278	0.303
Share Taking ACT at Health Center	0.119	0.095	0.063	0.084	0.078
Targeting at Drug Shop	1.000	0.563	0.750	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	0.386
Targeting at Health Center (Medium)[b]	0.75	0.75	0.75	0.75	0.75
Targeting at Health Center (High)	1.000	1.000	1.000	1.000	1.000
Targeting at Health Center (Low)	0.65	0.65	0.65	0.65	0.65
Under- and Over-Treatment: Preferred Estimates (assuming Medium Targeting at Health Center)					
Overall Targeting	0.844	0.606	0.750	0.747	0.795
Over Treatment	0.048	0.266	0.143	0.152	0.129
Under Treatment	0.583	0.347	0.317	0.287	0.207
Under- and Over-Treatment: Alternative Estimates (assuming High Targeting at Health Center)					
Overall Targeting	1.000	0.664	0.795	0.805	0.846
Over Treatment	0.000	0.227	0.117	0.117	0.096
Under Treatment	0.506	0.285	0.276	0.231	0.155
Under- and Over-Treatment: Alternative Estimates (assuming Low Targeting at Health Center)					
Overall Targeting	0.781	0.583	0.732	0.723	0.774
Over Treatment	0.068	0.282	0.153	0.166	0.142
Under Treatment	0.614	0.372	0.333	0.309	0.227

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 (Π) 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 A: 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.[26]

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.

Appendix B: Analysis Samples

This appendix describes which sample is used in each table. Indeed given our experimental design we have to omit certain subgroups from certain analyses. For example, when analyzing

[26] 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).

the impact of subsidies on medication choices we ignore those sampled for surprise RDT tests since obviously getting tested has the potential to change medication decisions. All of our analyses similarly require selecting out a portion of the sample but which portion differs across tables. The table below indicates which subgroup is present in which analysis.

Appendix Table B1. Analysis Samples

Treatment Group	Surprise RDT Test?	Visited Drug Shop?	Took ACT?	Sample Size: Admin. Data	Sample Size: Endline Illnesses	Table 1	Table 2	Table 3	Table 4: PA	Table 4: PB	Table 5	Table 6: Col 1	Table 6: Col 2	Table 6: Col 3	Table 6: Col 4	Table A2*, Col 3-5	Table A4: Col 1-2	Table A4: Col 3	Table A5**: Col 1	Table A5**: Col 2
Control	No	No	No	8	136	A	E									E				
		Yes	No	0		A	E									E				
		Yes	Yes	172	32	A	E									E				
ACT Subsidy	No	No	No	296	283	A	E		A		E					E	A			
		Yes	No	0		A	E		A		E					E	A	A	A	
		Yes	Yes	201	180	A	E	E	A		E					E	A	A	A	A
	Yes	No	No	280	277	A			A			A				E	A			
		Yes	No	1		A			A			A	A			E	A	A	A	
		Yes	Yes	206	176	A		A	A	A		A	A	A		E	A	A	A	A
ACT + RDT Subisdy	No	No	No	186	173	A					E					E	A			
		Yes	No	12		A					E					E	A	A	A	
		Yes	Yes	138	130	A					E					E	A	A	A	A
	Yes	No	No	716	733	A					E	A				E	A			
		Yes	No	67		A					E	A	A		A	E	A	A	A	
		Yes	Yes	506	494	A					E	A	A	A	A	E	A	A	A	A
			Totals	2789	2614	2789	631	206/180	984	206	1993	1776	780	712	573	2614	2609	1131	1131	1051

Notes: Tables marked with asterisks include samples that are not easily described by the guide. We provide additional detail on those samples below. We do not break down the endline data by whether the patient sought care at the drug shop since this is not relevant for any of the tables. The letter "A" indicates that the sample is drawn from the administrativce data based, while the letter "E" indicates a sample drawn from the endline database. This table gives the total eligible sample for a given table. In some cases (e.g. Table 3) the actual sample is somewhat lower due to missing data for the outcome variable. The total eligible sample in Panel B of Table 4 is split between columns 1 and 2.

* The eligible sample in columns 1 and 2 of Table A2 is all households (regardless of subsidy treatment or surprise RDT status) interviewed at endline.

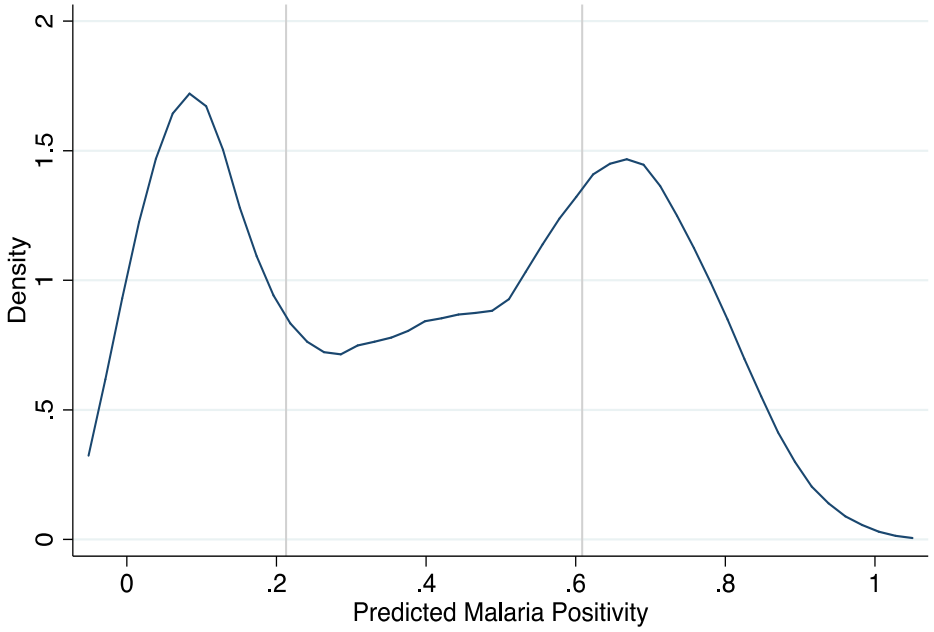
** The eligible sample in column 3 of Table A5 includes all first illness episodes treated with either a malaria test or an antimalarial in households not selected for a surprise RDT. The sample in column 4 is then limited to those illness episodes treated with an antimalarial.

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

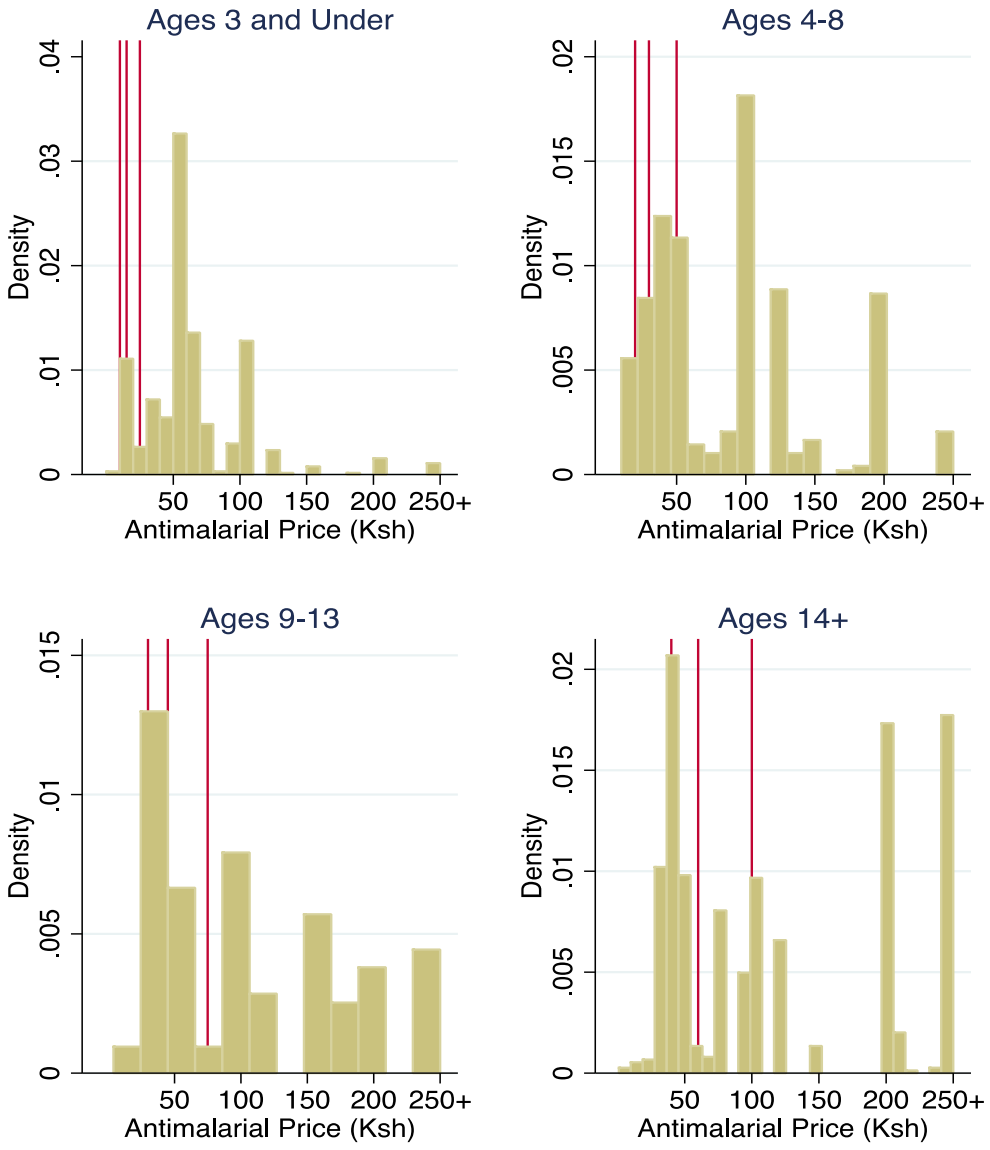
Notes: The exchange rate at the time of the study was around 78 Ksh to US$1. The tables reads as follows. Column 1: The unsubsidized ACT cost is Ksh 500 ($6.25) for an adult dose (age 14+). 80%, 88% and 92% subsidies correspond to 100 Ksh ($1.25), 60 Ksh ($0.75) and 40Ksh ($0.50) for an adult dose, respectively.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



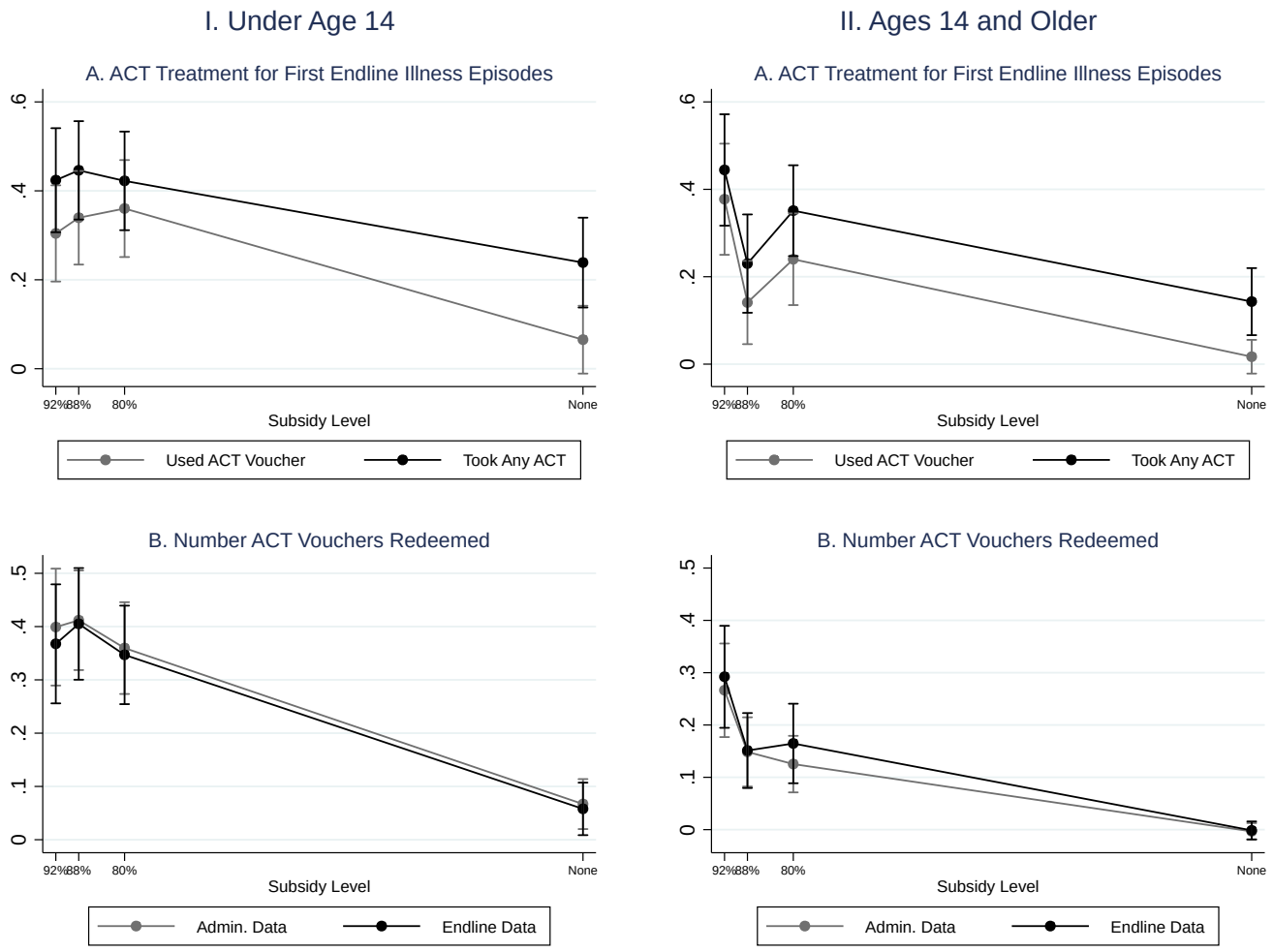
Notes: Data source: endline survey. Sample limited to first illness episodes. Graph shows kernel density estimate. Gray vertical lines demarcate tertiles of predicted

Figure A3. Distribution of Unsubsidized Antimalarial Prices by Patient Age



Notes: Data are from observed purchases of unsubsidized antimalarials at the four study drug shops. Red vertical lines indicate the three subsidized ACT prices for each age group.

Figure A4. ACT Demand by Subsidy Level and Age



Notes: All panels exclude households randomly selected to receive a surprise RDT test and households randomly selected to receive RDT vouchers. All regressions include strata fixed effects as well as controls for the age of the household head. Whiskers give 95 percent confidence intervals based on robust standard errors (clustered at the household level in endline data).

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 Mal	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 Subsidie	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. 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 voucher)	0.005	0.415	0.012
N	2609	2609	1131

Notes: Data source: administrative drug shop data. Sample includes all 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 A5. The Impact of RDT Subsidies on Predicted Positivity of Malaria Treatment Seekers and ACT Takers

	(1)	(2)	(3)	(4)
	First Redemptions at Drug Shop (Admin. Data)		First Illness Episodes at Endline	
Sample limited to those who:	Visited Drug Shop	Took ACT at Drug Shop	Took Antimalarial or Malaria Test	Took Antimalarial
A. Across all ACT Subsidy Levels				
RDT Subsidy	-0.001	0.031*	0.000	0.006
	(0.016)	(0.016)	(0.020)	(0.021)
B. By ACT Subsidy Level				
RDT Subsidy × 92% ACT Sub	0.028	0.049	0.020	0.028
	(0.030)	(0.030)	(0.036)	(0.037)
RDT Subsidy × 88% ACT Sub	-0.006	0.029	-0.021	0.000
	(0.027)	(0.027)	(0.032)	(0.035)
RDT Subsidy × 80% ACT Sub	-0.024	0.014	0.004	-0.006
	(0.027)	(0.027)	(0.033)	(0.035)
DV Mean (ACT 92%, No RDT)	0.435	0.437	0.433	0.444
N	1127	1048	1125	1014

Notes: The dependent variable is predicted malaria positivity. Robust standard errors, clustered at the household level, when relevant. Columns 3 and 4 exclude households who did not receive vouchers for subsidized RDTs but were selected for a surprise RDT test at the drug shop. ***, **, and * indicate significance at the 1, 5, and 10 percent levels respectively.

Appendix Table A6. 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.

Appendix Table A7. Significant Results - Original P-Values and FDR-Adjusted Q-Values

Table Number					
2 (Cols 1-4)	2 (Cols 5-9)	3	4	5	6
0.187***	-0.079*	0.187**	-0.057**	0.216***	0.127*
0.022	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*
0.118	*0.001*	*0.000*	*0.000*	*0.000*	*0.000*
0.225***	-0.106**	0.182**	-0.080***	0.258***	0.081**
0.031	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*
0.154	*0.001*	*0.000*	*0.000*	*0.000*	*0.000*
0.161***	-0.096***	0.112***	0.256*	0.252***	0.163**
0.009	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*
0.066	*0.001*	*0.000*	*0.000*	*0.000*	*0.000*
0.178***	-0.110***	0.107**		0.152***	
0.013	*0.000*	*0.000*		*0.000*	
0.084	*0.001*	*0.000*		*0.000*	
0.222***	-0.097**	0.111**		0.215***	
0.036	*0.011*	*0.000*		*0.000*	
0.154	*0.074*	*0.000*		*0.000*	
0.249***	-0.085*			0.263***	
0.033	*0.071*			*0.000*	
0.154	*0.303*			*0.000*	
0.217***	-0.072**			0.229***	
0.002	*0.036*			*0.000*	
0.015	*0.154*			*0.000*	
0.206***	-0.100***			0.166***	
0.088	*0.020*			*0.000*	
0.377	*0.118*			*0.000*	
0.167***	-0.077*			0.070**	
0.000	*0.000*			*0.000*	
0.001	*0.000*			*0.000*	
0.159***	-0.109*				
0.000	*0.000*				
0.001	*0.000*				
0.167***					
0.000					
0.001					
0.173***					
0.000					
0.002					

Notes: Regression coefficients are in plain text, unadjusted p-values are in italics, and sharpened q-values that control the false discovery rate are in bold italics. The sharpened q-values are calculated using all p-values from Tables 2-6 using the methodology of Benjamini et al. (2006).