

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

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Both under- and over-treatment of communicable diseases are public goods. 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 trade-off 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 one-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. (JEL D12, D82, I12, O12, O15)

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 trade-off 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

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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 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 World Health Organization's (WHO) essential drugs list, fewer than 15 percent of African children with malaria were treated with ACTs (WHO 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 policymakers 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 preexisting information available to beneficiaries,

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. 2000). Subsequent innovations in antimalarial medicines have been successively less able to withstand parasite resistance (Terlouw et al. 2003).

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

and information provision through over-the-counter rapid malaria testing, which should work to align the preferences of the beneficiary and the policymaker.

Specifically, we designed a field experiment to gauge the extent of the access-targeting trade-off for ACT subsidies, and to test whether subsidizing malaria rapid diagnostic tests (RDTs) sold over-the-counter alongside ACTs can break this trade-off. 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 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.

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 (i) households have at least some private information about the probability that an illness episode is actually malaria; and (ii) 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 one-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, Panosian, and Gelband 2004). The AMFm program was controversial, and the initiative proved to be quite costly—only two years of drug copayments for the seven countries in the AMFm pilot cost over \$450 million.³ 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, p. 1). Due in part to these criticisms, in late 2012 the AMFm board decided to roll the AMFm into the core Global Fund grant facility. Under this new policy, which took effect in 2014, countries continue to have the option to use malaria control resources to subsidize retail sector ACTs. However, 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 over-treatment 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 over-treatment 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 trade-off 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, Hammer, and Leonard 2008; Welch, Schwartz, and Woloshin 2011; Adhvaryu 2014). Second, we contribute to the literature on treatment-seeking behavior in resource-constrained environments,

³“The Global Fund: Heal Thyself.” *The Economist*, November 24, 2012.

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, Mliga, and Mariam (2002) in Tanzania; Banerjee, Deaton, and Duflo (2004) in Rajasthan (India); and Leonard (2007, 2009) in Tanzania and Cameroon, respectively.

The remainder of the paper proceeds as follows. Section I provides some background facts on the malaria burden and treatment options in rural Africa, as well as the AMFm subsidy. Section II develops a theoretical framework for studying treatment-seeking behavior in this environment, and identifies the key trade-offs inherent to heavily subsidizing ACTs. Section III describes our experimental design and data. We present results in Sections IV and V and discuss implications and external validity in Section VI.

I. 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 five years old (WHO 2013). Children less than five years old 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 five years old 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.⁴ 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 (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.

⁴The 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 (WHO 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 2000s and their use is not yet widespread at public health facilities, especially in rural areas.

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 (online 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, Hanson, and Goodman 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, Svensson, and Yanagizawa-Drott 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 copayment (a “global subsidy”), the AMFm aimed 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 was not formally restricted, but the aim was 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 \$0.50.⁵ 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).

II. 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 trade-off between appropriate/inappropriate use inherent to retail sector ACT subsidies. The trade-off is embedded in the following two policy outcomes: (i) under-treatment: the share of true malaria episodes that do not get treated with ACTs; and (ii) 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.

⁵Retail 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).

Unfortunately in our empirical analysis, we cannot directly observe under-treatment and over-treatment rates. This is because it was not logically 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.

A. Household Decision Making

We consider an environment where, when faced with an illness shock, the household has three possible actions, $a \in \{h, s, n\}$: (i) seek diagnosis at a formal health facility (receiving ACTs if positive): $a = h$; (ii) bypass the public health sector and buy ACTs at the drug shop: $a = s$; (iii) 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.⁶ The expected value of taking a particular action $a \in \{h, s, n\}$ depends on this probability, and is denoted by $V^a(\pi)$. It can be decomposed as follows:

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

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

$$(1) \quad V^s(\pi) \geq \max\{V^h(\pi), V^n(\pi)\}.$$

⁶It 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.

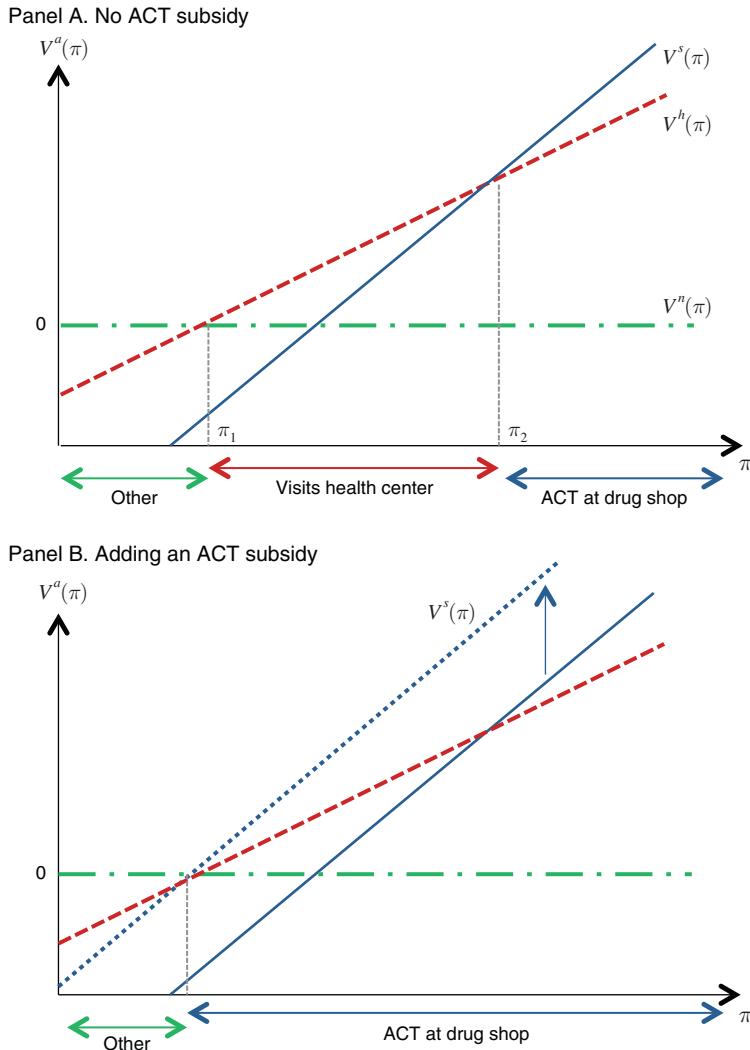


FIGURE 1. THEORETICAL IMPACT OF ACT AND RDT SUBSIDIES

(Continued)

Panel A of Figure 1 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 .⁷ 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).

⁷ The normalization we use is $\text{norm}[V^a(\pi)] = V^a(\pi) - V^n(\pi)$. Since individual choices depend on differences between the values of different options, this normalization does not affect any of our conclusions.

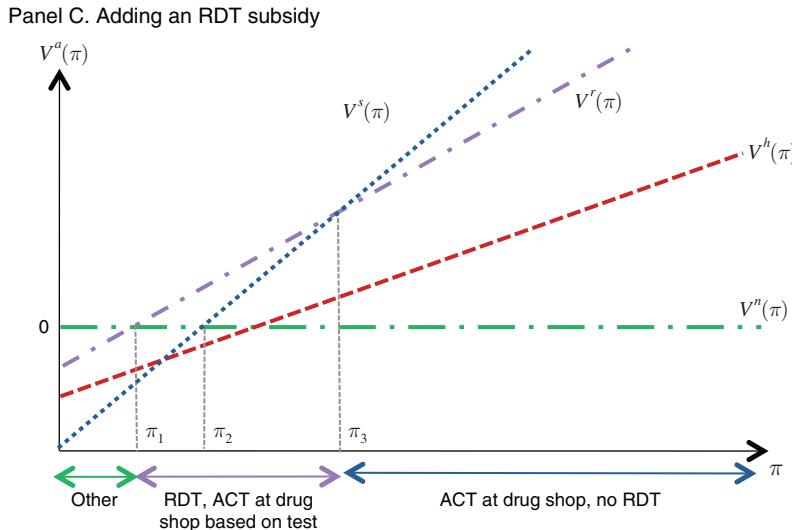


FIGURE 1. THEORETICAL IMPACT OF ACT AND RDT SUBSIDIES (Continued)

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 the drug shop if the RDT is positive.

B. 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 panel B of Figure 1. Access (the fraction of illnesses treated with ACTs) therefore increases, even if all crowd-out is from the health center: in this case malaria-negative illnesses previously screened out at the health center will now receive ACTs at the drug shop. Note that this increase in access always comes at the expense of decreased targeting. This is because crowd-out from the health center always worsens targeting, and crowd-out from doing nothing (action n) increases ACT taking for illnesses with lower malaria probabilities than those that were treated before the price reduction. The key assumption driving this result is that households are willing to pay more for ACTs when they think they are more likely to have malaria—that is, that $V^s(\pi) - V^n(\pi)$ increases with 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 was 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.

C. 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: (i) 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; (ii) if the test is negative, the individual will be more likely to select an appropriate medication.⁸ Panel C of Figure 1 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 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

⁸There are other potential advantages to taking an RDT that we discuss in Section VC.

subsidy.⁹ 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.

III. Study Design, Data, and Empirical Background

A. Experimental Design

The experiment was conducted in the districts of Busia, Mumias, and Samia in Western Kenya between May and December of 2009. Malaria is endemic in this region with transmission occurring year-round, but with two peaks corresponding to heavy rain in May–July and October–November. Like much of sub-Saharan Africa, 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 4-kilometer radius) of each of these shops.¹⁰ 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.¹¹ 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.

⁹Note that if people internalized the externality that over-treatment 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 over-treatment but as long as households have imperfect information on their true malaria status, the potential trade-off between access and over-treatment would remain.

¹⁰Participating 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.

¹¹The 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).

Catchment area census: target 2,928 households

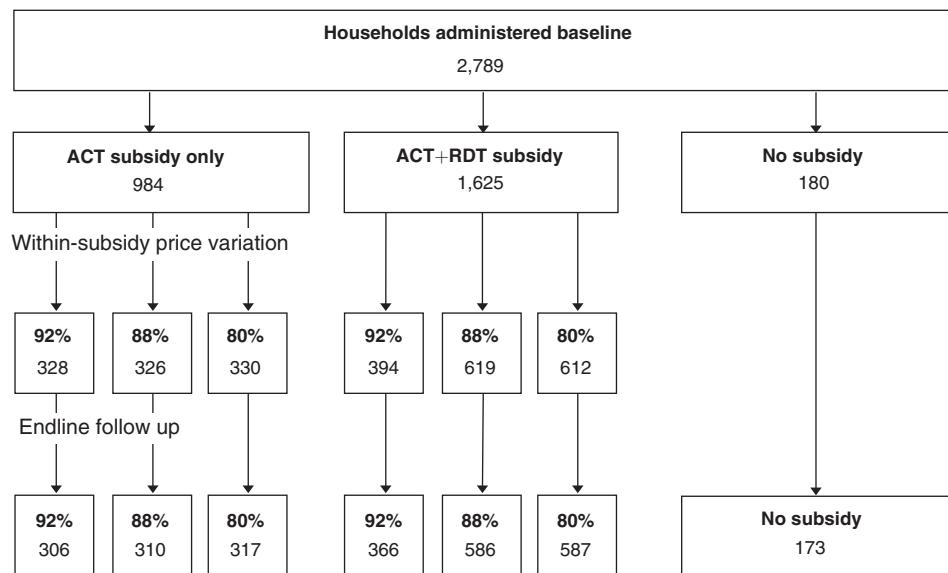


FIGURE 2. EXPERIMENTAL DESIGN AND ATTRITION: NUMBER OF HOUSEHOLDS PER STUDY ARM

Notes: At the end of the baseline survey each household received two ACT vouchers and, if sampled for the RDT subsidy, two RDT vouchers. Forty-nine 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 subsidy levels we group them together for simplicity. Details for the impact of the different RDT subsidies are provided in online Appendix Table A6.

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.¹² The second group received an ACT subsidy only. This treatment was meant to reflect outcomes under the planned AMFm pilot for Kenya (i.e., without RDTs). The third group received vouchers for both subsidized ACTs and RDTs.

Within the two ACT subsidy groups (“ACT subsidy only” and “ACT+RDT subsidy”), households were randomly assigned to an ACT subsidy level of 92, 88, or 80 percent (corresponding to \$0.50, \$0.75, and \$1.25 for an adult dose, respectively). The 92 percent subsidy level corresponds to the Kenyan government’s target retail price of KSh 40 under the AMFm. The lower subsidy amounts reflect prices that could be realized if the subsidy amount were reduced, potentially to fund RDT subsidies. This price range also roughly corresponds to the price range for the

¹²The 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.

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 online 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 subsample 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.¹³

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. Online 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.¹⁴

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, online 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, Krieger, and Yekutieli 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

¹³ Respondents could request a refund for the ACT they had just purchased if the test result was negative. Ninety-three percent of those offered the surprise RDT consented to be tested (or consented for their sick dependent to be tested).

¹⁴ 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 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.

TABLE 1—BASELINE SUMMARY STATISTICS

| Control group mean (1) | Regression coefficients and standard errors | | | | | |
|--|---|--------------------------------|--------------------------------|---------------------|--|---------------------|
| | 92 percent ACT subsidy (T1) | 88 percent ACT subsidy (T2) | 80 percent ACT subsidy (T3) | RDT subsidy (T4) | Joint test: all subsidies = 0 (6) | Observations (7) |
| | | | | | | |
| <i>Characteristics of interviewed household head</i> | | | | | | |
| Female | 0.867 [0.341] | 0.017 (0.029) | 0.029 (0.028) | 0.040 (0.028) | 0.010 (0.012) | 1.25 {0.287} |
| Age (years) | 41.7 [17.3] | -1.98 (1.46) | -3.22** (1.44) | -2.44* (1.45) | 0.185 (0.626) | 1.61 {0.170} |
| Education (years) | 5.10 [4.00] | 0.141 (0.343) | 0.381 (0.341) | 0.151 (0.342) | 0.169 (0.161) | 1.17 {0.323} |
| Literate | 0.575 [0.496] | 0.047 (0.042) | 0.050 (0.042) | 0.027 (0.042) | 0.000 (0.020) | 0.621 {0.647} |
| Married | 0.783 [0.413] | -0.015 (0.035) | 0.004 (0.035) | 0.006 (0.034) | -0.015 (0.016) | 0.514 {0.725} |
| Subsistence farmer | 0.589 [0.493] | 0.052 (0.042) | 0.039 (0.042) | 0.059 (0.042) | -0.005 (0.019) | 0.612 {0.654} |
| Number dependents | 4.12 [2.78] | -0.263 (0.223) | -0.096 (0.221) | -0.077 (0.222) | 0.021 (0.098) | 0.809 {0.519} |

(Continued)

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.¹⁵

B. 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 (km) from the drug shop for which vouchers were given and 6.6 km 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

¹⁵ 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.

TABLE 1—BASELINE SUMMARY STATISTICS (*Continued*)

| | Control group mean | Regression coefficients and standard errors | | | | | |
|--|--------------------|---|------------------------|------------------------|-------------------|-------------------------------|--------------|
| | | 92 percent ACT subsidy | 88 percent ACT subsidy | 80 percent ACT subsidy | RDT subsidy | Joint test: all subsidies = 0 | Observations |
| | | (1) | (2) | (3) | (4) | (5) | (6) |
| <i>Household characteristics</i> | | | | | | | |
| Number members | 5.48 [2.77] | -0.354 (0.217) | -0.233 (0.214) | -0.197 (0.215) | 0.024 (0.092) | 0.885 {0.472} | 2,789 |
| Fraction adults (ages 14+) | 0.623 [0.235] | -0.035* (0.020) | -0.048*** (0.019) | -0.024 (0.020) | 0.002 (0.009) | 2.23* {0.063} | 2,337 |
| Acres land | 2.72 [3.69] | -0.660** (0.330) | -0.601* (0.327) | -0.571* (0.324) | 0.197* (0.117) | 1.63 {0.164} | 2,250 |
| Distance from drug shop (km) | 1.68 [0.917] | 0.012 (0.023) | 0.012 (0.022) | 0.002 (0.022) | 0.010 (0.011) | 0.523 {0.719} | 2,788 |
| Distance from closest clinic (km) | 6.57 [2.47] | -0.018 (0.060) | -0.036 (0.059) | -0.043 (0.059) | 0.044* (0.027) | 0.796 {0.528} | 2,785 |
| <i>Baseline malaria knowledge and health practices</i> | | | | | | | |
| Number bednets | 1.77 [1.43] | -0.031 (0.120) | -0.060 (0.121) | 0.028 (0.120) | 0.005 (0.057) | 0.476 {0.753} | 2,784 |
| Share HH members slept under net | 0.561 [0.397] | 0.023 (0.034) | 0.006 (0.034) | 0.030 (0.034) | -0.012 (0.017) | 0.612 {0.654} | 2,661 |
| Only mosquitoes transmit malaria | 0.517 [0.501] | 0.045 (0.042) | 0.011 (0.042) | 0.024 (0.042) | -0.020 (0.020) | 0.842 {0.499} | 2,789 |
| Heard of ACTs | 0.399 [0.491] | 0.016 (0.042) | 0.017 (0.041) | 0.030 (0.042) | 0.001 (0.020) | 0.197 {0.940} | 2,771 |
| ACT is preferred antimalarial | 0.207 [0.406] | -0.023 (0.034) | -0.029 (0.034) | -0.049 (0.033) | -0.002 (0.015) | 0.978 {0.418} | 2,771 |
| Heard of RDTs | 0.128 [0.335] | 0.039 (0.030) | 0.020 (0.029) | 0.021 (0.029) | -0.011 (0.014) | 0.682 {0.604} | 2,786 |
| Treats water regularly | 0.408 [0.493] | -0.036 (0.041) | -0.018 (0.041) | 0.004 (0.041) | 0.023 (0.019) | 1.13 {0.339} | 2,779 |
| Number of presumed malaria episodes last month | 1.20 [1.22] | 0.015 (0.102) | -0.008 (0.103) | -0.029 (0.103) | 0.033 (0.050) | 0.200 {0.939} | 2,789 |
| <i>Cost per episode (among those seeking care)</i> | | | | | | | |
| Total cost (US \$) | 1.63 [1.86] | 0.140 [0.293] | -0.040 [0.250] | -0.217 [0.238] | 0.131 [0.174] | 0.725 {0.575} | 1,319 |
| Sample size in treatment | 180 | 328 | 326 | 330 | 1,625 | | |

Notes: 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. The exchange rate at the time of the study was around 78 Ksh to US\$1.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Source: Baseline survey.

balance across our experimental groups, we regressed each dependent variable in Table 1 on a dummy variable for each of the three ACT subsidy levels and a dummy variable for the RDT subsidy. Columns 2–5 present coefficients and standard errors from these regressions. The sixth column presents *F*-statistics and *p*-values for a test

of whether 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.

C. 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 5 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 (online Appendix Table A2).¹⁶ 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 (i) care was sought at a participating drug shop

¹⁶There is evidence that the three ACT subsidy groups have differential reporting in terms of the incidence of illness (column 1 of Table A2, 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).

and (ii) 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. Online Appendix A gives additional detail on how we constructed predicted positivity for all illness episodes enumerated at endline.

D. 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 panel A of Figure 3, 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 five years old (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 the malaria literature. Smith et al. (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.”¹⁷

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

¹⁷The 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.

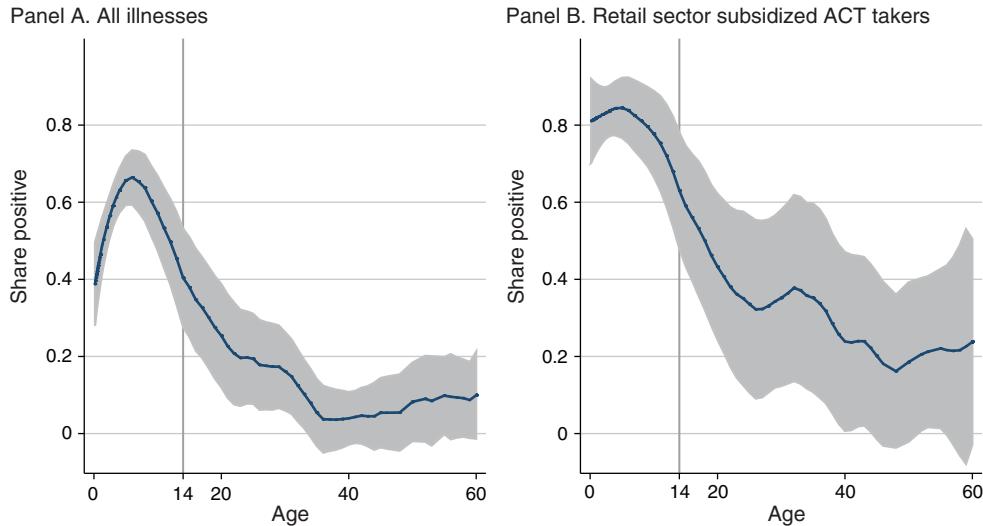


FIGURE 3. MALARIA POSITIVITY AMONG ALL ILLNESSES AND SUBSIDIZED ACT TAKERS

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.

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 $p < 0.001$).

IV. 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 IV.A and IV.B respectively. Then in subsection IV.C we discuss underlying mechanisms that could be driving our results. After discussing impacts of the RDT subsidy on A and T in Section V, we plug our estimates into the formulas for UT and OT in Section VI, to assess the cost effectiveness of alternative subsidy regimes.

A. 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:

$$(2) \quad y_{eh} = \delta + \mathbf{ACTsub}_h' \boldsymbol{\alpha} + \mathbf{x}_h' \boldsymbol{\gamma} + \lambda_{strata} + \varepsilon_{eh},$$

TABLE 2—IMPACT OF ACT SUBSIDY ON TREATMENT SEEKING AND ACT ACCESS

| | Took ACT from drug shop (1) | Took ACT from health center (2) | Visited drug shop (4) | Visited health center (5) | Sought no care (6) | Took malaria test (7) | Took antibiotic (8) | |
|---|--------------------------------|------------------------------------|--------------------------|------------------------------|-----------------------|--------------------------|------------------------|----------------------|
| <i>Panel A. Pooled impact</i> | | | | | | | | |
| Any ACT subsidy | 0.187*** (0.038) | 0.222*** (0.031) | -0.038 (0.030) | 0.167*** (0.046) | -0.079* (0.042) | -0.096*** (0.036) | -0.014 (0.038) | -0.072** (0.034) |
| <i>Panel B. Impact by subsidy level</i> | | | | | | | | |
| B1. ACT subsidy = 92 percent | 0.225*** (0.053) | 0.249*** (0.046) | -0.024 (0.037) | 0.159*** (0.058) | -0.055 (0.053) | -0.110*** (0.042) | -0.031 (0.048) | -0.046 (0.043) |
| B2. ACT subsidy = 88 percent | 0.161*** (0.050) | 0.217*** (0.043) | -0.056 (0.037) | 0.167*** (0.058) | -0.070 (0.052) | -0.097** (0.042) | -0.042 (0.047) | -0.062 (0.040) |
| B3. ACT subsidy = 80 percent | 0.178*** (0.048) | 0.206*** (0.042) | -0.035 (0.035) | 0.173*** (0.054) | -0.106** (0.047) | -0.085* (0.045) | 0.023 (0.046) | -0.100*** (0.038) |
| p-value: B1 = B2 = B3 = 0 | 0.000*** | 0.000*** | 0.498 | 0.004*** | 0.164 | 0.048** | 0.533 | 0.066 |
| p-value: B1 = B2 = B3 | 0.531 | 0.723 | 0.660 | 0.968 | 0.535 | 0.846 | 0.362 | 0.304 |
| DV mean (control group) | 0.190 | 0.071 | 0.119 | 0.488 | 0.286 | 0.226 | 0.214 | 0.185 |
| Observations | 631 | 631 | 631 | 631 | 631 | 631 | 631 | 631 |

Notes: “Substandard” malaria treatment includes non-ACT antimicrobials 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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Source: Endline survey.

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. Panel A of Table 2 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.¹⁸ 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

¹⁸If 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 online 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.

for a surprise RDT at the drug shop, as these could modify the effect of the ACT subsidy on treatment. (Online 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 of 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 one-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 16–23 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 not seeking any care decreases by 9–11 percentage points (around 42 percent: column 6 of Table 2). 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 over-treatment with antibiotics) or negatively (an increase in under-treatment). Finally, we also find some marginally significant evidence that the ACT subsidy crowded out other antimalarials and antipyretics (fever reducers) (results not shown).

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

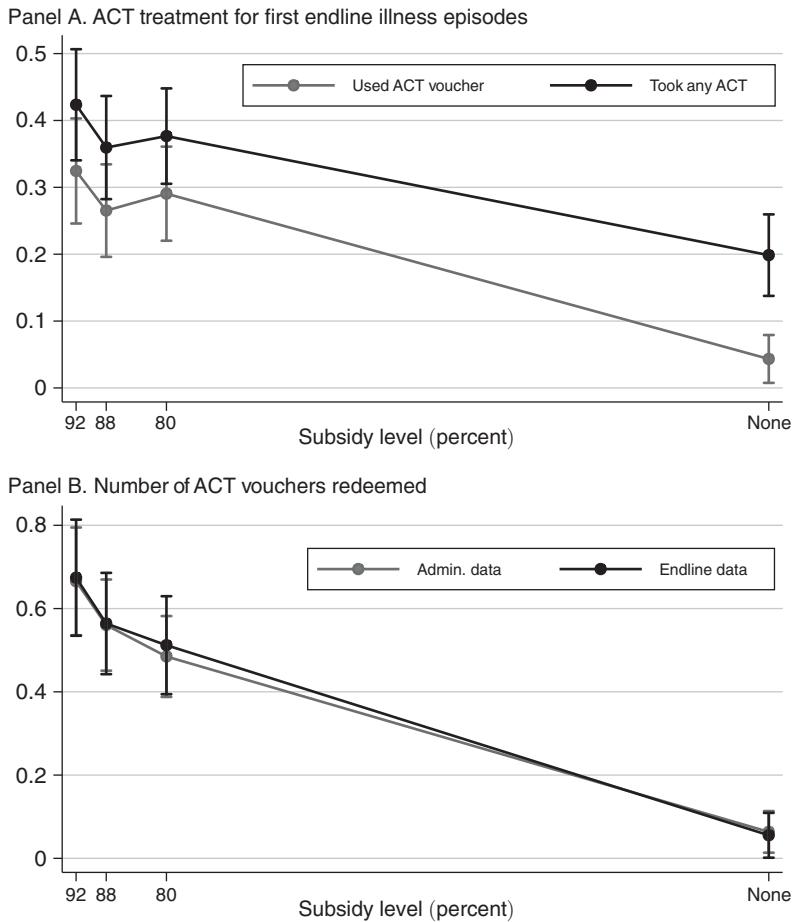


FIGURE 4. ACT DEMAND BY SUBSIDY LEVEL

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).

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.

B. 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:¹⁹

$$(3) \quad pos_h = \beta_0 + \beta_1 ACT88_h + \beta_2 ACT80_h + \varepsilon_h,$$

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 under-weight 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 column 1 of Table 3. 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 Table 3). 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 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*

¹⁹ 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 VIIA, we will conservatively assume that targeting of retail sector ACTs in that regime is 100 percent.

TABLE 3—IMPACT OF RETAIL SECTOR ACT SUBSIDY ON ACT TARGETING

| | Actual malaria status (1) | Predicted positivity (2) | Predicted positivity (3) |
|----------------------------------|------------------------------|-----------------------------|-----------------------------|
| A. ACT subsidy = 88 percent | 0.187** (0.081) | 0.112*** (0.042) | 0.111** (0.053) |
| B. ACT Subsidy = 80 percent | 0.182** (0.084) | 0.107** (0.043) | 0.040 (0.052) |
| <i>p</i> -value: A = B = 0 | 0.038** | 0.012** | 0.104 |
| <i>p</i> -value: A = B | 0.955 | 0.906 | 0.179 |
| DV mean (ACT 92 percent, no RDT) | 0.563 | 0.424 | 0.422 |
| Observations | 190 | 189 | 178 |
| Data source | Admin. | Admin. | Endline |

Notes: The omitted category is the 92 percent ACT subsidy group. Sample in columns 1 and 2 include all first ACT voucher redemptions among households selected for a surprise RDT and no RDT voucher (in column 2, one 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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

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.

C. 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

TABLE 4—MECHANISMS BEHIND ACT TARGETING EFFECTS

| | Used first voucher for patient under 14 (1) | Used first voucher for patient 14 or older (2) |
|--|---|--|
| <i>Panel A. Does the ACT subsidy level reallocate ACTs across dosage groups?</i> | | |
| A. ACT subsidy = 88 percent | 0.035 (0.035) | -0.057** (0.027) |
| B. ACT subsidy = 80 percent | 0.031 (0.034) | -0.080*** (0.026) |
| <i>p</i> -value: A = B = 0 | 0.540 | 0.007*** |
| DV mean (ACT 92 percent, no RDT) | 0.268 | 0.171 |
| Observations | 984 | 984 |
| Subsample | All households | All households |
| | Surprise RDT result: patient under 14 | Surprise RDT result: patient 14+ |
| <i>Panel B. Does the ACT subsidy level reallocate ACTs within dosage groups?</i> | | |
| A. ACT subsidy = 88 percent | 0.060 (0.082) | 0.256* (0.148) |
| B. ACT subsidy = 80 percent | 0.066 (0.083) | 0.170 (0.160) |
| <i>p</i> -value: A = B = 0 | 0.687 | 0.192 |
| DV mean (ACT 92 percent, no RDT) | 0.791 | 0.214 |
| Observations | 132 | 58 |
| Additional controls | None | None |

Notes: The omitted category is the 92 percent 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 four ACT dose groups (based on patient age). Heteroskedasticity robust standard errors in parentheses.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

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.²⁰

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

²⁰Online Appendix Figure A4 replicates Figure 4 by age group, to graphically show the notably different patterns of demand for adults and children.

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 panel A of Figure 4, appears to be nonlinear 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 nonlinearity is likely explained by the fact that the cheapest alternative (non-ACT) antimalarial treatments in our study area 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 online 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.²¹ Over-treatment 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.

V. Results: Impact of Adding an RDT Subsidy

A. 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).²² We estimate a pooled RDT treatment

²¹ 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.

²² 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 significance on behavior for this group. Our results are largely unchanged, though less precisely estimated given the drop in sample size, when excluding these households.

TABLE 5—IMPACT OF RDT SUBSIDY ON TREATMENT SEEKING AND ACT ACCESS BY ACT PRICE

| | Visited drug shop (1) | Visited health center (2) | Sought no care (3) | Took malaria test (4) | Took RDT test (5) | Took microscopy test (6) | Took ACT (7) | Took antibiotic (8) |
|---|--------------------------------|------------------------------------|-----------------------------|--------------------------------|----------------------------|-----------------------------------|--------------------|---------------------------|
| <i>Panel A. Across all ACT subsidy levels</i> | | | | | | | | |
| RDT subsidy | 0.004 (0.026) | -0.013 (0.022) | 0.010 (0.018) | 0.216*** (0.023) | 0.215*** (0.017) | -0.014 (0.018) | 0.018 (0.026) | 0.020 (0.017) |
| DV mean (no RDT) | 0.657 | 0.212 | 0.123 | 0.207 | 0.076 | 0.125 | 0.389 | 0.110 |
| <i>Panel B. By ACT subsidy level</i> | | | | | | | | |
| RDT subsidy × 92% | -0.005 (0.048) | -0.018 (0.042) | 0.029 (0.032) | 0.258*** (0.044) | 0.263*** (0.034) | -0.019 (0.034) | 0.002 (0.050) | 0.004 (0.033) |
| ACT subsidy | | | | | | | | |
| RDT subsidy × 88% | 0.026 (0.046) | -0.045 (0.041) | 0.007 (0.030) | 0.252*** (0.039) | 0.229*** (0.030) | 0.000 (0.032) | 0.042 (0.044) | -0.016 (0.030) |
| ACT subsidy | | | | | | | | |
| RDT subsidy × 80% | -0.012 (0.043) | 0.023 (0.035) | -0.003 (0.033) | 0.152*** (0.040) | 0.166*** (0.029) | -0.021 (0.030) | 0.016 (0.041) | 0.070** (0.028) |
| ACT subsidy | | | | | | | | |
| 88% ACT subsidy | -0.006 (0.058) | -0.002 (0.052) | 0.014 (0.038) | -0.013 (0.048) | 0.004 (0.032) | -0.016 (0.041) | -0.067 (0.058) | -0.011 (0.038) |
| 80% ACT subsidy | 0.009 (0.055) | -0.041 (0.047) | 0.020 (0.040) | 0.050 (0.049) | 0.028 (0.032) | 0.007 (0.040) | -0.058 (0.056) | -0.047 (0.035) |
| p-value: RDT terms jointly = 0 | 0.938 | 0.612 | 0.832 | 0.000*** (0.000***) | 0.000*** (0.000***) | 0.851 | 0.787 | 0.079* |
| DV mean (ACT 92%, No RDT) | 0.667 | 0.222 | 0.104 | 0.194 | 0.069 | 0.125 | 0.444 | 0.125 |
| Observations | 1,993 | 1,993 | 1,993 | 1,993 | 1,993 | 1,993 | 1,993 | 1,993 |

Notes: “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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Source: Endline survey.

effect (panel A), as well as the RDT treatment effects separately by ACT subsidy level (panel B) with the following regression:

$$(4) \quad y_{eh} = \beta_0 + \beta_1 RDT_h \times ACT92_h + \beta_2 RDT_h \times ACT88_h \\ + \beta_3 RDT_h \times ACT80_h + \beta_4 ACT88_h + \beta_5 ACT80_h \\ + \mathbf{x}'_h \gamma + \lambda_{strata} + \varepsilon_{eh}.$$

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 15–26 percentage points (column 4 of Table 5). 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 (online Appendix Table A4). Columns 7–8 show that despite this large increase in diagnostic testing, we observe no overall change in use of ACTs or antibiotics (we also find no evidence of change in use of other antimalarials). 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.

B. *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 of Table 6, and separately by subsidy levels in panel B. Focusing first on the pooled results, column 1 of panel A 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 of Table 6 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 eight 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

TABLE 6—IMPACT OF RDT SUBSIDY ON ACT TARGETING

| | | Surprise RDT reveals that patient is malaria-positive | | Proportion that redeemed RDT voucher, conditional on seeking treatment at drug shop (4) |
|---|---|--|--|--|
| | Household sought treatment at drug shop (1) | Sample: patients who visited drug shop (2) | Sample: patients who bought subsidized ACT at drug shop (3) | |
| <i>Panel A. Across all ACT subsidy levels</i> | | | | |
| RDT subsidy | 0.025 (0.026) | 0.009 (0.039) | 0.081** (0.039) | 0.818 |
| <i>Panel B. By ACT subsidy level</i> | | | | |
| RDT subsidy × 92% ACT subsidy | 0.028 (0.045) | 0.127* (0.070) | 0.163** (0.070) | 0.792 |
| RDT subsidy × 88% ACT subsidy | 0.052 (0.044) | -0.058 (0.063) | 0.018 (0.062) | 0.837 |
| RDT subsidy × 80% ACT subsidy | -0.010 (0.047) | -0.047 (0.068) | 0.061 (0.067) | 0.818 |
| DV mean (ACT 92%, no RDT) | 0.429 | 0.556 | 0.563 | — |
| Observations | 1,776 | 755 | 687 | 573 |

Notes: 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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Source: Administrative drug shop data.

upper bound.²³ The limited targeting impact of RDTs is confirmed by an analysis based on predicted malaria positivity, presented in online Appendix Table A5.

C. 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 one-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

²³ 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 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 column 1 of Table 3.

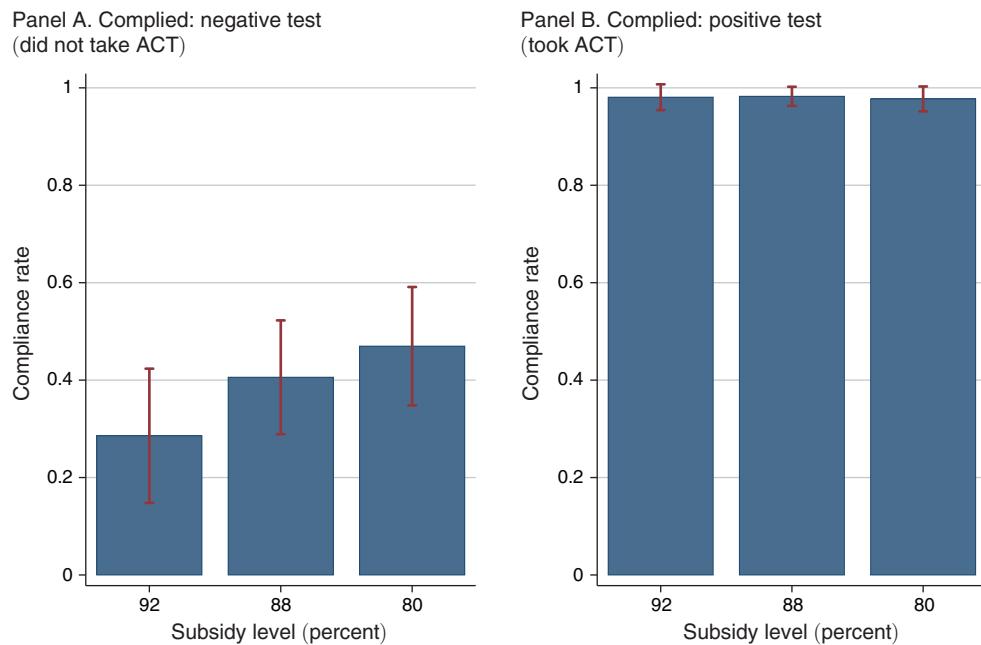


FIGURE 5. COMPLIANCE WITH MALARIA TEST RESULTS

Notes: 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 .

Source: Administrative drug shop data.

ACT subsidy level decreases, but at best less than one-half of those testing negative choose to forgo the ACT. While we explicitly advised that patients five years old and younger 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 years old 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: (i) only 16 percent of households used both ACT vouchers by the end of the study; (ii) we see no relationship between the timing of the first voucher redemption and malaria status at the time of redemption; and (iii) 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

TABLE 7—ESTIMATED IMPACTS OF VARIOUS SUBSIDY SCHEMES ON UNDER- AND OVER-TREATMENT

| | No subsidy (1) | ACT 92 percent subsidy (2) | ACT 88 percent subsidy (3) | ACT 80 percent subsidy (4) | ACT 80 percent + RDT subsidy (5) |
|--|-------------------|-------------------------------------|-------------------------------------|-------------------------------------|---|
| <i>Experimental estimates of access and drug shop targeting</i> | | | | | |
| 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 |
| <i>Assumptions for estimates of under- and over-treatment</i> | | | | | |
| Share of illness episodes that are malaria ^a | 0.386 | 0.386 | 0.386 | 0.386 | 0.386 |
| Targeting at health center (medium) ^b | 0.750 | 0.750 | 0.750 | 0.750 | 0.750 |
| Targeting at health center (high) | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| Targeting at health center (low) | 0.650 | 0.650 | 0.650 | 0.650 | 0.650 |
| <i>Under- and over-treatment: Preferred estimates (assuming medium targeting at health center)</i> | | | | | |
| 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 |
| <i>Under- and over-treatment: Alternative estimates (assuming high targeting at health center)</i> | | | | | |
| 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 |
| <i>Under- and over-treatment: Alternative estimates (assuming low targeting at health center)</i> | | | | | |
| 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: Targeting (T) is the share of ACTs taken for illness episodes that are malaria. Over-treatment (OT) is the share of non-malaria episodes treated with an ACT. Under-treatment (UT) is the share of malaria episodes not treated with an ACT. See Section III for the formulas relating T , OT , and UT to the estimated parameters.

^aThe assumption on the share of illness episodes that are malaria (T) 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 IVC for details.

^bWe consider three possible levels of targeting at health centers since there is no clear evidence from the literature on this parameter.

Source: Authors' computations.

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).

VI. Discussion: Mapping to Under- and Over-Treatment

A. Cost Effectiveness

So far our discussion of the results has focused on access and targeting, but as detailed in Section III, 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.²⁴

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 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 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.²⁵ 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.

²⁴We 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.

²⁵Moreover, 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).

B. Welfare

Drawing conclusions about welfare requires information on three inputs:

- (i) 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.
- (ii) 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 under-treatment with antibiotics for pneumonia).
- (iii) 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 (i), (ii), and (iii). In terms of comparing the “no subsidy” to the 80 percent subsidy, we find that at the 80 percent subsidy (i) is higher, which increases welfare, but (ii) and (iii) 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 (i) compared to (ii) and (iii).

Calculating a specific numerical assessment of the welfare difference would require strong 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 (i) would outweigh the welfare costs of increasing (ii) and (iii). There are two main factors behind this reasoning. First, the welfare effect of (ii) (over-treatment) is likely second-order compared to the effect of (i) (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, p. 45). Thus, we do not expect the cost of capital to be so high that the increase in (iii) (the subsidy cost) outweighs the mortality benefits of reducing under-treatment. That said, if over-treatment with antimalarials delays appropriate treatment for other high-risk illnesses (e.g., antibiotics for pneumonia), the effect of over-treatment 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-Saharan 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 over-treatment with antibiotics) or negatively (an increase in under-treatment with antibiotics).

C. Heterogeneity

As discussed in the theoretical section, the distributional impact of the subsidies is particularly important. In online Appendix H we re-run the main analysis allowing for heterogeneity in effects by (i) socioeconomic status (proxied by literacy status of the interviewed head of household), and (ii) 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 over-treatment more among literate-headed households, and those households have lower rates of compliance with RDTs.

D. 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. Online 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.²⁶ 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

²⁶The 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 of online Appendix Table A6 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.

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 three 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.

VII. 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 anti-malarials, 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-malarial 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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