Patient vs. provider incentives for malaria care in Kenyan pharmacies: A cluster randomized controlled trial

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Abstract

Health policy aims to ensure that patients are able to access high quality care when appropriate and prevent patients from consuming unnecessary treatments. Achieving this balance requires coordination between patients and providers, which can be hindered if incentives are misaligned. I study how patient and pharmacist decision-making in the context of malaria case management is affected by financial incentives. I test whether demand- or supply-side incentives for rapid tests and high quality antimalarials only to malaria-positive cases improve malaria case management and align incentives towards socially-optimal antimalarial use. Using a cluster randomized trial design in 140 pharmacies in malaria-endemic zones in Kenya, I randomize patient discounts and pharmacist performance incentives and compare their effectiveness and cost-effectiveness to the status quo standard of care. In preliminary analyses, I find that both patient subsidies and pharmacy incentives for diagnostic testing significantly increase usage of

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testing and may nudge malaria positive individuals to purchase high quality antimalarials. Data collection is ongoing until early 2022, and analysis is in progress. These early results suggest that appropriately calibrated and targeted financial incentives are promising for changing patient and provider behavior, with implications for quality of care.

1 Introduction

Health policy aims to solve two opposing challenges: ensure that patients are able to access high quality care when appropriate and prevent patients from consuming unnecessary treatments. Achieving this balance requires coordination between patients and providers. In many low-income countries, health care markets are decentralized, making top-down coordination difficult. However, both over- and under-treatment are ubiquitous across disease areas and have negative consequences for patient outcomes [8, 37]. For example, studies in both the US and China have found high levels of unnecessary antibiotic prescribing [7, 14], which affects patient outcomes and contributes to growing rates of drug resistance, which is a societal cost. Studies across sub-Saharan Africa have found that large shares of malaria-negative patients receive antimalarials at health centers and pharmacies [6, 4, 32, 1], which contributes to resistance and limits medication supply for those who may need it most.

Malaria is an important clinical area to study how coordination between patients and providers impacts quality of care because it is a well-understood illness, it has a high disease burden, and nearly all deaths and serious illness are preventable through effective, and inexpensive medication that can be targeted to patients [33]. Diagnostic tests are widely available across high-burden areas in sub-Saharan Africa and are highly accurate in confirming the presence or absence of malaria parasites in a symptomatic patient. In practice however, few malaria patients are diagnosed prior to getting treated. This contributes to a gap between treatment and need: missed diagnoses result in avoidable illness, and over-prescription of antimalarials can lead to drug resistance. In the context of pharmacies, where over half of malaria patients in Kenya access treatment [29], this may be due to misaligned incentives between patients and pharmacists. Studies in Kenya have found that patients may not ask to be tested because they have prior beliefs about their malaria status [25], the cost of the test is prohibitive [4, 31], or they do not want to wait for the diagnostic test result. Pharmacists may skip testing because they are optimizing patient preferences and profit motivations [7, 23].

I test whether financial incentives can improve coordination between patients and providers to impact malaria treatment and cost-effectively mitigate the social costs of misallocating

antimalarials. Using a cluster randomized controlled trial in 140 pharmacies in malaria-prone counties in Kenya, I evaluate the effect of patient subsidies and provider performance incentives on malaria testing and treatment decisions. I investigate whether financial incentives are more effective at improving diagnostic testing and treatment targeting when they are given to patients through subsidies (demand-side) or providers through performance incentives (supply-side). Pharmacies were randomized to a status quo control group or one of three treatment groups: (1) patient discounts for rapid tests and for high quality treatments conditional on a positive test; (2) pharmacy incentives for selling rapid tests to diagnose fevers and for selling high quality treatments conditional on a positive test; and (3) patient discounts and pharmacy incentives for rapid test use and high quality treatment use for confirmed malaria-positive cases. This design allowed me to evaluate the impact of two-part incentive structures as well as to examine the causal effect of targeting that incentive to the patient (demand-side) or the provider (supply-side). Prior literature that has studied the impact of demand-side subsidies on malaria care has found them to be effective at improving testing but not as effective at improving test result adherence [4, 32, 31]. This study is the first to my knowledge to compare supply-side and demand-side incentives at this scale.

The study takes place in Kenya, where over 3.5 million people fall ill with malaria annually, with children, pregnant women and people living near Lake Victoria and on the coast most vulnerable to infection [20, 15]. Over half of malaria patients in Kenya access treatment via pharmacies, often the preferred access point for primary care given pharmacies' convenience and reliable presence even in areas that are underserved by public health care clinics and hospitals [29, 3]. Given that pharmacies play a crucial role in providing access to malaria case management in Kenya, it is essential that they provide appropriate diagnostic testing and low-cost, effective and appropriate medicines for treatment.

In preliminary analyses, I find that both patient subsidies and provider incentives are effective at increasing diagnostic testing uptake and may be effective at improving malaria treatment targeting. Patient subsidies increase the likelihood that a symptomatic patient takes a rapid test by 26 percentage points, from a control group mean of 7 percent. Provider incentives increase the likelihood of rapid test uptake by 23 percentage points, which is indistinguishable from the demand-side approach. This result is consistent with what has been found in prior literature on subsidies for health goods: demand-side subsidies are effective in encouraging adoption of the target behavior when the incentive is appropriately timed. I also find that provider incentives increase the likelihood that a patient purchases high quality treatment with a diagnostic test by 10 percentage points compared to a control group mean of 5 percent, with directional evidence that demand-side incentives have similar effects. I find that both demand- and supply-side incentives reduce the likelihood that a patient purchases malaria

treatment without a confirmatory diagnosis by between 21-22 percentage points, compared to 83 percent in the control group. I find no evidence of intervention effects on the volumes of malaria patients seeking care at study pharmacies, suggesting that these findings are driven by behavior change rather than patient sorting. These results are preliminary, as data collection is ongoing through early 2022.

This paper makes four contributions. First, it contributes to our understanding of how incentives targeted at the demand-side or the supply-side can affect health decision-making. Financial incentives are well-established tools used around the world to promote a wide range of health behaviors. Demand-side incentives all operate based on the assumption that either price itself is a barrier to adopting a health behavior, or an incentive can nudge people to overcome other nonpecuniary barriers. Price experiments for health treatments have shown that people do not respond uniformly to prices, and instead the nature of the health decision and timing of the benefits matter [11, 10, 13, 4, 32]. On the supply side, providers influence patient health decisions using their expertise, preferences, and sometimes biases which can have significant effects on quality of care. They can act as gatekeepers to reduce unnecessary medical treatments, or promote overuse [23, 7]. Provider performance incentives have focused on improving quality of care, particularly for maternal and child health outcomes [2, 17]. These studies suggest that properly incentivizing providers can lead to improvements in health care utilization and key health outcomes, but the evidence has been limited to a relatively narrow set of indicators and outcomes. This paper bridges these two literatures in the context of malaria care.

Second, it adds to the literature on how individualized health information and financial incentives can be combined to change health behavior. Lack of accurate information is another barrier to adoption of desirable health behaviors. Evidence suggests that health information is necessary, but not always sufficient to change health behaviors. However, information combined with financial incentives has shown more promise in encouraging health behavior adoption [27, 24, 12]. Additionally, the quality of the information matters: general health information tends to be less effective in changing individual behaviors than individually tailored messages targeted at the key decision-makers [19]. Studies that have examined whether the information provided by a malaria diagnostic test changes treatment-seeking behavior have found mixed results – information is effective in steering some patients towards appropriate treatment options, especially when coupled with an incentive, but many elect to ignore test results when making treatment decisions [4, 31]. This study combines targeted financial incentives with personalized health information in a way that leverages the two steps of the testing and treatment decision-making process.

Third, this paper contributes to our understanding of how pharmacists make decisions. Pharmacies are important access points to health care in many low- and middle-income country contexts and are under-studied in the literature on provider motivation. Prior studies in Kenya have used vouchers that patients could redeem at participating pharmacies but have not studied the decision of malaria case management from the pharmacist's perspective. There is one other ongoing study, to my knowledge, which tests pharmacy incentives and patient subsidies for malaria testing and treatment [38].

Finally, this paper adds to the evidence on the cost-effectiveness of financial incentives for improving malaria case management. I develop a cost-effectiveness framework to quantify the societal costs of over-treatment and benefits of appropriate treatment targeting from a health systems perspective. This is important to contextualize this program's impact and feasibility for scale. I can compare the cost-effectiveness of this approach to improve malaria care with others that have been evaluated in the literature. The framework that I develop for assessing cost-effectiveness can be extended to other settings that are characterized by diagnostic testing availability and over-treatment that can have negative social consequences.

The remainder of this paper is organized as follows: Section 2 provides an overview of the setting, including the malaria context in Kenya. Section 3 discusses the theoretical framework and hypotheses for the main research questions and outcomes. Section 4 describes the experimental design and provides information about the intervention implementation. Section 5 describes the primary and secondary outcomes, and the methods used for analysis of the main impacts. Section 6 presents preliminary experimental results on the main outcomes and effects, including a discussion of results on incentive targeting on the demand and supply side. Section 7 discusses the remaining data that are being collected and pending analyses, Section 8 describes the cost-effectiveness analysis framework, and Section 9 concludes the paper.

2 Background on malaria and Kenyan context

This section provides a description of the malaria burden in Kenya and recent policy advances for handling it, and the role of pharmacies. It follows with a description of the standard of care for appropriate malaria case management, and a brief overview of the evidence on subsidies and performance incentives to improve malaria case management in Kenya.

2.1 Malaria in Kenya

There are an estimated 190 million malaria cases and 391,500 deaths in sub-Saharan Africa per year, which have significant economic impacts disproportionately affecting the poor [16, 39, 18]. In Kenya, over 70% of the population is at risk of malaria, with children, pregnant women and people living near Lake Victoria and on the coast most vulnerable to infection [20].[1] While the current mortality and morbidity burden of malaria remains high, it has fallen over the past decade with improvements in treatment accessibility and prevention [39]. This decline has coincided with targeted global efforts to increase the use of high quality treatments and decrease the price of these medicines in the public and private sectors.

Since 2006, the public health sector in Kenya has offered malaria treatment for free conditional on diagnosis [36], but many patients seek care in the private sector to avoid common barriers such as long wait times, consultation fees, and unreliable medication supply [21]. In fact, over half of malaria patients access treatment via pharmacies, choosing to incur some monetary cost for treatment to get more reliable and faster access to care [4]. Availability of malaria treatment in the private sector is largely due to a subsidy program run by the Affordable Medicines Facility-Malaria between 2011-2016, which provided manufacturer incentives to lower prices of high quality antimalarials for patients to less than $1 USD per treatment course. This subsidy program — which substantially increased access to antimalarials through Africa's private sector pharmacies and drug sellers — ended in 2016 but resulted in an enduring shift to low cost antimalarials in the private sector [34].

One unintended consequence of manufacturer-level subsidies for antimalarials has been low levels of diagnostic testing and high levels of unnecessary treatment. Prior work in Kenya has found that only 29% of patients who self-report having malaria were diagnosed [4]. This is consistent with recent sales trends in Kenyan pharmacy data: malaria tests (rapid test or microscopy) account for only 10% of antimalarial product sales.[2]

[1] Historically, the peak rainy seasons in these areas occur between October-December, and March-June, but recent disruptions in weather patterns and El Niño/La Niña phenomena have led to delayed onset of rains and more unpredictable rainy seasons in recent years. Appendix Figure 8, which plots antimalarial sales volumes over time from pharmacy level microdata from 2018-2020, shows that antimalarial sales spike between November - January and May - July but remain elevated all year in these endemic areas.

[2] Despite low levels of testing, pharmacy staff are knowledgeable about appropriate case management: 87% of providers who responded to a clinical vignette about appropriate case management at baseline (N=164) said they would first have the patient who presented with malaria-like symptoms obtain a diagnostic test prior to prescribing treatment.

2.2 Malaria case management

Malaria presents with general flu-like symptoms including fever, chills and headache. Because malaria symptoms are non-specific, diagnosis based on symptoms alone is insufficient. Instead, the World Health Organization and the Kenyan National Malaria Guidelines recommend that all cases of suspected malaria be diagnosed by microscopy or rapid test.[3] Both sets of guidelines limit antimalarial treatment to cases with positive tests, and patients with negative test results should be reassessed for other common causes of fever and treated appropriately [33, 30]. After diagnosis, the recommended treatment for uncomplicated malaria is a three-day course of artemisinin combination therapy (ACT) for children and adults.[4]

Accurate diagnosis of suspected malaria prior to treatment provides private benefits by giving information to individuals about their illness status and social benefits by decreasing overall rates of overtreatment of febrile illness with antimalarials, which can lead to drug resistance and wasted resources [22]. Private and social benefits may not perfectly align: the cost of a rapid test is equivalent to the cost of antimalarials, and patients must wait 15 minutes for test results so they may prefer to just purchase treatment and then return to the pharmacy if symptoms persist. The primary social benefit of testing is to prevent drug resistance, which is becoming an emerging problem with antimalarials in sub-Saharan Africa [22]. This is particularly important as individuals may make decisions that seem optimal for themselves in the short run, without internalizing the potential downstream effects of their choices.

2.3 Subsidies for malaria case management in Kenya

Over the past decade, there have been many efforts to increase usage of diagnostic testing for malaria in Kenya which have largely focused on using patient subsidies. There are three overarching findings from this literature: (1) subsidizing rapid tests increases diagnosis rates, (2) subsidizing high quality treatment increases patient demand for it, and (3) subsidizing diagnostic tests has mixed results for subsequent treatment decisions. Early efforts examining the effect of subsidizing rapid tests on testing and treatment decisions found that subsidies increase use of diagnostic tests and make some progress on improving treatment targeting but do not close the gap [5, 26].

[3] Rapid diagnostic tests have a sensitivity of 78% and a specificity of 94%, compared to microscopy which is 57% sensitive and 99% specific.[28, 35] In Kenya, microscopy costs around $1.70 USD (excluding any consultation fees) and rapid tests cost around $1.00 USD (average prices from pharmacy sales data from 2019-2021).

[4] More specifically, this is the treatment course for uncomplicated *P.Falciparum* malaria, for children and adults, excluding pregnant women in their first trimester.

A large randomized controlled trial led by Cohen, Dupas and Schaner experimentally varied subsidy levels for high quality antimalarials and cross-randomized a rapid test subsidy in Kenya's malaria endemic areas. This study evaluated the extent of the tradeoff between access to high quality antimalarials and appropriate treatment targeting at different subsidy levels, and whether introducing subsidized rapid tests improved treatment targeting [4]. They found that subsidizing treatments increased use, and that relatively more modest subsidy levels improved targeting. They also found that rapid test subsidies increased testing uptake but did not improve treatment targeting. A more recent randomized controlled trial in Kenya introduced a free rapid test administered by community health workers and a voucher for subsidized treatment for malaria positive individuals that could be redeemed at a pharmacy [31]. Authors of this study found that the combined subsidy increases diagnostic testing and improves treatment targeting.

All of these papers suggest that price is an important barrier for patients when making decisions about malaria testing and treatment, and that using price subsidies to encourage diagnostic testing can improve uptake and can make some progress in improving treatment targeting. However, the size, delivery mechanism and conditionality of the subsidies matter and there is not a consensus yet on the best combination of these elements.

3 Theoretical framework

The primary goal of clinical decision-making is to ensure that patients who seek care are given the diagnosis and treatment recommendations that are best suited to their illness episode. This requires coordination between the patient and provider, as well as interpretation of often noisy and incomplete signals. For many common illnesses, providers can rely on clinical guidelines that provide a set of decision rules to aid in diagnosis and correct case management. For malaria, clinical guidelines are clear: confirm the malaria diagnosis with either a rapid diagnostic test or microscopy prior to administering antimalarials. Rapid tests for malaria are widely available, affordable, and can be administered by a wide range of health professionals. Despite this, most malaria cases are treated presumptively, without any formal diagnosis.

Two types of errors can occur when antimalarials are administered without a diagnostic test: overtreatment of malaria-negative individuals with antimalarials, and undertreatment of malaria-positive individuals with high quality antimalarials (Figure 2). Overtreatment occurs when malaria negative patients still get prescribed antimalarials. This is problematic for two reasons: first, the patient does not receive the appropriate care that she needs given

her true underlying illness status.[5] Second, over-prescribing antimalarials can lead to a rise in drug-resistant strains of malaria. This is a well-founded public health concern,[6] and individuals may not make private decisions that align fully with what is best from a societal perspective. Undertreatment occurs when malaria-positive patients do not get prescribed high quality antimalarials. This may lead patients with a confirmed need for high-quality medication to take a less effective option. This could in turn delay their recovery and put more strain on the health care system, especially if their illness progresses from an uncomplicated to a complicated case of malaria.

Here, I discuss a simple framework of patient and pharmacist decision-making that illustrates their objective functions, and then illustrate how supply- and demand-side incentives may shift behavior towards improved diagnoses and treatment targeting. This theoretical framework informs my experimental design and main hypotheses comparing demand- and supply-side incentives. Then, I aggregate the individual testing and treatment decisions to societal optimals and discuss how this parameter is measured in the experimental design.

3.1 Patient and pharmacist decisions

Patients and pharmacists make a series of coordinated decisions, which can be influenced by a variety of factors, to appropriately manage a suspected malaria case. I illustrate this sequence for patients in Figure 3.[7] The starting point for this framework is that the patient is symptomatic and has decided to seek care at a pharmacy. The first decision that the patient makes is whether or not to take a diagnostic test. The decision to test depends on factors like availability, the pharmacist's recommendation, cost, and the patient's own beliefs about her illness status. If the diagnostic test is expensive, especially relative to the treatment, the patient may avoid purchase due to low willingness or inability to pay.[8] If the patient does not get diagnosed, she does not gain any additional information about her illness status, and she must decide whether to purchase high quality antimalarials or not. If the patient does get diagnosed, she learns her malaria status with a high degree of certainty. At this point, the treatment choices are the same as if she did not get diagnosed, but the clinically

[5] In practice, there are different possible diagnoses given a set of observable symptoms for a malaria-negative patient, including harmless viral infections, and more serious problems that require different treatments to cure.

[6] Chloroquine-resistant P. falciparum has spread to nearly all areas of the world where falciparum malaria is transmitted, making this drug ineffective.[39]

[7] Because this study's target population is febrile patients who seek care at pharmacies, the conceptual framework restricts the scope of the decision to after a patient has already made the decision to seek care in the private sector as opposed to either (a) not seeking care at all, or (b) seeking care at a public clinic.

[8] See Technical Appendix 12.1 for the derivation of the patient's testing and treatment decision.

appropriate course of action is clear.

Pharmacists instead balance patient utility with their own profit motivation when they decide what to prescribe for a given patient encounter, as illustrated in the utility function below:

$$V_t(U^*, \Pi_t) = \alpha E[U^*] + (1 - \alpha)\Pi_t$$

where $E[U^*]$is the optimal expected utility for patients, Π_t represents pharmacy profits from a particular transaction, and α represents the weight that the pharmacist places on the patient's utility as opposed to profitability, which can be thought of as an altruism parameter measuring pro-patient preferences.

The interventions tested in this study aim to align patient and pharmacist objective functions by using financial incentives that incentivize testing and only treating malaria-positive cases. For patients, this reduces the cost barrier to testing and incentivizes use of high quality antimalarials if appropriate. For pharmacists, this makes diagnostic testing more attractive from the perspective of firm profitability by compensating for any lost medication sales that would come from malaria-negative patients choosing to not purchase antimalarials.

3.2 Hypotheses for comparing supply/demand incentives

Now, I generate the hypotheses tested in the experiment when comparing supply and demand-side incentives for testing and treatment targeting.[9] I motivate this by considering a competitive market environment without asymmetric information between patients and pharmacists. Let the aggregate demand for rapid tests be $x(p) = \sum_i x(p_i)$,which is continuous and non-increasing at all $p > 0$. Similarly, let aggregate supply of rapid tests be $q(p) = \sum_i q(p_i)$, which is continuous and nondecreasing at all $p > 0$. Finally, let p^* be the solution to $x(p^*) = q(p^*)$, the equilibrium price which equates aggregate supply and demand.

Suppose that there is a change in p through the introduction of a demand-side subsidy of size s. This subsidy lowers the rapid test price to patients at all points along the demand curve, which results in an outward shift of aggregate demand for rapid tests by the size of the subsidy. This yields a new equilibrium price and quantity, $p^s$and $x(p^s) = q(p^s)$. The price received by pharmacists is p^s, while the price paid by patients is $p^s - s$. The magnitude of the pass-through of the subsidy, and by extension the change in equilibrium price and quantity, depends on the elasticity of the aggregate demand curve. We expect the following changes in equilibrium prices for pharmacists and patients under these extreme scenarios:

[9] I only illustrate the rapid test case, as the treatment targeting case is parallel.

- Perfectly inelastic demand: Complete pass through of subsidy to patients, with equilibrium price received by pharmacists $p^s = p^*$and the equilibrium price paid by patients $p^* - s$.

- Perfectly elastic demand: Zero pass through of subsidy to patients, with equilibrium price received by pharmacists of $p^* + s$ and the equilibrium price paid by patients p^*.

In general, patients will see more of the subsidy passed through to them in the form of lower prices when aggregate demand is more inelastic, and pharmacists will be able to capture more of the subsidy when aggregate demand is more elastic.

If instead of a demand-side subsidy, a supply-side incentive of size s was introduced to reward pharmacists for each rapid test sold, this would induce an outward shift of the aggregate supply curve by the size of the subsidy. In a competitive market with no asymmetric information and the ability to adjust prices, a demand and supply side subsidy of the same magnitude will yield the same changes in equilibrium price and quantity. This yields a new equilibrium price and quantity, $p^s$and $x(p^s) = q(p^s)$. The price received by pharmacists is p^s, while the price paid by patients is $p^s - s$. As in the subsidy case, the extent to which the incentive is passed through to patients in the form of lower prices depends on the elasticity of the aggregate supply curve. We expect the following changes in equilibrium prices for pharmacists and patients under these extreme scenarios:

- Perfectly elastic supply: Complete pass through of incentive to patients in the form of lower prices, with equilibrium price received by pharmacists $p^s = p^*$and the equilibrium price paid by patients $p^* - s$.

- Perfectly inelastic supply: Zero pass through of incentive to patients, with equilibrium price received by pharmacists of $p^* + s$ and the equilibrium price paid by patients p^*.

The model highlights the hypotheses that will be tested empirically for both rapid test uptake and malaria treatment targeting:

- *Hypothesis 1:* In a competitive market without information asymmetries, supply-side and demand-side incentives of the same magnitude increase (decrease) equilibrium quantity (price) by the same amount. If this is the case, I expect to see no difference in rapid test uptake and treatment targeting between demand- and supply-side approaches, or the combination of the two.

- *Hypothesis 2:* If a pharmacy has market power, then supply-side incentives will be captured by the pharmacy, which will result in a smaller change in equilibrium quantity and price relative to demand-side subsidies of the same magnitude. If this is the case, I expect that demand-side incentives will yield higher uptake of rapid tests and treatment targeting than supply-side incentives.

- *Hypothesis 3:* If a pharmacy exercises supplier induced demand, then supply-side incentives will induce pharmacists to sell more RDTs, which will result in a larger change in equilibrium quantity relative to demand-side subsidies of the same magnitude. If this is the case, I expect that supply-side incentives will yield higher uptake of rapid tests and treatment targeting than demand-side incentives.

- *Hypothesis 4*: In the absence of complementarities between demand- and supply-side incentives, the hybrid intervention will result in a change in equilibrium quantity and price that is in between demand- or supply-side incentives alone.

- *Hypothesis 5*: If complementarities between demand- and supply-side incentives exist, the hybrid intervention will result in a change in equilibrium quantity and price that is greater than either demand- or supply-side incentives alone.

Depending on the type of market structure and ability of pharmacies to set prices, demand- and supply-side incentives may have different implications for rapid test uptake and treatment targeting. My hypotheses make predictions for both quantity and price, however I will only be able to measure changes in quantity through my experiment.

3.3 Optimizing treatment targeting

The optimal outcomes from a public health perspective are that (1) confirmed malaria positive patients choose to be treated with high quality antimalarials, and (2) malaria negative patients choose to not be treated with antimalarials, and instead seek further consultation for their symptoms. I write these optimal end state outcomes as conditional probabilities:

$$P(a_1)^{m_1} = P(a_1)^{m_1 t_1} P(m_1)^{t_1} P(t_1) + P(a_1)^{m_1 t_0} P(m_1)^{t_0} P(t_0) \tag{1}$$

$$P(a_0)^{m_0} = P(a_0)^{m_0 t_1} P(m_0)^{t_1} P(t_1) + P(a_0)^{m_0 t_0} P(m_0)^{t_0} P(t_0) \tag{2}$$

where for $i \in \{0, 1\}$: a_i is whether the patient takes a high quality antimalarial, m_i is whether the patient has malaria (true illness status), and t_i is whether the patient takes a rapid test. The superscripts are the conditionality statements: for example, $P(a_1)^{m_1}$is the probability that an individual takes a high quality antimalarial conditional on being malaria positive. Additionally, $P(t_0) = 1 - P(t_1)$. In order to measure the probability that an individual takes a high quality antimalarial conditional on being malaria positive (Equation 1), I need to measure the probability that (1) an individual takes an ACT, conditional on being tested and malaria positive, (2) an individual is malaria positive conditional on being tested, (3) an individual is tested, (4) an individual takes an ACT, conditional on not being tested and being malaria positive, (5) an individual is malaria positive conditional on not being tested, and (6) an individual not being tested. Equation 2 can be interpreted similarly.

Each of these components will be measured in the experimental design and allows for identifying the share of malaria positive individuals who obtain high quality antimalarials and the share of malaria negative individuals who do not obtain antimalarials — the two dimensions of treatment targeting.

I expect the interventions to increase rapid test uptake, and high quality antimalarial targeting through three mechanisms: (1) subsidized testing and treatment conditional on confirmed positivity, (2) increased incentive for pharmacists to promote testing and high quality treatments, and (3) increased supply. Reducing the price of testing is expected to increase the probability of patients choosing to get tested, which will increase the information available to patients and pharmacists when recommending treatment options. Reducing the price of high quality antimalarials conditional on a positive diagnosis is expected to increase the probability of malaria-positive patients choosing to take high quality treatment. Providing pharmacists with incentives for prescribing rapid tests is expected to increase the probability of pharmacists offering symptomatic patients a rapid test, and incentives for treatment targeting is expected to increase the probability of pharmacists offering malaria positive patients high quality treatment. Finally, increased supply of both rapid tests and high quality antimalarials will ensure that these products are available when patients seek treatment.

We can differentiate Equations 1 and 2 with respect to rapid test price (c_t) and treatment price (c_a), to show how changes in price may affect each of the components in these equations[10]:

[10]I reproduce only the differentiation with respect to test price, as the result for treatment price follows a parallel structure.

$$P'_{c_t}(a_1)^{m_1} = P'_{c_t}(a_1)^{m_1 t_1} P(m_1)^{t_1} P(t_1) + P'_{c_t}(m_1)^{t_1} P(a_1)^{m_1 t_1} P(t_1) + P'_{c_t}(t_1) P(a_1)^{m_1 t_1} P(m_1)^{t_1} + P'_{c_t}(a_1)^{m_1 t_0} P(m_1)^{t_0} P(t_0) + P'_{c_t}(m_1)^{t_0} P(a_1)^{m_1 t_0} P(t_0) + P'_{c_t}(t_0) P(a_1)^{m_1 t_0} P(m_1)^{t_0} \quad (3)$$

$$P'_{c_t}(a_1)^{m_0} = P'_{c_t}(a_1)^{m_0 t_1} P(m_0)^{t_1} P(t_1) + P'_{c_t}(m_0)^{t_1} P(a_1)^{m_0 t_1} P(t_1) + P'_{c_t}(t_1) P(a_1)^{m_0 t_1} P(m_0)^{t_1} + P'_{c_t}(a_1)^{m_0 t_0} P(m_0)^{t_0} P(t_0) + P'_{c_t}(m_0)^{t_0} P(a_1)^{m_0 t_0} P(t_0) + P'_{c_t}(t_0) P(a_1)^{m_0 t_0} P(m_0)^{t_0} \quad (4)$$

Changing the price of a rapid test affects the end state probabilities in three places: (1) testing, (2) malaria positivity conditional on testing, and (3) treatment conditional on positive test.[11] The interventions are designed to experimentally vary the price of testing and treatment, so I expect to see exogenous variation in each of these probabilities through the experiment. These end state probabilities are key inputs to the cost effectiveness analysis, which is described in Section 8.

4 Experimental design

This section describes the intervention details and the experimental design. The study flow diagram can be found in Figure 4.

4.1 Experimental treatments: Targeted incentives for testing and treatment

The treatment arms incentivize patients, pharmacists, or both to use malaria rapid tests and to use high quality treatment when confirmed by diagnostic tests. The magnitude of the incentive is held fixed at 200 Kes (~$2 USD) across all three treatment arms.[12] This amount

[11] These also allow me to recover the probabilities of not getting treatment conditional on being malaria negative.

[12] The incentive amount is consistent with prior literature, was determined after an extensive pilot phase, and was calibrated to ensure pharmacy profitability would not be adversely affected, compared to the status quo.

is either given entirely to the patient in the form of a subsidy, entirely to the pharmacy in the form of an incentive divided between the pharmacy owner and attendant, or split between the patient and the pharmacy in the hybrid arm. The four intervention arms are as follows (also in Appendix Table 8):

1. Control group: pharmacy is an active user of the basic sales and inventory management digital platform, and pharmacy manages their own stock of malaria diagnostic tests and treatments. Patients purchase diagnostic tests and treatment at market prices, and pharmacies stock and price these products according to their business practices.

2. Patient incentive group: In addition to the features present at control pharmacies, clients who seek care for suspected malaria cases are eligible for a subsidized rapid test (90% subsidy, a 10 Kes copay) and a subsidized high quality treatment (80% subsidy, a 30 Kes copay) conditional on a confirmed positive malaria diagnosis.

3. Pharmacy incentive group: In addition to the features present at control pharmacies, the pharmacy owners receive an incentive to sell the rapid test (90 Kes), and an additional incentive to prescribe high quality treatments to malaria-positive patients (80 Kes). Pharmacy attendants receive a 30 Kes incentive for recording transaction information in the malaria case management platform.

4. Hybrid group: In addition to the features present at control pharmacies, the clients are eligible for discounted rapid tests (60% subsidy, a 40 Kes copay) and discounted treatment conditional on a positive test result (60% subsidy, a 60 Kes copay). Pharmacy owners receive an incentive to sell rapid tests (15 Kes), and an additional incentive to prescribe high quality treatments to malaria-positive patients (15 Kes). Pharmacy attendants receive a 30 Kes incentive for recording transaction information in the malaria case management platform.

The interventions were operationalized by Maisha Meds, a Kisumu-based healthcare technology company that provides sales and inventory management support to small pharmacies and clinics throughout Kenya. All pharmacies in the sample were existing users of the Maisha Meds sales management platform, which records all pharmacy transactions and product stock. The incentive interventions were integrated into this digital platform and managed centrally by the Maisha Meds team. This means that subsidy and incentive amounts are automatically calculated based on the products that are being bought/sold and verified by implementation staff independent of the pharmacies prior to disbursement to ensure implementation fidelity.

In all three treatment arms, pharmacy staff receive training on the malaria case management digital platform and proper rapid test administration, and rapid tests and high quality treatments are provided on consignment through the program. In all four study arms, pharmacy staff use the basic sales tracking tool to collect details of all pharmacy transactions during the study period and are provided information at the start of the study period about the importance of diagnostic testing for suspected malaria prior to treatment.

4.2 Sample selection and pharmacy randomization

The geographic area of the study consists of counties in malaria endemic and epidemic regions in western Kenya (Figure 5).[13] Within these counties, all pharmacies that were part of the Maisha Meds network were mapped and screened for eligibility. There is significant heterogeneity in what qualifies as a private sector pharmacy in this setting. To ensure adequate regulatory oversight and homogeneity among study sites, only pharmacies that were registered businesses with Kenya's Pharmacy and Poisons Board at the time of onboarding were eligible to participate in the study. In order to be eligible for the study, pharmacies needed to be licensed and registered, be active users of the Maisha Meds digital sales and inventory management tool, be at least 0.5 km away from other study sites,[14] and be willing to be randomized to one of the study arms.

In total, 140 pharmacies across twelve counties in the malaria endemic and epidemic areas of Kenya's western regions were selected to be part of the study. Pharmacies that met these criteria were sequentially randomly assigned to one of the four arms in waves, stratified on average monthly malaria product sales volumes (above/below median), urban/rural, location of pharmacy in lake endemic county, and participation in earlier pilot study phase.[15] Figure 6 shows the geographic span of the experiment across the target regions in Kenya and the final selection of pharmacies. Because interventions were randomized at the pharmacy level, every person seeking care for suspected malaria is eligible for the same intervention.

4.3 Experimental timeline and data collection

Below is the study timeline and a description of the primary sources of data:

[13]The counties included in the study are: Bungoma, Busia, Homa Bay, Kakamega, Kisumu, Migori, Siaya, and Vihiga. Study counties that are not part of the lake endemic region, but still have significant levels of malaria burden, are Bomet, Kisii, Nyamira, Kericho, and Nakuru.

[14]The average distance between study sites is 6.24 km (range of 0.5 km to 46.2 km).

[15]Randomization was done in Stata 16 by the lead investigator.

Jun-Nov '21	**Experiment launch**: baseline pharmacy survey; staggered onboarding of 140 pharmacies to intervention and study
Aug '21-Feb '22	**Monitoring of implementation**: implementation team monitors intervention implementation in all pharmacies through regular outreach calls and random site visits; ongoing administrative data collection through digital platform
Oct-Dec '21	**Patient exit survey**: survey of random sample of approximately 2000 patients who seek care for malaria-like symptoms at study pharmacies
Jan '22	**Control group testing**: testing of random subset of approximately 350 pharmacy clients at control group sites to obtain test positivity rate
Feb '22	**Pharmacy endline survey**: survey of all pharmacy staff and owners at conclusion of the data collection period

I use the following data sources for analysis:

1. *Baseline data:*

   (a) Pharmacy owner survey: survey with owners of 140 pharmacies about number of staff, pharmacy business operations, patient volumes, pharmacy characteristics, costs and revenues, and knowledge of malaria case management.

   (b) Pharmacy staff survey: survey with 140 client-facing pharmacy staff on malaria case management knowledge, worker motivation, and use/familiarity with the status quo digital platform used to manage sales and inventory.

2. *Administrative data:*

   (a) Sales data: continuously collected transaction data including prices and quantities of products purchased, location, date, and time of sale, and pharmacy staff who made the sale for at least 12,000 eligible patient encounters.

   (b) Malaria case management data: continuously collected transaction data on all rapid test and treatment purchases made through incentive program, including information on age/gender of patient, rapid test result, prices and quantities of medications purchased, method of purchase (cash/mobile money), location, date, and time of sale, and pharmacy staff who made the sale.

3. *Patient exit survey data:* survey with a random sample of approximately 2000 eligible adult pharmacy clients across all study sites (10-20/site). In order to be eligible, clients must have sought care for malaria symptoms for themselves or a family member present at the pharmacy with them. This survey includes information on quality of care, symptoms, prices and quantities of medications and diagnostic tests purchased, beliefs about their illness status, malaria test result if applicable, and basic demographics.

4. *Testing subsample data:* data on test positivity from testing of random subset of approximately 350 pharmacy clients at control group sites to obtain test positivity rate in a sample unaffected by the interventions.

5. *Endline data:*

   (a) Pharmacy owner survey: survey with 140 pharmacy owners about number of staff, pharmacy business operations, patient volumes, pharmacy characteristics, costs and revenues, and altruistic tendencies.

   (b) Pharmacy staff survey: survey with 140 client-facing pharmacy staff on malaria case management knowledge, worker motivation, use/familiarity with the digital platform used to manage sales and inventory and manage malaria cases, and altruistic tendencies.

5 Methods

Here, I define the primary and secondary outcomes, present the sample size considerations, and describe the regression framework used in Section 6.

5.1 Primary and secondary outcomes: Malaria case management

All study outcomes are measured in individuals who sought treatment for suspected malaria (for themselves or a household member present with them at the time of care-seeking) at participating study pharmacies. These outcomes are obtained from the administrative data collected at the point of care by the digital tools for tracking sales and managing malaria cases.

There are two primary outcomes:

1. **Rapid test use**: The proportion of rapid test sales recorded at participating pharmacies as a share of total sales to clients with suspected malaria

2. **High quality test with treatment**: Proportion of high quality treatments sold at participating pharmacies with a confirmatory diagnosis (as measured through diagnostic test result)

There are four secondary outcomes:

1. **High quality treatment without test:** Proportion of high quality treatments sold at participating pharmacies without any diagnosis (as measured through diagnostic test sale)

2. **High quality treatment use:** Proportion of high quality antimalarial sales recorded at participating pharmacies as a share of total antimalarial sales

3. **Antimalarial sales**: Proportion of sales recorded at participating pharmacies that are antimalarial products

4. **Inappropriate antimalarial targeting**: Proportion of all antimalarials sold in participating pharmacies that are unaccompanied by a confirmatory diagnosis (as measured through diagnostic test result)

These outcomes examine different dimensions of the same two-part question of patient demand for the information provided by rapid tests and treatment choice. Outcome measure 1 (rapid test uptake) examines uptake for a particular illness episode, when the choice of whether to purchase a diagnostic test is salient and can be thought of in terms of how much the patient values the information provided by that test relative to the cost. Connecting these outcomes to the hypotheses described in Section 3.2, I predict that that compared to the control group, uptake of rapid tests will be higher at subsidized prices. Comparing the two subsidy levels to each other (T1 and T3), I will learn the price elasticity of demand for rapid tests. Comparing the subsidy to the provider incentives (T1 and T2), I will learn whether incentives are passed through to patients. Outcome measure 2 (high quality test with treatment) examines uptake for a given confirmed malaria case, of a high quality treatment. There are alternative treatment options readily available at pharmacies, so this outcome measures the relative impact of subsidy/incentive on treatment choice.

5.2 Sample size calculations

The study has two primary outcomes of interest, rapid test uptake and hiqh quality treatment targeting. The primary comparisons are the effect on the primary outcomes of each of the

three interventions relative to the control arm and the demand-side intervention to the supply-side intervention. The study is powered to detect meaningful minimum effects for each of these primary comparisons at $\beta = 0.85$. For control group comparisons, the study is powered to detect a minimum detectable effect (MDE) of 0.13 for rapid test uptake and 0.12 for high quality treatment targeting. For treatment arm comparisons, the study is powered to detect between 0.17-0.19 MDEs for rapid test uptake and 0.14-0.16 MDEs for high quality treatment targeting. Standard formulas for cluster randomized control trials with individual-level binary outcomes were used to calculate the power for each comparison [9]. These power calculations were conducted conservatively, not adjusting for covariates and using the Bonferroni correction to adjust for multiple comparisons.

5.3 Empirical strategy

The results in this section compare each intervention arm to the control group (status quo pharmacy care experience), so I present all results in terms of these comparisons. Additionally, I discuss any significant differences between demand-side incentives (T1) and supply-side incentives (T2). All analyses are conducted at the patient level[16], and an intention-to-treat (ITT) framework is used. Some clients at intervention pharmacies refuse to purchase rapid tests and treatments through the intervention platform (at a fixed reduced price, in T1 and T3) and will elect to make other purchases or none. By including all eligible malaria patients in an ITT analysis, rather than only patients who elect to take up the intervention assigned to the pharmacy, I preserve the unbiasedness benefits of randomization.

For all binary outcomes, I estimate unadjusted and adjusted logistic regressions using the following regression framework.

$$Pr(Y_{ip}) = expit(\beta_0 + \beta_1 T_{1ip} + \beta_2 T_{2ip} + \beta_3 T_{3ip} + \lambda_s + \epsilon_{ip}) \tag{5}$$

where Y_{ip} is a malaria testing or treatment outcome, T_{jip} are treatment assignment indicators for each intervention j for individual i seeking care at pharmacy p, with the control group as the reference category, $\lambda_s$are strata fixed effects, and $\epsilon_{ip}$is the error term. The β terms represent the log-odds of the treatment effect of each intervention relative to the control group. I report all results in terms of marginal effects in relation to the control group mean, which can be interpreted as elasticities. I also report p-values from Wald tests comparing the marginal effect coefficients of the demand side and supply side interventions. A p-value of less

[16]This is equivalent to febrile illness episode level since most patients in our sample only have had one symtomatic pharmacy visit during the study period.

than 10 percent on these tests indicates that supply and demand-side incentive targeting have differential impact on the outcome of interest. The estimates produced by Equation 5 do not account for baseline pharmacy-level differences in malaria case management between groups, nor do they account for potential confounders that are not completely balanced at baseline (Table 1). I include number of months active on the sales management platform, whether the owner is female, and average monthly baseline malaria sales volumes as covariates in this adjusted model, as these were unbalanced at baseline ($\boldsymbol{X_p}$). I estimate equation 6 to adjust for this imbalance and improve precision.

$$Pr(Y_{ip}) = expit(\beta_0 + \beta_1 T_{1ip} + \beta_2 T_{2ip} + \beta_3 T_{3ip} + \lambda_s + \boldsymbol{X}_p + \epsilon_{ip}) \tag{6}$$

I report all results from the adjusted regressions in the same way that I do for the unadjusted model. In addition to looking at each intervention separately, I report results from pooling the interventions, to measure overall impact of any incentive program on outcomes of interest.

6 Impact of targeted incentives on diagnostic testing and treatment targeting

This section examines the effect of the three interventions on malaria diagnostic testing and treatment targeting outcomes and compares demand- and supply-side approaches to each other. Additionally, it examines the effect of the interventions on antimalarial sales more broadly in study sites. Below I discuss characteristics of the sample at baseline, the results comparing each of the intervention arms to the status quo pharmacy practices (control group) and compare the patient subsidies to the pharmacy incentives. All analyses in this section are preliminary, as data collection is ongoing through February 2022.

6.1 Balance and eligibility checks

Table 1 reports the experimental balance checks at baseline. Balance checks are done at the facility level, as this is the unit of randomization and is the level at which baseline data is available. Column 1 reports overall sample means, and Columns 2-5 report group means for the control group and each of the treatment arms. Significance stars in columns 3-5 indicate that the difference in means when compared to the control group is significant at the 10 percent level or more. Number of months active on the digital sales management platform and whether the pharmacy is owned by a woman are significantly different between T2 and

the control group. Pharmacies in T3 have on average, lower baseline malaria sales volumes than the control group. Columns 6-8 p-values from pairwise means comparisons between T1 and T2, T1 and T3, and T2 and T3, respectively. Compared with T1 facilities, T3 facilities have higher baseline RDT sales and T2 facilities were newer adopters of the sales management platform. No other outcomes assessed at baseline show meaningful imbalances.

Randomization was done prior to enrolling facilities in the study for sites that met all eligibility criteria, due to operational necessity of conducting in person site visits to introduce the program and the study at the same time. Appendix Table 9 reports balance on baseline variables obtained from the administrative data between facilities that when offered participation in the program and study accepted (in sample) and those that declined. Column 3 reports the p-values from pairwise means comparisons of facilities in the sample and those outside of the sample. Facilities that declined to participate in the program and study tend to be ones that have been using the digital sales management platform for longer than facilities in the sample frame. No other meaningful imbalances are found. Appendix Table 10 reports descriptive results from regressing the primary and secondary outcomes on the sample baseline characteristics.

6.2 Results

Table 2 reports results for the primary outcomes, rapid test uptake and treatment targeting, from the unadjusted models, and 3 reports results from the adjusted models. Figure 7 shows the intervention arm means from the unadjusted regressions for the primary outcomes. All three intervention arms significantly improve rapid test uptake when compared to the control group (Column 2). In the control group, only 7 percent of malaria patients purchase a rapid test at the time of making their treatment decisions. By contrast, the patient subsidy intervention increases this likelihood by 25 percentage points. The provider incentive intervention increases rapid test uptake by 21 percentage points compared to the control group, and the hybrid intervention results in a 25 percentage point increase compared to the control group. The adjusted model shows similar effects: all three intervention arms result in an increase in rapid test uptake of between 23-26 percentage points, compared to the control group. I do not find a statistically significant difference between supply- and demand-side incentives on rapid test uptake. As such, I also report results of pooling the interventions (Column 1 of Tables 2 and 3).

Table 2, Columns 3 and 4, present results on the effects of the interventions on uptake of high quality treatment for malaria-positive patients for the pooled interventions and each

one separately. I find suggestive evidence that the provider incentive and the hybrid incentive interventions increase the likelihood that a suspected malaria patient purchases a high quality treatment with an accompanying test by 8 percentage points (Column 4) compared to a control group base of 5 percent. In the adjusted model (Table 3, Columns 3 and 4), I find stronger effects of improved treatment targeting in the provider incentive and hybrid intervention arms. Specifically, the provider incentive arm increases the likelihood that a suspected malaria patient purchases a high quality treatment with an accompanying test by 10 percentage points, while the hybrid arm improves this outcome by 7 percentage points compared to the control group. I do not find a statistically significant difference between supply- and demand-side incentives on treatment targeting in either model. The pooled intervention effects on treatment targeting of high quality antimalrials with a confirmatory diagnosis is 10 percentage points, representing a three-fold improvement in targeting antimalarials.

Tables 2 and 3, Columns 5 and 6, present results on the effects of interventions on an alternative measure of treatment targeting, the likelihood that high quality antimalarials are purchased without a confirmatory diagnosis. Looking at the pooled intervention effects (Column 5) from the adjusted models, I find a 23 percentage point decrease in the likelihood that medication is sold without a diagnostic test, compared to a control group mean of 83 percent. Looking at each intervention separately, I find between a 21-22 percentage point reduction in this outcome, with no differences across intervention arms. The magnitude of these effects almost perfectly offset the increased use of diagnostic testing (Columns 1 and 2), suggesting that this reduction in poorly targeted treatments is due the combined effect of malaria positive patients getting treated and malaria negative patients electing to not purchase medication. The unadjusted model shows similar results.

Tables 4 and 5 report unadjusted and adjusted models for the secondary outcomes. I look at pharmacy visits that resulted in a malaria product sale as a proportion of total pharmacy visits (Columns 1 and 2), high quality treatment uptake as a share of all antimalarial medication sales (Columns 3 and 4), and high quality treatment uptake with test as a share of all high quality treatment uptake (Columns 5 and 6). The first outcome is a check for whether volumes of patients with malaria symptoms changed across intervention arms. The first outcome can be thought of as a check for whether the interventions are effective in discouraging malaria negative patients from purchasing high quality treatments. The third outcome can be thought of as an alternative measure of treatment targeting for malaria positive patients, restricting only to those patients who purchased high quality medication for uncomplicated cases.

I do not find any significant changes in the overall number of suspected malaria patients that

seek care at study pharmacies, by intervention arm (Columns 1 and 2). On average, 19% of pharmacy patients purchase antimalarial products across all study sites. This suggests that these results are not due to selection of patients in or out of the study sites, but instead reflect behavior change that is taking place at the point of interaction between pharmacists and patients. Directionally, all three interventions lead to reductions in high quality treatment sales, with stronger effects in the adjusted model. This suggests that the interventions are effective at shifting patients away from purchasing treatments if they are unlikely to be malaria positive. Finally, I examine an alternative measure of treatment targeting for malaria positive patients, which restricts to only patients who purchase high quality antimalrials (Columns 5 and 6). All interventions significantly increase the number of high quality medications sold with a test 10-15 percentage points, depending on the model. Again, there does not appear to be any difference between demand and supply side targeting approaches for these outcomes. Results are robust to using linear probability models, which are reported in Appendix Tables 11 and 12.

6.3 Summary

The interventions have a meaningful impact on both rapid test uptake and treatment targeting. For rapid test uptake, all three approaches to targeting financial incentives (supply side, demand side, or both) yield large increases in the likelihood that pharmacy patients with suspected malaria get diagnosed. For treatment targeting, I find evidence that provider side incentives and a hybrid incentive model which provides both patient subsidies and provider performance incentives are effective at increasing the likelihood that high quality treatments are appropriately targeted to patients who have a confirmed malaria diagnosis, when compared to the control group. Finally, I find that all interventions reduce the likelihood that antimalarials are sold without an accompanying test. Linking these early findings back to the hypotheses, I do not find evidence of either market power or supplier-induced demand for rapid test uptake, malaria treatment targeting or overall antimalarial sales.

Taken together, these results suggest that appropriately calibrated and targeted financial incentives are promising for improving malaria case management in a pharmacy setting. Incentivizing providers, as is done in the provider only arm and the hybrid arm, may be important for improving treatment targeting outcomes in particular. These results are drawn from the administrative sales data collected between mid-June and the end of December 2021 from 140 pharmacies that had been onboarded and active in the study. They are not yet final and should be interpreted cautiously. Data collection is ongoing until February 2022, and analyses will be updated as more data become available.

7 Analysis plan for remaining data collection

This section describes the remaining data collection and study activities, as well as the remaining pre-specified regression analyses (AEA Registry # 0004705) that will be carried out once data collection is complete.

As of December 31, all 140 pharmacies had been enrolled in the study. Pharmacy owner baseline surveys have been conducted with 135 pharmacy owners, and pharmacy staff baseline surveys have been conducted with 98 staff members from all 140 sites. Patient exit surveys began in October and have been completed with 1601 patients across 124 study sites (89% of the final survey sample, 13 patients/pharmacy). Control group testing to obtain a malaria positivity rate will be done in January 2022 by trained lab technicians. Incentive interventions will run in all study sites through the end of February 2022, and a pharmacy owner and staff endline survey will be conducted at that point to conclude the study period. Analysis of administrative data will continue as more data become available and will be supplemented by analysis of survey data.

I will conduct the main regression analyses presented in this paper with the full sample. This includes evaluating the impact of each intervention arm compared to the control group for rapid test uptake and treatment targeting and comparing the relative effectiveness of demand-side and supply-side incentives for these primary outcomes. I will also compare the effectiveness of the hybrid incentive arm to the demand- and supply-side approaches for rapid test uptake and treatment targeting, to understand whether there are complementarities between the two and to measure the price-elasticity of demand for tests and treatments. These regression analyses will be carried out using the same specifications that are used in Section 5.3, and are all pre-specified in the registered pre-analysis plan. Finally, I will conduct a full cost-effectiveness analysis using parameters obtained from the study data (described in more detail in Section 8).

Additionally, I plan to assess the following dimensions of treatment effect heterogeneity in exploratory analyses:

1. Does the effectiveness of targeted incentives for rapid tests and high quality treatments on treatment targeting vary by provider quality, as measured by patient's assessment of provider quality of care (patient exit surveys) and provider altruism score (pharmacy owner/staff endline survey)?

2. Does the effectiveness of targeted incentives for rapid tests and high quality treatments on treatment targeting vary by provider motivation (pharmacy owner/staff survey)?

3. Do treatment effects differ by provider gender, and gender concordance between provider and patient?

4. Do treatment effects differ by provider type (pharmacy owner or attendant)?

5. Do treatment effects differ by patient gender, age, or socioeconomic status (patient exit surveys)?

These results will contribute to the evidence base on the impact of incentives targeted to patients or providers on health decision-making, the cost-effectiveness of these types of programs from a societal perspective, and how pharmacists make decisions.

8 Cost effectiveness analysis framework

A cost-effectiveness analysis will be carried out after all data have been collected for this study. Below is a planned cost effectiveness analysis framework, which relies on the theoretical model described in section 3.3 and parameters recovered from the experimental design.

8.1 Overview

In order to analyze the efficiency of each incentive intervention, I will conduct a cost-effectiveness analysis from a health system perspective (including program costs, costs incurred by the patient, and public health sector costs of over- or under-treatment). To evaluate cost-effectiveness, I will calculate the ratio between the change in benefits to the change in costs across each intervention arm compared to the status quo. I will look at two measures of benefits separately: the change in (1) malaria positive patients getting high quality treatments, and (2) and malaria negative patients not getting antimalarials. The framework described below allows me to measure whether the interventions tested in the experiment cost-effectively improve adherence to malaria clinical guidelines and reduce the societal costs of misallocating antimalarials.

8.2 Benefits

In Section 3.3, I presented the conditional probabilities for the optimal end state outcomes: (1) confirmed malaria positive patients choose to be treated with high quality antimalarials,

and (2) malaria negative patients choose to not be treated with antimalarials, and instead seek further consultation for their symptoms. They are reproduced below:

$$P(a_1)^{m_1} = P(a_1)^{m_1 t_1} P(m_1)^{t_1} P(t_1) + P(a_1)^{m_1 t_0} P(m_1)^{t_0} P(t_0) \tag{7}$$

$$P(a_0)^{m_0} = P(a_0)^{m_0 t_1} P(m_0)^{t_1} P(t_1) + P(a_0)^{m_0 t_0} P(m_0)^{t_0} P(t_0) \tag{8}$$

The benefits of the program can be derived from the above probabilities, which directly map onto the parameters that are measured through the experimental design and data collection activities. Below are the expressions for measuring the change in the key components of the program benefits that will be used to evaluate cost effectiveness (reproduced from Section 3.3):

$$\begin{aligned} P'_{c_t}(a_1)^{m_1} = {} & P'_{c_t}(a_1)^{m_1 t_1} P(m_1)^{t_1} P(t_1) + P'_{c_t}(m_1)^{t_1} P(a_1)^{m_1 t_1} P(t_1) + \\ & P'_{c_t}(t_1) P(a_1)^{m_1 t_1} P(m_1)^{t_1} + P'_{c_t}(a_1)^{m_1 t_0} P(m_1)^{t_0} P(t_0) + \\ & P'_{c_t}(m_1)^{t_0} P(a_1)^{m_1 t_0} P(t_0) + P'_{c_t}(t_0) P(a_1)^{m_1 t_0} P(m_1)^{t_0} \end{aligned} \tag{9}$$

$$\begin{aligned} P'_{c_t}(a_1)^{m_0} = {} & P'_{c_t}(a_1)^{m_0 t_1} P(m_0)^{t_1} P(t_1) + P'_{c_t}(m_0)^{t_1} P(a_1)^{m_0 t_1} P(t_1) + \\ & P'_{c_t}(t_1) P(a_1)^{m_0 t_1} P(m_0)^{t_1} + P'_{c_t}(a_1)^{m_0 t_0} P(m_0)^{t_0} P(t_0) + \\ & P'_{c_t}(m_0)^{t_0} P(a_1)^{m_0 t_0} P(t_0) + P'_{c_t}(t_0) P(a_1)^{m_0 t_0} P(m_0)^{t_0} \end{aligned} \tag{10}$$

Table 6 displays each component of the benefits equations above along with the method of obtaining this parameter measurement from the experimental design and data collection. Once data collection is complete, I will estimate the program benefits for each intervention using these parameters and compare them to the status quo standard of care.

8.3 Costs

The costs of the program can be broken down into direct costs of running the program, the costs of overtreating malaria negative patients with antimalarials, and other non-programmatic costs to patients of participating in the program:

$$TotalCosts = \sum_{t=0,1,2,3} incentive_t(patients_t) + c_a\left[P(m_0)(untested_t) + negativetx_t\right] + timecost_t(patients_t)$$

where $t \in 0, 1, 2, 3$ is one of the three treatment arms or the control group.

The program costs for the interventions will be obtained from the implementing organization and will include the total incentive amount for each product sold as part of the program. To estimate the total program costs, I use the number of rapid tests sold during the study period through the program multiplied by the intervention arm specific incentive amount for rapid tests (for T1 and T2 this is $1.20 USD, for T3 this is $1.10). I add that to the number of high quality treatments sold during the study period through the program (conditional on a positive test outcome) multiplied by the intervention arm specific incentive amount (for T1 and T2 this is $0.80 USD, for T3 this is $0.90).

The cost of overtreating malaria negative patients is a second key parameter in my cost effectiveness analysis. I will get a measure of the total number of malaria negative patients in each intervention arm by adding together (a) the number of malaria negative patients who purchase antimalarials despite having a negative malaria test result, (b) the number of untested patients multiplied by the probability that they are malaria-positive obtained from the testing data. For an estimate of the total cost of over-treatment in this group, I will multiply the total number of estimated malaria negative patients in each treatment arm by the average cost of the antimalarials purchased.

In addition to the direct costs incurred by the program and the costs of overtreatment of malaria negative patients, patients incur time costs to participate in the program. I use the patient exit survey to estimate the average time spent at the pharmacy seeking treatment in each intervention arm and multiply that by the average hourly wage in the study area, which I will obtain from publicly available survey data. Then, to estimate the total time cost for all participants in the intervention, I multiply that by the total number of study participants.

Table 7 displays each component of the cost equation along with the method of measuring this parameter. Once data collection is complete, I will estimate the program costs for each intervention using these parameters and compare them to the status quo standard of care.

9 Conclusion

This paper analyzes the effect of demand- and supply-side incentives on malaria testing and treatment targeting, examining how patients and pharmacists make decisions. Different from other studies, the experiment directly compares parallel incentive interventions that are targeted to either patients in the form of subsidies or pharmacists in the form of performance incentives. My preliminary results suggest that both patient subsidies and provider incentives are effective at increasing diagnostic testing uptake and may increase the likelihood that high quality treatments are appropriately targeted to malaria-positive patients. However, this study is not without its limitations. The first is the use of administrative data from pharmacy sales records for the main outcomes. Pharmacies may report sales, and malaria cases, inconsistently, or in a way that may bias the results. In order to validate the administrative data, I collect patient exit survey data on a random subset of the sample. The second limitation is that this study identifies the impact on rapid test and treatment decisions at the time of care-seeking and does not follow up with patients to assess their illness trajectory or future behavior. I am not able to report results on adherence to treatment regimen, or whether malaria-negative patients received further counseling. Future research should examine the longer-term effects of these different incentive interventions on patient health outcomes. Finally, while this study suggests that both supply- and demand-side incentives are promising for increasing diagnostic testing and that supply-side incentives are important for improving appropriate access to high quality treatments, these findings may not generalize to other health decisions or contexts.

Taken together, the results suggest that pharmacists influence patients' testing and treatment decisions, and quality of care may be improved by factoring this into future interventions. This provides impetus for future research on how pharmacists influence patient decision-making and whether incentives can be effectively used to improve quality of care in other clinical contexts. These findings also raise more questions about pharmacist and provider motivations, and how they influence quality of care. These are all possible avenues for future research. In terms of policy implications, this study highlights the importance of considering both patient and provider motivations when designing programs to influence health decisions. It suggests that incentives can be effective at improving quality of care when appropriately targeted and emphasizes the important role that pharmacies play in providing access to essential health services in many contexts.

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10 Figures

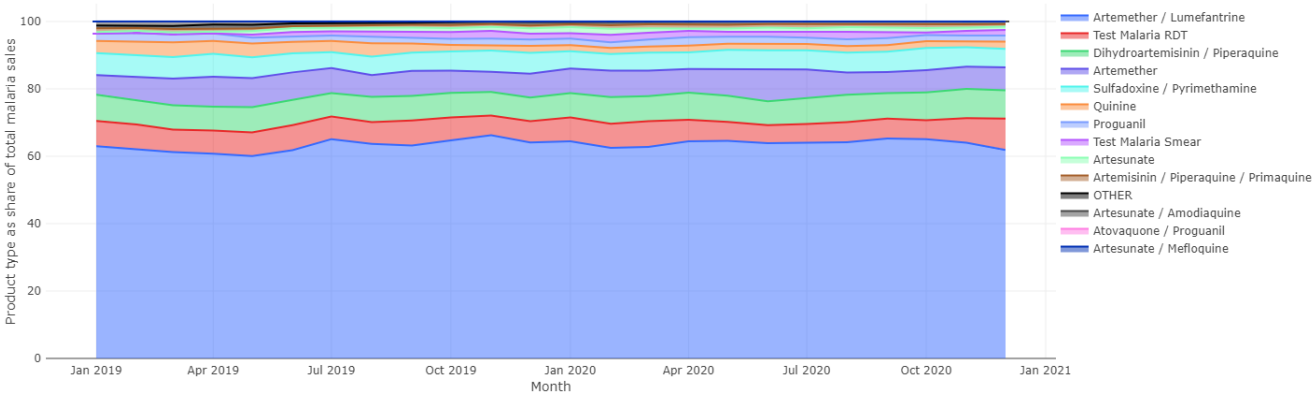

Figure 1: Antimalarial sales by product type (Back: 2.1)

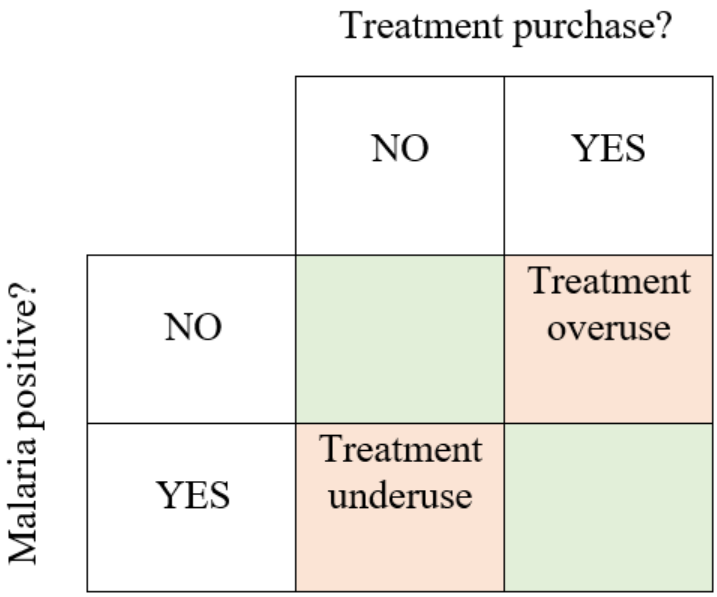

Figure 2: Types of errors (Back: 3)

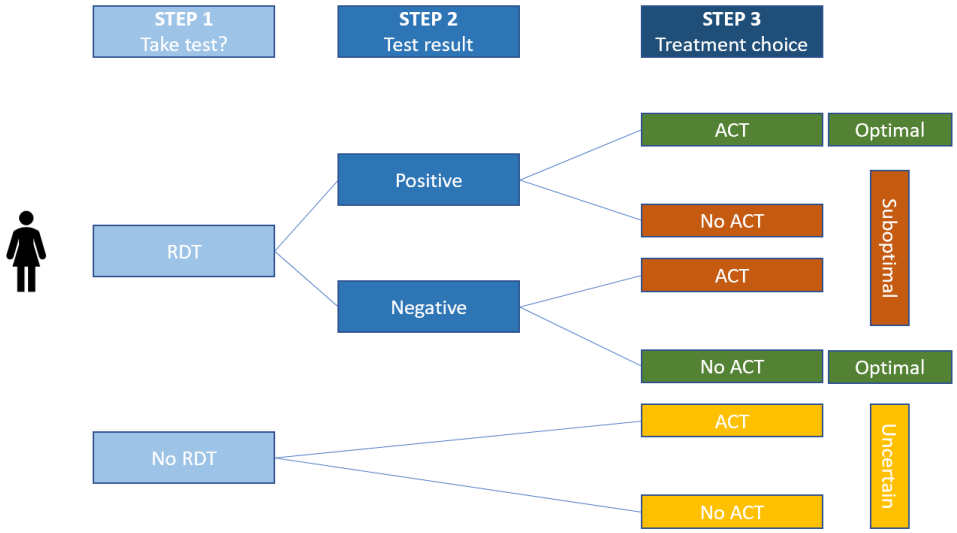

Figure 3: Decision to diagnose fever with an RDT prior to treatment (Back: 3.1)

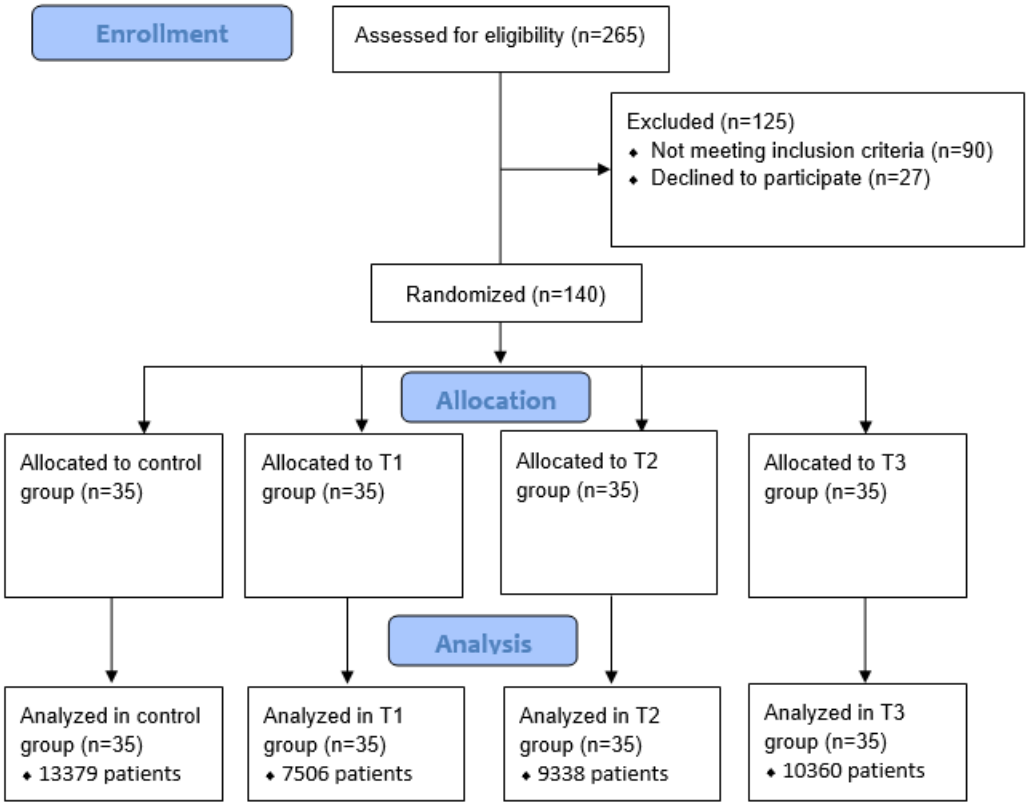

Figure 4: Study flow diagram (Back: 4)

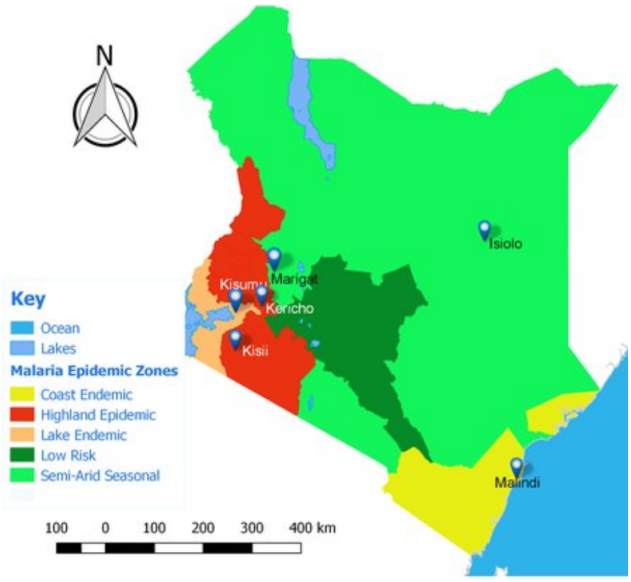

Figure 5: Malaria zones in Kenya (Back: 4.2)

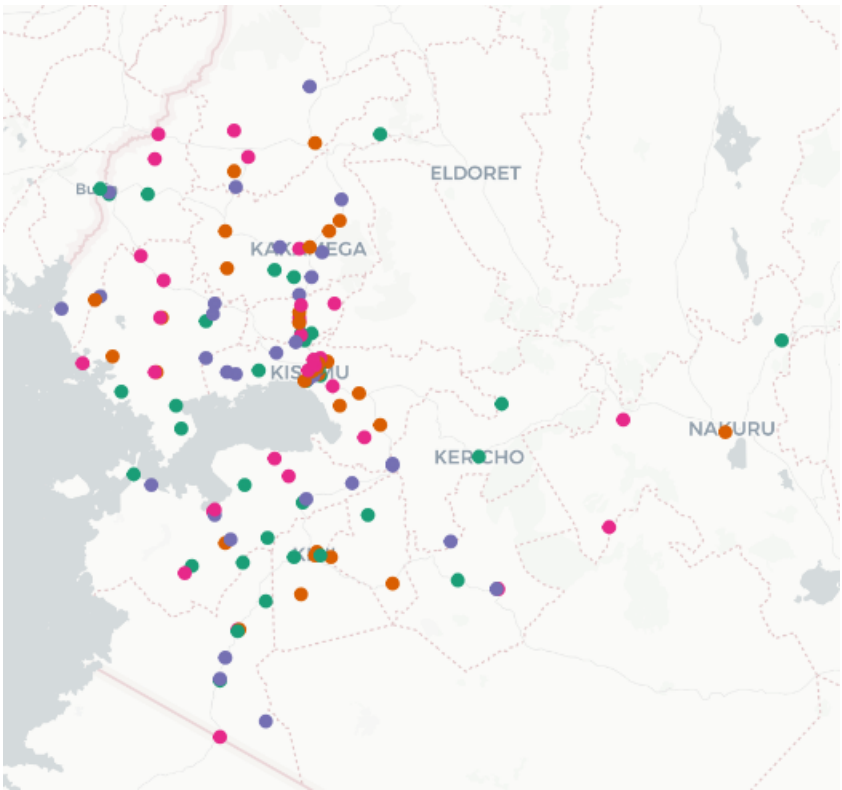

Figure 6: Map of study sites (Back: 4.2)

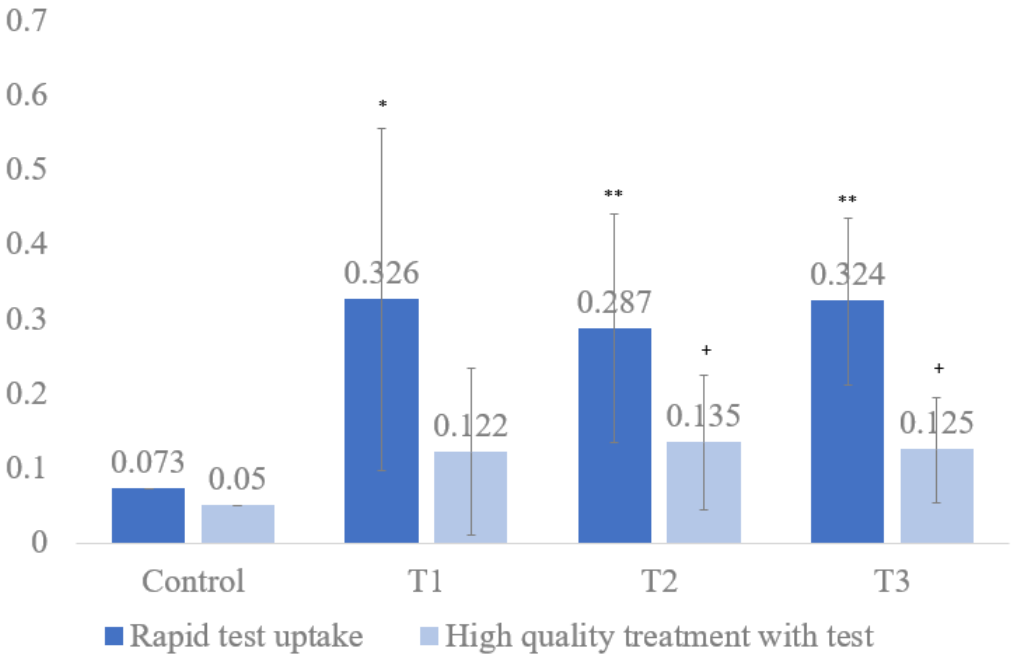

Figure 7: Proportions of primary outcomes by intervention arm, unadjusted regression model (Back: 6.2)

11 Tables

Table 1: Baseline Balance Between Treatment Arms (Back: 6.1)

	(5) Overall	(1) C	(2) T1	(3) T2	(4) T3	(6) T1 vs. T2	(7) T1 vs. T3	(8) T2 vs. T3
Number of months active on digital sales management tool	8.171	10.143	9.057	5.371**	8.114	0.083	0.686	0.21
	(0.81)	(1.77)	(1.59)	(1.36)	(1.69)			
Below median baseline malaria product sales	0.41	0.37	0.46	0.37	0.46	0.474	1	0.474
	(0.04)	(0.08)	(0.09)	(0.08)	(0.09)			
Average monthly malaria product sales, 2019-2020	54.38	65.291	55.009	53.598	43.622*	0.912	0.321	0.374
	(4.45)	(10.41)	(9.14)	(8.85)	(6.79)			
Average monthly high quality treatment sales, 2019-2020	46.675	53.025	46.973	48.054	38.648	0.928	0.441	0.38
	(3.79)	(6.71)	(8.53)	(8.41)	(6.53)			
Average monthly rapid test sales, 2019-2020	5.128	4.304	7.847	4.832	3.53	0.265	0.049	0.488
	(0.73)	(0.90)	(2.04)	(1.74)	(0.69)			
Site was in earlier pilot study phase	0.157	0.171	0.2	0.171	0.086	0.763	0.177	0.291
	(0.03)	(0.07)	(0.07)	(0.07)	(0.05)			
Site is in an urban area	0.307	0.2	0.286	0.4*	0.343	0.321	0.613	0.627
	(0.04)	(0.07)	(0.08)	(0.08)	(0.08)			
Site is in a malaria endemic county	0.843	0.886	0.771	0.8	0.914	0.775	0.103	0.177
	(0.03)	(0.06)	(0.07)	(0.07)	(0.05)			
Site is a pharmacy	0.556	0.444	0.684	0.5	0.571	0.321	0.521	0.729
	(0.06)	(0.12)	(0.11)	(0.15)	(0.14)			
Percentage of pharmacy staff who are female	0.431	0.441	0.348	0.5	0.435	0.131	0.405	0.531
	(0.04)	(0.07)	(0.07)	(0.07)	(0.08)			
Age of pharmacy owner	36.808	37.931	35.481	36.96	36.778	0.541	0.667	0.952
	(0.84)	(1.06)	(1.70)	(1.69)	(2.64)			
Average age of pharmacy staff	30.08	30.4	30.182	29.625	30.182	0.757	1	0.776
	(0.66)	(1.55)	(1.09)	(1.40)	(1.34)			
Female pharmacy owner	0.284	0.417	0.196*	0.24	0.263	0.704	0.581	0.86
	(0.04)	(0.09)	(0.07)	(0.09)	(0.10)			
Number of pharmacy staff	1.493	1.543	1.543	1.486	1.4	0.656	0.263	0.478
	(0.04)	(0.09)	(0.10)	(0.09)	(0.08)			
Number of sites	140	35	35	35	35			

Significance stars (< 0.1, ** < 0.05, *** < 0.01) indicate significant differences between intervention and control groups (Cols 1-4). P-values from comparisons between intervention arms are reported (Cols 5-7). Pharmacies in T2 were newer to the pharmacy network compared to T1, and pharmacies in T1 sold more rapid tests at baseline compared to T3. Otherwise, all other pairwise comparisons are not statistically significant at the 10% level or below.*

Table 2: Impact on Primary Outcomes, Unadjusted Models (Back: 6.2)

	Rapid test uptake		ACT uptake with test		ACT uptake without test	
	(1)	(2)	(3)	(4)	(5)	(6)
Pooled interventions	.279**		.0913*		-.186*	
	(0.062)		(0.041)		(0.082)	
T1		.253*		.0715		-.188
		(0.117)		(0.057)		(0.128)
T2		.214**		.0846+		-.153
		(0.078)		(0.046)		(0.096)
T3		.251**		.0747*		-.191*
		(0.057)		(0.036)		(0.084)
Control mean	0.073	0.073	0.051	0.051	0.831	0.831
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2} \neq \gamma_{T3}$)		0.908		0.975		0.915
N	40538	40538	40583	40583	40169	40169

Reporting marginal effects from the control group, from logistic regressions

Standard errors are clustered at the facility level

Controls: Strata FE

Wald test comparisons of difference in marginal effects (γ) between treatment arms

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table 3: Impact on Primary Outcomes, Adjusted Models (Back: 6.2)

	Rapid test uptake		ACT uptake with test		ACT uptake without test	
	(1)	(2)	(3)	(4)	(5)	(6)
Pooled interventions	.277**		.0951*		-.227**	
	(0.054)		(0.039)		(0.064)	
T1		.264*		.07		-.22+
		(0.103)		(0.050)		(0.114)
T2		.234**		.103*		-.207*
		(0.073)		(0.051)		(0.082)
T3		.238**		.0702*		-.217**
		(0.054)		(0.029)		(0.069)
Control mean	0.073	0.073	0.051	0.051	0.831	0.831
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2} \neq \gamma_{T3}$)		0.969		0.802		0.992
N	40538	40538	40583	40583	40169	40169

Reporting marginal effects from the control group, from logistic regressions
Standard errors are clustered at the facility level
Controls: months active on platform, baseline malaria sales, female owner, strata FE
Wald test comparisons of difference in marginal effects (γ) between treatment arms
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table 4: Impact on Secondary Outcomes, Unadjusted Models (Back: 6.2)

	Antimalarial sales overall		ACT uptake overall		ACT uptake with test, ACT sales	
	(1)	(2)	(3)	(4)	(5)	(6)
Pooled interventions	-.0111		-.118		.142**	
	(0.027)		(0.076)		(0.055)	
T1		.0267		-.125		.104
		(0.031)		(0.086)		(0.085)
T2		-.00726		-.0743		.117+
		(0.033)		(0.073)		(0.064)
T3		-.0303		-.126+		.131*
		(0.032)		(0.069)		(0.051)
Control mean	0.193	0.193	0.881	0.881	0.057	0.057
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2} \neq \gamma_{T3}$)		0.212		0.218		0.964
N	206279	206279	40583	40583	32135	32135

Reporting marginal effects from the control group, from logistic regressions

Standard errors are clustered at the facility level

Controls: Strata FE

Wald test comparisons of difference in marginal effects (γ) between treatment arms

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table 5: Impact on Secondary Outcomes, Adjusted Models (Back: 6.2)

	Antimalarial sales overall		ACT uptake overall		ACT uptake with test, ACT sales	
	(1)	(2)	(3)	(4)	(5)	(6)
Pooled interventions	-.00334		-.155**		.152**	
	(0.021)		(0.049)		(0.050)	
T1		.0154		-.156*		.116
		(0.023)		(0.074)		(0.090)
T2		-.00635		-.11*		.143*
		(0.032)		(0.050)		(0.070)
T3		-.014		-.151**		.131**
		(0.027)		(0.049)		(0.045)
Control mean	0.193	0.193	0.881	0.881	0.057	0.057
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2} \neq \gamma_{T3}$)		0.529		0.434		0.969
N	206279	206279	40583	40583	32135	32135

Reporting marginal effects from the control group, from logistic regressions
Standard errors are clustered at the facility level
Controls: months active on platform, baseline malaria sales, female owner, strata FE
Wald test comparisons of difference in marginal effects (γ) between treatment arms
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table 6: CEA Benefit Components (Back: 8.2)

Benefit component in CEA framework	*Method of measuring parameter using experimental set up*
$P(a_1)^{m_1 t_1}$& $P(a_1)^{m_1 t_0}$	Treatment uptake for malaria positive patients with an accompanying test (or not): measured using administrative data and malaria test positivity rate, measured in random sample of control group patients
$P(m_1)^{t_1}$ & $P(m_1)^{t_0}$	Malaria positivity rate for individuals who get tested (or not), measured in the control group using administrative and patient survey data, and test positivity measured in random sample of control group patients
$P(t_1)$ & $P(t_0)$	Rapid test uptake in the control group, measured using administrative data
$P^{'}_{c_t}(a_1)^{m_1 t_1}$	Change in treatment uptake for malaria positive patients with an accompanying test: measured from ITT estimates (which randomly vary price) and malaria test positivity rate from the intervention arms using administrative data
$P^{'}_{c_t}(a_1)^{m_1 t_0}$	Change in treatment uptake for malaria negative patients without an accompanying test: measured from ITT estimates (which randomly vary price), and malaria positivity rates from the control group and the tested sample in intervention groups to recover estimated malaria probability in the untested group for each intervention arm
$P^{'}_{c_t}(m_1)^{t_1}$	Change in malaria positivity with a rapid test, obtained from ITT estimates (which randomly vary price)
$P^{'}_{c_t}(m_1)^{t_0}$	Change in malaria positivity in untested sample, estimated using malaria positivity rates from the control group and the tested sample in intervention groups
$P^{'}_{c_t}(t_1)$ & $P^{'}_{c_t}(t_0)$	Change in rapid test uptake measured from the treatment effect estimates (which randomly vary price) in the intervention arms

Table 7: CEA Cost Components (Back: 8.3)

Cost component in CEA framework	*Method of measuring parameter using experimental set up*
$incentive_t(patients_t)$	Total cost of the incentives, measured by multiplying the arm-specific incentive amount by the number of patients who purchased the incentivized product (rapid test or high quality treatment).
$c_a P(m_0)(untested_t)$	Total cost of treating untested malaria-negative patients, measured by multiplying the average treatment cost by the number of untested patients and the probability of being malaria-negative (inverse of the malaria test positivity rate measured in the control group).
$c_a(negativetx_t)$	Total cost of treating confirmed malaria-negative patients with antimalarials, measured by multiplying the average treatment cost by the number of tested, malaria-negative individuals. Obtained from the administrative data.
$timecost_t(patients_t)$	Total time cost of seeking malaria treatment, measured by multiplying the arm-specific average time cost to patients measured in the patient exit surveys by the total number of patients who sought care during the study period and local average wages.

12 Technical Appendix

12.1 Patient decision: Private costs only

The patient's decision to buy high quality treatment and rapid test can be modeled as a two-stage discrete choice experiment. I solve for stage 2, and then recursively for stage 1:

Stage 2: Decision to buy high quality treatment. There are two versions of the patient decision at this stage: (1) the decision of whether or not to buy treatment given some true information about the patient's illness status (from test result), and (2) the decision of whether or not to buy treatment given the patient's own beliefs about the illness status, but no improved information from testing.

In (1) a patient's utility in stage 2 (U_2) is:

$$U_{2,1}(z_i) = \left\{ \begin{array}{ll} B(z_i) - p, & \text{for } z_i = 1 \\ 0, & \text{for } z_i = 0 \end{array} \right\}$$

where she has full information about her illness status and therefore only purchases high quality treatment if she is positive ($z_i = 1$). $B(z_i)$ is the benefit of taking treatment when sick and p is the price (cost) of the medication. In this simplified model, the negative social costs of over use of antimalarials is not incorporated into private decision-making, making the only cost the price of the drug.

In (2), a patient's utility in stage 2 is:

$$U_{2,2}(z_i) = \left\{ \begin{array}{ll} (z_i) - p, & \text{for } z_i = 1 \\ -p, & \text{for } z_i = 0 \end{array} \right\}$$

where in this case, the utility of taking Artefan when positive is the same as in the first scenario, but the utility when malaria-negative is different. Here, the benefit of taking malaria medication is only realized when the patient truly has malaria (when $z_i = 1$). Because there is uncertainty in illness status, the patient will purchase the medication even when she is malaria negative ($z_i = 0$) if she is sufficiently confident in her ability to diagnose malaria. Therefore, the patient will incur the cost (p) every time, but the benefit only when malaria-positive.

In each scenario of the second stage, the expected utility is as follows, over the true probability of being malaria positive, denoted by π_i:

$$E[U_{2,1}(z_i)] = \pi_i(B(z_i) - p) + (1 - \pi_i)0 = \pi_i(B(z_i) - p) \tag{11}$$

$$E[U_{2,2}(z_i)] = \pi_i(B(z_i) - p) + (1 - \pi_i)(-p) = \pi_i B(z_i) - p \tag{12}$$

Stage 1: Patient's decision to get tested. The patient's decision to purchase a rapid test is driven by her perception of its value. The value of the rapid test is the value of the information obtained from a diagnosis. So, individuals will purchase the rapid test if the expected utility in Stage 2 with the addition of that information (minus the cost of the test) is greater than the expected utility in Stage 2 without that information.

Utility in Stage 1 (U_1) can be broken down into utility from purchasing the rapid test or not (denoted here as r):

$$U_1(\pi_i|r = 1) = E[U_{2,1}(z_i)] - p_r \tag{13}$$

$$U_1(\pi_i|r = 0) = E[U_{2,2}(z_i)] \tag{14}$$

Patient i will buy the rapid test if $U_1(\pi_i|r = 1) \geq U_1(\pi_i|r = 0)$. We now solve for the marginal case:

$$\begin{aligned} U_1(\pi_i|r = 1) - U_1(\pi_i|r = 0) &= 0 \\ E[U_{2,1}(z_i)] - p_r - E[U_{2,2}(z_i)] &= 0 \\ \pi_i(B(z_i) - p) - p_r - \pi_i B(z_i) + p &= 0 \\ (1 - \pi_i)p - p_r &= 0 \end{aligned}$$

Rearranging, we arrive at our desired equilibrium condition:

$$(1 - \pi_i) = \frac{p_r}{p} \tag{15}$$

On the right hand side of this final result, we have the probability of not having malaria (malaria negative). This is the individual's risk in being malaria positive. On the left hand side, we have the ratio between the cost of the rapid test and the cost of the treatment. This suggests that for patients, the value of the rapid test is greater when the price of Antimalarials is high (relative to test price) and there is uncertainty about one's malaria status.

Implications of this result. There are a few implications of this threshold equilibrium condition by looking at the price ratio:

1. The share of patients who will choose to purchase rapid tests at a given price is decreasing in the malaria prevalence rate (test is less valuable for high π)
2. As the ratio of prices increases (relative price of treatment is smaller than test price), the share of patients purchasing a rapid test will be smaller
3. As the price ratio decreases (relative price of treatment is larger than test price), the share of patients purchasing a rapid test will be larger

It is important to note that this exercise has assumed that the only relevant cost is the price of the rapid test and the treatment option. This simplification excludes two important costs: the first is the time cost of getting a rapid test. Tests take about 15 minutes from when they are administered to when results are ready, so this is an additional non-pecuniary cost that patients may face when making this decision. The second cost is the social cost of not getting the treatment most appropriate to the illness status (i.e. not getting malaria medication in the case of a negative malaria status). This social cost is not factored into private decision-making, but should be considered when setting prices for these products.

(Back: 3.1)

13 Appendix

13.1 Appendix Figures

13.2 Appendix Tables

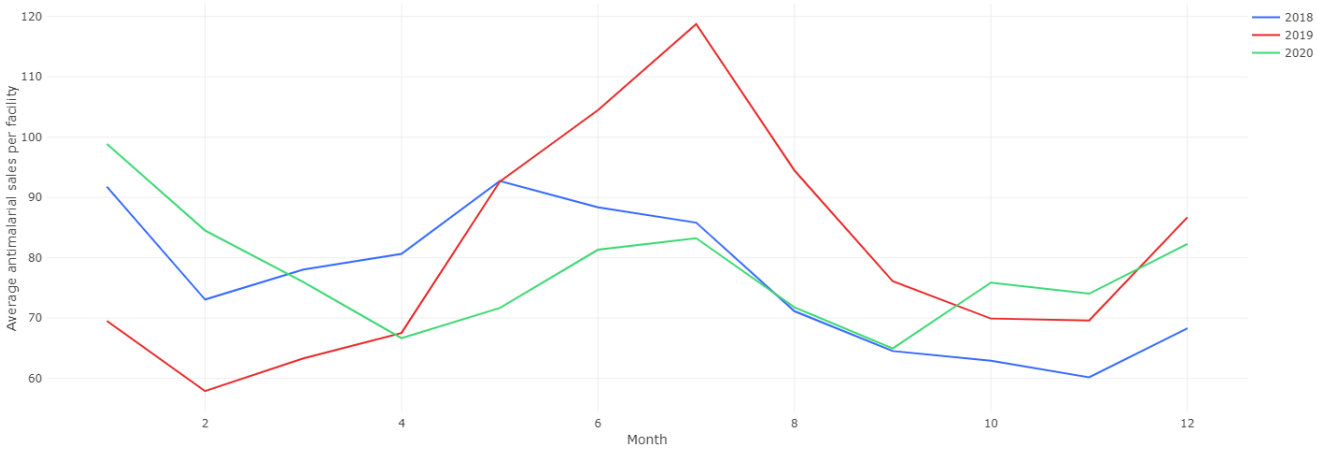

Figure 8: Malaria sales, seasonal trends (Back: 2.1)

Table 8: Incentive amount details, by treatment arm (Back: 4.1)

Subsidy and incentive amounts

	Control (C)	Patient discount (T1)	Pharmacy incentive (T2)	Both (T3)
Patient discounts? (USD)				
Rapid test	-	$0.90	-	$0.60
ACT (malaria +)	-	$1.10	-	$0.80
ACT (malaria -)	-	$0.00	-	$0.00
Provider incentives (USD)				
Rapid test	-	-	$0.90	$0.20
ACT (malaria +)	-	-	$0.80	$0.10
ACT (malaria -)	-	-	$0.00	$0.00
Transaction completion	-	-	$0.30	$0.30
Total incentive amount (USD)	$0.00	$2.00	$2.00	$2.00

Table 9: Baseline Balance between Facilities in Sample and Refusals (Back: 6.1)

Variable	(1) In sample	(2) Declined	(3) (2)-(1)
Number of months active on digital sales management tool	12.04	16.81	4.76**
	(9.43)	(8.52)	(0.01)
Average monthly malaria sales, 2019-2020	63.81	66.60	2.79
	(64.66)	(75.81)	(0.85)
Average monthly quality treatment sales, 2019-2020	54.70	61.48	6.78
	(55.05)	(73.28)	(0.59)
Average monthly rapid test sales, 2019-2020	6.68	5.43	-1.25
	(10.66)	(11.36)	(0.59)
Site was in earlier pilot study phase	0.16	0.23	0.07
	(0.37)	(0.43)	(0.32)
Site is in an urban area	0.31	0.34	0.04
	(0.46)	(0.48)	(0.69)
Site is in a malaria endemic county	0.84	0.86	0.01
	(0.37)	(0.36)	(0.84)
Site is a pharmacy	0.56	0.56	-0.00
	(0.50)	(0.51)	(1.00)
Observations	140	35	175

Notes: In sample facilities include those that have been onboarded and those that are pending.

Table 10: Primary/secondary outcomes regressed on baseline characteristics (Back: 6.1)

	(1) Rapid test uptake	(2) ACT uptake with test	(3) ACT uptake without test	(4) Antimalarial sales overall	(5) ACT uptake overall	(6) ACT uptake with test, ACT sales
Number of months active on digital sales management tool	.00206	.00228^{+}	-.00188	.0000982	.000565	.00296^{+}
	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Below median baseline malaria sales	.203**	.0366	-.169**	-.00709	-.137**	.109*
	(0.065)	(0.035)	(0.063)	(0.031)	(0.049)	(0.054)
Average monthly malaria sales, 2019-2020	-.00064	-.000674^{+}	-.00485*	-.00217**	-.00547**	-.000237
	(0.001)	(0.000)	(0.002)	(0.001)	(0.002)	(0.001)
Average monthly quality treatment sales, 2019-2020	-.00155^{+}	-.000459	.00712**	.00311**	.0066**	-.0016^{+}
	(0.001)	(0.000)	(0.002)	(0.001)	(0.002)	(0.001)
Average monthly rapid test sales, 2019-2020	.0142**	.0087**	-.0107**	.0021	-.00209	.0127**
	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)
Site was in earlier pilot study phase	-.0298	-.00861	.0482	-.00321	.0413	-.00971
	(0.049)	(0.037)	(0.055)	(0.038)	(0.048)	(0.048)
Site is in an urban area	.0164	.00684	.0248	-.0379^{+}	.0248	.00311
	(0.052)	(0.026)	(0.051)	(0.022)	(0.041)	(0.040)
Site is in a malaria endemic county	.069	.0623*	-.108	.109**	-.0401	.102*
	(0.077)	(0.026)	(0.077)	(0.027)	(0.068)	(0.050)
Site is a pharmacy	.668**	.241**	-.658**	.209**	-.41**	.733**
	(0.051)	(0.064)	(0.047)	(0.075)	(0.080)	(0.048)
Percentage of pharmacy staff who are female	.136*	.0564	-.144*	-.027	-.0933*	.107^{+}
	(0.069)	(0.038)	(0.065)	(0.023)	(0.045)	(0.057)
Age of pharmacy owner	.00686*	.00165	-.00753*	.00242*	-.00586*	.00615*
	(0.003)	(0.001)	(0.003)	(0.001)	(0.003)	(0.003)
Average age of pharmacy staff	.00223	.00139	-.00279	-.00187	-.00155	.00121
	(0.005)	(0.003)	(0.005)	(0.002)	(0.004)	(0.004)
Female pharmacy owner	-.204**	-.0878*	.188**	.0973**	.103**	-.156**
	(0.062)	(0.036)	(0.060)	(0.035)	(0.039)	(0.054)
Number of staff	.0851	.0567*	-.102^{+}	.0339	-.0452	.0599
	(0.056)	(0.028)	(0.055)	(0.022)	(0.040)	(0.044)
Constant	-.301	-.157^{+}	1.36**	-.0426	1.2**	-.324*
	(0.206)	(0.085)	(0.208)	(0.079)	(0.156)	(0.159)
N	40583	40583	40214	206279	40583	32140

Reporting regression results from linear probability models for primary/secondary outcomes on baseline characteristics
Standard errors are clustered at the facility level
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table 11: Impact on Primary Outcomes, Linear Probability Models (Back: 6.2)

	Rapid test uptake		ACT uptake with test		ACT uptake without test	
	(1)	(2)	(3)	(4)	(5)	(6)
T1	.241^{+}	.265*	.0705	.0728	-.186	-.229^{+}
	(.122)	(.107)	(.062)	(.056)	(.131)	(.118)
T2	.204*	.248**	.084^{+}	.106^{+}	-.151	-.217*
	(.088)	(.091)	(.050)	(.058)	(.102)	(.093)
T3	.23**	.246**	.0697^{+}	.0741*	-.182*	-.215**
	(.058)	(.062)	(.036)	(.035)	(.080)	(.072)
Control mean	.073	.073	.051	.051	.831	.831
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2} \neq \gamma_{T3}$)	0.952	0.987	0.966	0.827	0.943	0.993
N	40583	40583	40583	40583	40214	40214

Both models include strata fixed effects

Standard errors are clustered at the facility level

Controls in columns 2, 4, 6 are: months active on sales platform, female owner, baseline malaria sales

Wald test comparison of difference in marginal effects (γ) between treatment arms

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table 12: Impact on Secondary Outcomes, Linear Probability Models (Back: 6.2)

	Antimalarial sales overall		ACT uptake overall		ACT uptake with test, ACT sales	
	(1)	(2)	(3)	(4)	(5)	(6)
1.tx$_a rm$	.0257	.0175	-.12	-.16*	.103	.129
	(.033)	(.023)	(.084)	(.071)	(.094)	(.094)
2.tx$_a rm$	-.00801	-.00842	-.0702	-.112*	.117	.159^{+}
	(.034)	(.032)	(.073)	(.054)	(.073)	(.082)
3.tx$_a rm$	-.0304	-.0117	-.121^{+}	-.148**	.122*	.146**
	(.033)	(.026)	(.066)	(.054)	(.051)	(.050)
Control mean	.193	.193	.881	.881	.057	.057
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2} \neq \gamma_{T3}$)	0.222	0.388	0.671	0.719	0.985	0.958
N	206279	206279	40583	40583	32140	32140

Both models include strata fixed effects

Standard errors are clustered at the facility level

Controls in columns 2, 4, 6: months active on sales platform, female owner, baseline malaria sales

Wald test comparison of difference in marginal effects (γ) between treatments

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$