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

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Link to most updated version.

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Abstract

A key aim of health policy is to ensure that patients are able to access high quality care when appropriate while minimizing the over-use of unnecessary treatments. Achieving this balance requires aligned decision-making between patients and providers, which can be hindered if incentives are incompatible with this aim. 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. I find that both patient subsidies and pharmacy incentives for diagnostic testing significantly increase usage of testing and may encourage malaria positive individuals to purchase high quality antimalarials. Patient subsidies increase the likelihood that a symptomatic patient takes a rapid test by 25 percentage points, from a control group mean of 8 percent. Provider incentives and the hybrid approach increase the likelihood of rapid test uptake by 20 and 25 percentage points, respectively, which is indistinguishable from the demand-side approach. I also find that all arms improve ACT targeting by more than 200%. I find that both demand- and supply-side incentives reduce the likelihood that a patient purchases malaria treatment without a confirmatory diagnosis by between 17-19 percentage points, compared to 81 percent in the control group. Taken together, these results suggest that appropriately calibrated and targeted financial incentives are promising for changing patient and provider behavior, with implications for quality of care. Additionally, the fact that supply- and demand-side incentives lead to comparable effects on diagnostic testing and treatment targeting suggests that both patients and pharmacists are effective channels for incentive targeting.

Introduction

A key aim of health policy is to ensure that patients are able to access high quality care when appropriate and prevent patients from consuming unnecessary treatments. Achieving this balance requires incentive alignment 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 [1, 2]. For example, studies in both the US and China have found high levels of unnecessary antibiotic prescriptions [3, 4], which affects patient outcomes and contributes to growing rates of drug resistance. Studies across sub-Saharan Africa have found that large shares of malaria-negative patients receive antimalarials at health centers and pharmacies [5–8], which contributes to resistance and limits medication supply for those who may need it most.

Malaria is an important clinical area to study how decisions made by patients and providers impact 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 [9]. 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. There are several reasons why diagnostic testing is low: relatively high cost of tests, affordable and accessible antimalarials, and established practices of diagnosis based on symptoms alone are a few major contributors. Low diagnostic testing 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 [10], this may be due to misaligned incentives between patients and pharmacists. Studies in Kenya have found that patients may not demand tests because of strong prior beliefs about their malaria status [11], the cost of the test is prohibitive [6, 12], or they do not want to wait for the diagnostic test result. Pharmacists may skip testing because they are optimizing perceived patient preferences and their own profit motivations [3, 13].

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), or both. While classical economic theory would suggest that demand- and supply-side incentives should result in the same impact on demand, they may differ if incentives are captured by the pharmacy and not passed through to patients, or if pharmacists encourage increased demand through non-price mechanisms such as improved counseling around the importance of diagnostic testing. I explore these hypotheses in this paper. Pharmacies were randomized to a status quo control group or one of three treatment groups, each with a two-part incentive: (1) patient subsidies for rapid tests and for artemisinin combination therapies (ACTs) conditional on a positive test; (2) pharmacy incentives for selling rapid tests to diagnose fevers and for prescribing ACTs conditional on a positive test; and (3) hybrid incentives (patient subsidies and pharmacy incentives) for rapid test use and ACTs 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 [6, 7, 12]. 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 [14, 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 [10, 16]. 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.

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 27 percentage points, from a control group mean of 8 percent. Pharmacy incentives increase the likelihood of rapid test uptake by 20 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 and the hybrid incentives increase the likelihood that a patient purchases ACTs with a diagnostic test by 7 percentage points compared to a control group mean of 6 percent, with directional evidence that demand-side incentives alone 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 17-19 percentage points, compared to 81 percent in the control group. I find no evidence of intervention effects on the share of malaria patients seeking care at study pharmacies, suggesting that these findings are driven by behavior change rather than patient sorting. When testing whether incentives were passed through to patients, I find suggestive evidence that providers in the pharmacy incentive group did not pass through incentives to patients when compared to the patient subsidy group. This means that incentive pass-through is unlikely to be a key mechanism that explains why demand- and supply-side incentives yield similar effects on uptake of tests and treatment targeting, and instead, other mechanisms such as improved provider counseling to encourage testing, or other forms of supplier-induced demand, may explain this finding.

I complement the analysis on testing and treatment decisions with a cost-effectiveness analysis, which provides a more complete picture of how to efficiently allocate resources. When only considering implementer costs, I find that patient subsidies cost \$3.26 for each additional patient that purchases an ACT and is malaria positive, and pharmacy incentives cost only \$4.01 for each additional patient that purchases an ACT and is malaria positive, when compared to the status quo standard of care. The hybrid approach (combined patient subsidies and pharmacy incentives) costs \$9.15 for each additional patient that purchases an ACT and is malaria positive, when compared to the status quo control group. When also considering the direct (medication) costs of over-treating malaria negative patients and the time costs for patient seeking care, I find that the patient subsidy and the provider performance incentive interventions are cost-saving, relative to the control group. The hybrid approach costs \$24 for each additional patient that purchases an ACT and is malaria positive, compared to the status quo control group. This suggests that all interventions are relatively low cost when compared to the status quo, and that patient subsidies and pharmacy incentives may be cost-saving depending on the perspective taken.

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 non-pecuniary 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 [6, 7, 17–19]. 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 [3, 13]. Provider performance incentives have focused on improving quality of care, particularly for maternal and child health outcomes [20, 21]. 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. Information combined with financial incentives has shown more promise in encouraging health behavior adoption [22–24]. But, 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 [25]. Studies that have examined whether 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 [6, 12]. This study leverages the two steps of the testing and treatment decision by providing a financial incentive for treatment conditional on the personalized health information provided by the test.

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 of financial incentives for malaria care in Kenya have used vouchers that patients could redeem at pharmacies but have not studied the pharmacist's role in malaria case management directly. To my knowledge, there is one other ongoing study which tests pharmacy incentives for malaria testing and treatment [26].

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 an implementer and societal perspective. 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 1 provides an overview of the setting, including the malaria context in Kenya. Section 2 discusses the theoretical framework and hypotheses for the main research questions and outcomes. Section 3 describes the experimental design and methods for the impact evaluation and cost-effectiveness analysis. Section 4 presents experimental results on the main outcomes and effects, including a discussion of results on incentive targeting on the demand and supply side, and cost-effectiveness. Section 5 concludes.

1 Background

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 provides institutional context for the pharmacy network used in this study. Finally, it provides an overview of how this experiment fits in with the existing literature on demand- and supply-side incentives for health behavior change, with a focus on incentives for malaria case management.

1.1 Malaria in Kenya

There are an estimated 190 million malaria cases and 391,500 deaths in sub-Saharan Africa per year, with the poor bearing the brunt of this health and economic burden [27–29]. 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 [14].[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 [28]. This decline has coincided with targeted global efforts to increase the use of ACTs 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 [30], but many patients seek care in the private sector to avoid common barriers such as long wait times, consultation fees, and unreliable medication supply [31]. 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 [6]. Availability of malaria treatment in the private sector is largely due to a subsidy program run by the Affordable Medicines Facility-Malaria (AMFm) between 2011-2016, which provided manufacturer incentives to lower prices of ACTs for patients to less than $1 USD per treatment course. In 2010, the AMFm initiative enacted a 95 percent ACT subsidy to pharmaceutical wholesalers in seven countries, including Kenya, to increase the availability and reduce price in the private sector. This approach was extensively evaluated, and in 2012 was modified to give countries more choice for how to use these funds. 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 [32].

[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.

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 [6]. This is consistent with recent sales trends in Kenyan pharmacy data: malaria tests account for only 10% of antimalarial product sales.[2]

1.2 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 [9, 35]. After diagnosis, the recommended treatment for uncomplicated malaria is a three-day course of artemisinin combination therapy (ACT) for children and adults.[4]

1.3 Subsidies for malaria case management in Kenya

Over the past decade, there have been many efforts to increase 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 (ACTs) 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 [36, 37].

There are two seminal studies in the last decade that evalued the impact of subsidies on testing and treatment decisions in Kenya. A large randomized controlled trial led by Cohen, Dupas and Schaner experimentally varied subsidy levels for ACTs and cross-randomized a rapid test subsidy in Kenya's malaria endemic areas. This study evaluated the extent of the trade-off between access to high quality antimalarials (ACTs) and appropriate treatment targeting at different subsidy

[2] Despite low levels of testing, pharmacy staff are knowledgeable about appropriate case management: 78% of providers who responded to a clinical vignette about appropriate case management at baseline (N=233) said they would first have the patient who presented with malaria-like symptoms obtain a diagnostic test prior to prescribing treatment.

[3] Rapid diagnostic tests have a sensitivity of 78% and a specificity of 94%, compared to microscopy which is 57% sensitive and 99% specific [33, 34]. 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.

levels, and whether introducing subsidized rapid tests improved treatment targeting [6]. 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 by Prudhomme O'Meara and colleagues 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 [12]. Authors of this study found that the combined subsidy increases diagnostic testing and improves treatment targeting.[5]

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.

[5]At the time of writing this draft, another research group from Duke University, Moi University, and the Clinton Health Access Initiative were conducting a linked field experiment in Nigeria and Kenya comparing pharmacy incentives and patient subsidies for malaria testing and treatment [26].

2 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 collaboration 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 ACTs (Figure 1). 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.[6] Second, over-prescribing antimalarials can lead to a rise in drug-resistant strains of malaria. This is a well-founded public health concern,[7] 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 ACTs. 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,[8] and then aggregate the individual testing and treatment decisions to the population level and discuss how these parameters are measured in the experimental design.

2.1 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 2.[9] The starting point for this

[6] 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.

[7] Chloroquine-resistant P. falciparum has spread to nearly all areas of the world where falciparum malaria is transmitted, making this drug ineffective.[28]

[8] This conceptual framework and model build from models developed in Lopez et al 2020 [13] and Cohen, Dupas and Schaner [6].

[9] 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

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, which is represented by Step 1 in the figure. 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.

2.2 Treatment targeting in the aggregate

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

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

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

where for $i \in \{0, 1\}$: a_i is whether the patient takes an ACT, 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, $Pr(a_1)^{m_1}$is the probability that an individual takes an ACT conditional on being malaria positive. Additionally, $Pr(t_0) = 1 - Pr(t_1)$. In order to measure the probability that an individual takes an ACT 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 ACTs and the share of malaria negative individuals who do not obtain antimalarials — the two dimensions of treatment targeting.

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.

I expect the interventions to increase rapid test uptake and appropriate ACT targeting through three mechanisms: (1) subsidized testing and treatment conditional on confirmed positivity, (2) increased incentive for pharmacists to promote testing and ACTs if positive, 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 ACTs 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 ACTs. Finally, increased supply of both rapid tests and ACTs 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]:

$$
\begin{aligned}
P^{'}_{c_t}(a_1)^{m_1} = {} & P^{'}_{c_t}(a_1)^{m_1t_1}Pr(m_1)^{t_1}Pr(t_1) + P^{'}_{c_t}(m_1)^{t_1}Pr(a_1)^{m_1t_1}Pr(t_1) + \\
& P^{'}_{c_t}(t_1)Pr(a_1)^{m_1t_1}Pr(m_1)^{t_1} + P^{'}_{c_t}(a_1)^{m_1t_0}Pr(m_1)^{t_0}Pr(t_0) + \\
& P^{'}_{c_t}(m_1)^{t_0}Pr(a_1)^{m_1t_0}Pr(t_0) + P^{'}_{c_t}(t_0)Pr(a_1)^{m_1t_0}Pr(m_1)^{t_0}
\end{aligned} \tag{3}
$$

$$
\begin{aligned}
P^{'}_{c_t}(a_1)^{m_0} = {} & P^{'}_{c_t}(a_1)^{m_0t_1}Pr(m_0)^{t_1}Pr(t_1) + P^{'}_{c_t}(m_0)^{t_1}Pr(a_1)^{m_0t_1}Pr(t_1) + \\
& P^{'}_{c_t}(t_1)Pr(a_1)^{m_0t_1}Pr(m_0)^{t_1} + P^{'}_{c_t}(a_1)^{m_0t_0}Pr(m_0)^{t_0}Pr(t_0) + \\
& P^{'}_{c_t}(m_0)^{t_0}Pr(a_1)^{m_0t_0}Pr(t_0) + P^{'}_{c_t}(t_0)Pr(a_1)^{m_0t_0}Pr(m_0)^{t_0}
\end{aligned} \tag{4}
$$

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 inputs to the cost effectiveness analysis, the methodology and assumptions of which are described in the next section.

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

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

3 Experimental Design and Methods

This section provides a description of the study site, experimental treatments, experimental design, and study timeline. I then describe the empirical strategy for the study, beginning with a description of the data sources, including the key outcome variables and how each is measured. I then proceed with a description of the analysis approach for the impact evaluation and cost-effectiveness analysis.

3.1 Study design

The study flow diagram can be found in Figure 4.

3.1.1 Study site

I recruited pharmacies from thirteen counties in malaria endemic and epidemic regions in western Kenya (Figure 3).[12] These counties account for 32% of Kenya's population, with a total population 15,231,090 individuals in 3.6 million households. The average household size in these counties is 4.6.

3.1.2 Experimental treatments

The treatment arms incentivize patients, pharmacists, or both to use malaria rapid tests and to use ACTs when confirmed by diagnostic tests. The magnitude of the incentive is held fixed at 200 Kes (~$2 USD) across all three treatment arms.[13] This amount 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 16):

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 subsidy 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 ACT (80% subsidy, a 30 Kes copay) conditional on a confirmed positive malaria diagnosis.

[12] 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.

[13] 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.

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 ACTs to malaria-positive patients (80 Kes). Pharmacy attendants receive a 30 Kes incentive for recording transaction information in the malaria case management platform and completing the sale of incentivized products.

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 ACTs to malaria-positive patients (15 Kes). Pharmacy attendants receive a 30 Kes incentive for recording transaction information in the malaria case management platform and completing the sale of incentivized products.

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. [14]

In all three treatment arms, pharmacy staff receive training on the malaria case management tool and proper rapid test administration, and rapid tests and ACTs 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.

3.1.3 Sample selection and pharmacy randomization

Within study 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

[14] 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.

sales and inventory management tool, be at least 0.5 km away from other study sites,[15] 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.[16] Figure 5 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.

3.2 Experiment timeline and data collection

Below is the study timeline[17] and a description of the primary sources of data:

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

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

[17]The study was initially planned to begin in June 2020, but was delayed due to COVID-19. The research and implementation teams followed Kenyan and UC Berkeley CPHS guidelines for conducting research while keeping study staff, implementation staff, and study subjects safe from COVID-19. All personnel and pharmacy staff were required to wear masks, maintain 1 meter distance from each other, and sanitize hands frequently. The research and implementation teams provided adequate PPE and hand sanitizer for all study and implementation personnel. Pharmacies were required by the Kenyan government to have all staff wearing masks, and have hand washing stations for staff and pharmacy clients, and pharmacies in our sample were compliant with these requirements during the study period. The pharmacy onboarding, patient exit survey, standardized patient visits, and control group testing activities were all done in person following appropriate COVID-19 precautions. The pharmacy baseline surveys were conducted over the phone.

Jun-Dec '21	**Experiment launch**: baseline pharmacy survey with 233 pharmacy owners and staff from all 140 sites; 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 '21-Jan '22	**Patient exit survey**: survey of random sample of 1654 adult clients who seek care for malaria-like symptoms at study pharmacies
Dec '21-Feb '22	**Standardized patient visits**: 412 visits by skilled enumerators presenting as suspected malaria patients to all pharmacies in the study sample, to obtain data on patient-pharmacist interaction, implementation fidelity, and quality of care
Jan-Feb '22	**Control group testing**: testing of random subset of 230 pharmacy clients at control group sites to obtain test positivity rate
Mar '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 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 about malaria case management knowledge, worker motivation, and use of the digital platform used to manage sales and inventory.

2. *Administrative data:*

   (a) Sales data: continuously collected transaction data including prices[18] and quantities of products purchased, location, date, and time of sale, and pharmacy staff who made the sale for over 50,000 malaria-related patient encounters between June 2021 - February 2022.

   (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, location, date, and time of sale. Over 8,000 malaria transactions logged between June 2021 - February 2022.

[18] Prices observed in the data are retail prices set by pharmacists in the digital tool.

3. *Patient exit survey data:* survey with a random sample of 1654 eligible adult pharmacy clients across all study sites (12.6 clients/site). [19] 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 230 pharmacy clients at control group sites to obtain test positivity rate in a sample unaffected by the interventions (8.5 clients tested/site, 28 sites participated). Additional test positivity data from administrative records from 10 control group pharmacies that kept records of tests conducted (N=2547) on-site between January-February 2022.[20]

5. *Endline data:*

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

   (b) Pharmacy staff survey: survey 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.

3.3 Primary and secondary outcomes

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 pharmacy by the digital tools for tracking sales and managing malaria cases.

There are three primary outcomes:

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

2. **ACT targeting, overall**: Proportion of malaria treatments sold at participating pharmacies that are high quality treatments (ACTs) sold with a

[19]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. Trained research staff visited each study pharmacy during an unannounced 5 day period, and screened all patients who exhibited malaria-related symptoms or purchased malaria products for eligibility. There were 1674 possible respondents screened, and 1654 respondents who completed the survey.

[20]These sample sizes were based on availability of testing data from pharmacy administrative records, and testing targets that balanced operational feasibility and research budget.

confirmatory diagnosis (as measured through diagnostic test result) to clients with suspected malaria

3. **ACT without test**: Proportion of high quality treatments (ACTs) sold at participating pharmacies without any diagnosis (as measured through diagnostic test sale)

These primary outcomes will be used to estimate parameters in the cost-effectiveness analysis. 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. 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 pharmacy incentives (T1 and T2), I will learn whether incentives are passed through to patients. Outcome measure 2 (ACT with test) 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. Outcome measure 3 (ACT without a test) examines whether malaria negative patients elect to not purchase antimalarials given their test outcome, and is an indirect measure of treatmentoveruse.

There are three secondary outcomes:

1. **ACT targeting, over ACTs**: Proportion of high quality treatments (ACTs) sold at participating pharmacies with a confirmatory diagnosis (as measured through diagnostic test result) to clients that purchase any ACT.

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

3. **ACT use:** Proportion of high quality treatment (ACT) sales recorded at participating pharmacies as a share of total antimalarial sales

These outcomes provide insight on a fuller picture of how the interventions impact patient volumes and care-seeking behavior at study pharmacies. Antimalarial sales and high-quality treatment use are measures of patient shares overall to answer the question of whether the interventions impacted overall likelihood of malaria-symptomatic patients to seek care at participating pharmacies.

3.4 Empirical strategy

3.4.1 Take-up of the intervention

I measure trial take-up as the subset of eligible pharmacies in the study area that consented to participate in the trial. The total number of eligible pharmacies in the study catchment area was obtained from the administrative records of the implementing partner. Each consenting pharmacy agreed to manage their sales through the digital tool, and to offer incentives (either supply- or demand-side) for malaria testing and treatment if assigned to one of the intervention arms.

I then determine the relationship between trial take-up and eight pharmacy-level characteristics: number of months active on digital sales management tool; baseline (2019-2020) average monthly malaria product sales, high quality malaria treatment (ACT) sales, and rapid diagnostic test sales; participation in earlier study phase, [21] urbanicity, location in a lake endemic county, and pharmacy type. In order to determine whether trial take-up is related to any of these covariates, I conduct pair-wise t-tests comparing these characteristics across pharmacies that declined to participate and those that elected to participate.

3.4.2 Treatment effects on testing and treatment targeting

I present all results of the program impact on testing and treatment targeting in terms of comparisons between each intervention arm and the control group (status quo pharmacy care experience). Additionally, I discuss any significant differences between demand-side incentives (T1) and supply-side incentives (T2), and across all three intervention arms (T1, T2, T3) and compare to the minimum detectable effects that the study was powered to detect. All analyses are conducted at the patient level[22], 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. The analyses specified in this section were pre-registered in a pre-analysis plan (AEARCTR-0004705). I discuss any deviations from the pre-analysis plan where relevant.

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

[21] 60 pharmacies participated in a Phase 1 of the study between November 2020-February 2021, where different levels of patient subsidies for malaria testing and treatment were randomized to patients. This study phase was stopped because of insufficient take-up and operational complexity, and these sites were balanced across treatment arms in the full study sample.

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

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

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, λ_s are strata fixed effects, and ϵ_{ip} is the error term. The β terms represent the log-odds of the treatment effect of each intervention relative to the control group, as percentage point changes. I report all results in terms of marginal effects in relation to the control group mean. I also report p-values from Wald tests comparing the marginal effect coefficients of the demand side and supply side interventions, and all three interventions. A p-value of less than 5 percent on these tests indicates that supply and demand-side incentive targeting have differential impact on the outcome of interest, or that the hybrid intervention has a differential impact than either the supply or demand side arms. 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 (Tables 4 and 5). I include variables that had significant imbalance with the control group at the 10% level or below at baseline as covariates in this adjusted model ($\boldsymbol{X_p}$), as specified in the pre-analysis plan. 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. This pooled regression specification was not pre-specified in the analysis plan, and is below:

$$Pr(Y_{ip}) = expit(\beta_0 + \beta_{pooled} \boldsymbol{T}_{ip} + \lambda_s + \boldsymbol{X}_p + \epsilon_{ip}) \tag{7}$$

3.4.3 Cost-effectiveness

In order to analyze the efficiency of each intervention, I conduct a cost-effectiveness analysis from the implementer's perspective and a societal perspective (including program costs, costs incurred by the patient, and direct costs of over-treatment). My CEA metric is the incremental cost per additional patient who takes ACTs and is malaria positive (so, is appropriately treated). To evaluate cost-effectiveness, I calculate the ratio of the change in benefits to the change in costs across each intervention arm compared to the status quo. I look at benefits defined as the change in patients taking ACTs that are malaria positive, and costs defined as (1) incentive costs and (2) the direct medication costs of over-treating malaria negative patients with 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 direct costs of misallocating antimalarials. The cost effectiveness analysis framework relies on the theoretical model described in 2.2 and parameters recovered from the experimental design.

Benefits are measured as patients who take ACTs appropriately (are malaria positive), therefore only patients who are malaria positive contribute to the benefits. To estimate the number of patients who get ACTs appropriately in each of the intervention arms, I use the following equation:

$$Beneficiaries_t = Pr(ACT|positive)_t \times ACT_t$$

where $Pr(ACT|positive)_t$ is the probability of purchasing an ACT conditional on being malaria positive, for each intervention arm t, and ACT_t is the number of patients in intervention arm t who purchase ACTs. This is the share of patients who purchase ACTs[23], multiplied by a hypothetical cohort of 10,000 patients. $Pr(ACT|positive)_t$ can be further expanded into a component that applies to patients who were tested for malaria and one that applies to patients who were not tested:

$$\begin{aligned} Pr(ACT|positive)_t = & \; Pr(ACT|positive\&tested)_t Pr(positive|tested)_t Pr(tested)_t + \\ & \; Pr(ACT|positive\&untested)_t Pr(positive|untested)_t Pr(untested)_t \end{aligned}$$

Each of these probabilities can be found from the parameters that are measured through the experimental design and data collection activities. $Pr(ACT|positive\&tested)$ is directly estimated from the administrative data in the treatment groups, and is the treatment arm specific mean in column 2 of Table 10. In the control group, this probability is estimated using the control group mean from column 3 of Table 7 (0.057) multiplied by the control group $Pr(positive|tested)$. $Pr(positive|tested)$ is obtained from administrative pharmacy data in all four arms. In the control group, this comes from aggregate reported test positivity rates from 2547 tests done in 10 control group sites that conducted testing between January-February 2022 and kept records. In the treatment groups, this comes from the administrative data collected through the study on individual test results, for patients who tested through the intervention. These parameters are the treatment arm specific means in column 1 of Table 10. $Pr(tested)$ is directly estimated from the administrative data in all four arms, and is the treatment arm specific mean in column 2 of Table 7. $Pr(ACT|positive\&untested)$ is estimated for all four arms and is the treatment group specific means of $Pr(ACT|untested)$ from column 6 in Table

[23]Obtained from intervention group specific means from Table 8 column 4.

7 multiplied by $Pr(positive|untested)$. $Pr(positive|untested)$ is estimated in the control group using data collected from the lab tech activity which tested a random subset of 230 patients who purchased antimalarials for a suspected illness at 28 control group sites but did not get tested prior between January-February 2022. In the treatment groups, $Pr(positive|untested) = Pr(positive) - Pr(positive|tested)$. $Pr(positive)$ is the unselected (for testing) malaria positivity rate, and is obtained from the control group testing data ($Pr(positive|tested) + Pr(positive|untested)$), and $Pr(positive|tested)$ is directly obtained from Table 10, as described above.

The inputs needed to calculate the number of beneficiaries in each intervention arm can be found in Table 3. I estimate the program benefits for each intervention using these parameters and compare them to the status quo standard of care, as well as to the next best alternative. For details on the sources of each parameter input for the benefits, please see Appendix Tables 26 and 27. For details on formulas used to calculate the benefits estimates, please see Appendix Table 28.

The costs can be broken down into direct costs of running the incentives program, the direct costs of over-treating malaria negative patients, and other non-programmatic costs to patients of participating in the program. To estimate these costs, I use the following equation:

$$TotalCost_t = c_t Patients_t + CostOverTx_t \times PatientsOverTx_t + CostTime_t$$

where $t \in (0, 1, 2, 3)$ is one of the three treatment arms or control group, c is the cost of administering the incentive interventions, $Patients$ is the number of patients who purchased an incentivized product, $CostOverTx$ is the cost of over-treating malaria negative patients with antimalarials, $PatientsOverTx$ is the number of patients who were treated unnecessarily, and $CostTime$ is the time cost to patients of obtaining care for their malaria symptoms in the pharmacy setting.

In order to estimate the costs of over-treating malaria negative patients, I first estimate the average cost of treatment for patients who did not get tested for malaria and the average cost of treatment for patients who did get tested for malaria. These cost estimates are directly observed from the administrative data, and I have estimates for each of these out of pocket costs for each of my intervention arms. Then I also observe the number of untested patients and number of tested patients in each treatment arm, again from the administrative data. I estimate the likelihood of being malaria negative condition on being untested, and the likelihood of being malaria negative conditional on being tested in each treatment arm. I use parameter estimates obtained from data collection activities for these probabilities. $Pr(negative|untested)$ is estimated in the control group using data collected from the lab tech activity which tested a random subset of 230 patients who purchased antimalarials for a suspected illness at 28 control group sites but did not get tested prior between January-February 2022. $Pr(negative|tested)$ is obtained in the control group from aggregate reported test positivity rates from 2547 tests done in 10 control group sites that conducted testing between January-February 2022 and kept records. In the treatment groups, $Pr(negative|untested) = Pr(negative) - Pr(negative|tested)$. $Pr(negative)$ is the unselected (for testing) malaria negativity rate, and is obtained from the control group testing data $(1 - (Pr(positive|tested)|Pr(positive|untested)))$, and $Pr(negative|tested)$ is directly obtained from the treatment arm specific means in column 1 of Table 10 $(1 - Pr(positive|tested))$.

Finally, I calculate the time cost to patients of obtaining care for their malaria symptoms in the pharmacy setting. This is relevant because patients may experience longer visit times if they elect to be tested for malaria, which may affect their decision. I obtain estimates of total time spent at pharmacy seeking care from the patient exit survey data (in minutes) for each intervention arm, and multiply that by an estimate of the local hourly wage to obtain a monetary measure of the time cost for care-seeking.

The inputs needed to calculate all cost parameters can be found in Table 3. For details on the sources of each parameter input for the costs, please see Appendix Tables 26 and 27. For details on formulas used to calculate the cost estimates, please see Appendix Table 28.

4 Impact evaluation results

This section presents the main results. Section 4.1 describes characteristics of the study sample, as well as the results of baseline balance tests between treatment and control groups as created through the randomization of incentive interventions. Section 4.2 describes the main intervention effects on testing and treatment targeting outcomes. Section 4.3 describes the main intervention effects on secondary outcomes, including on malaria patient shares and antimalarial sales. Section 4.5 describes the results of the cost-effectiveness analysis.

4.1 Trial take-up and sample characteristics

Incentives for rapid diagnostic tests and high quality treatments (ACTs) were randomized across 140 pharmacies at baseline, with 35 assigned to the control group (25%), 35 assigned to the patient subsidies group (25%), 35 assigned to the pharmacy incentives group (25%), and 35 assigned to the hybrid group that received both patient subsidies and pharmacy incentives (25%). Tables 4 and 5 report the experimental balance checks at baseline (for pharmacy-level variables from the administrative data and survey data, respectively), and shows that randomization was fairly balanced across a large set of prespecified covariates. Out of 84 tests conducted, 8 are significant at the 10 percent level or more. When I conduct a joint test for orthogonality using a multinomial logit model with treatment assignment as the categorical outcome, I find that the χ^2-test produces a p-value of 0.46. This suggests that these covariates are not jointly predictive of group assignment. In my adjusted models, I control for covariates that were unbalanced at baseline from comparisons with the control group, consistent with my pre-specified analysis plan.

I measure fidelity to implementation as whether there were any malaria transactions logged in the digital sales tracking platform for a study pharmacy during the study period (June 2021-February 2022). By this metric, 8 facilities out of 140 were inactive during the study period (1 in the patient subsidy arm, 2 in the pharmacy incentive arm, 2 in the hybrid arm, and 3 in the control group). For the remaining facilities, the number of active facilities by month can be found in Figure 6, and the number of facilities actively selling incentivized malaria tests and treatments can be found in Figure 7. The pharmacy onboarding occurred between June-December 2021, so the increase in the number of active facilities over this time period is due to pharmacies being onboarded to the study in a staggered way.

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 17 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 (refusals). Column 3 reports the differences between the two group means and the significance stars from a t-test comparing the difference in group means. Facilities that declined to participate in the program and study had been using the digital sales platform for longer than facilities in the sample frame. No other meaningful imbalances were found. Appendix tables 18 and 19 report descriptive results from regressing the primary and secondary outcomes on the sample baseline characteristics.

4.2 Treatment effects on testing and malaria 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. My main analyses are intention-to-treat (ITT), since some pharmacies who were assigned the interventions may not have fully administered the incentives throughout the study period and some pharmacy clients may not have been offered the interventions. By including all pharmacies and all malaria clients in an ITT analysis, rather than only including those pharmacies and malaria clients that received the assigned intervention, I preserve the unbiasedness benefits of randomization. However, this will provide a lower bound estimate of the average treatment on the treated (TOT) effect. To complement the ITT analysis, I run additional analyses in Section 4.3 looking at testing and treatment targeting among malaria patients in the treatment groups that received the interventions. Standard errors are clustered at the pharmacy level for all analyses, and all analyses in Sections 4.2 and 4.3, with the exception of some secondary outcomes that will be noted in that section, were pre-specified in the analysis plan.

My first set of analyses answers the first research question: how do targeted incentives impact uptake of malaria diagnostic testing? The first two columns in Tables 6 and 7 show the marginal effects of unadjusted and adjusted logistic models for the outcome measure, rapid test uptake, among all patients who purchased malaria products. In the full sample, there were 51,486 pharmacy clients who purchased malaria related products (a range of treatments, and diagnostic tests) between June 2021 and February 2022, and 13,585 (26%) who purchased a rapid diagnostic test during their pharmacy visit. I present results for both the pooled treatment effect - whether any of the incentive interventions had an impact on this outcome - and for each of the three incentive interventions, in columns 1 and 2 of these tables, respectively. Unadjusted results in Table 6, column 1 show that there is a 28 percentage point increase in the likelihood that patients purchase a rapid diagnostic test when seeking malaria-related care at any of the pharmacies with the incentive interventions, compared to the control group ($p < 0.01$, control group mean = 0.08). Looking at each intervention separately, I find that all three incentive approaches yield similar outcomes. Patient subsidies (T1) increases the likelihood that a patient purchases a rapid diagnostic test by 25 percentage points

($p < 0.05$), pharmacy incentives (T2) increases this likelihood by 20 percentage points ($p < 0.01$), and the hybrid approach increases this likelihood by 25 percentage points ($p < 0.01$). The Wald tests that test for equality across the three intervention arms, and T1 compared with T2 are unable to reject the null hypothesis that there are no differences across the interventions (research questions 3-4, for the diagnostic testing outcome). Table 7, columns 1 and 2 show results of the adjusted analysis on rapid test uptake. These results are consistent with what was found in the unadjusted models - the pooled intervention effect is 25 percentage points ($p < 0.01$), with each intervention separately increasing the likelihood that a patient purchases a rapid diagnostic test by between 20 and 27 percentage points. Again, I am unable to reject the null hypothesis that all three incentive approaches yield the same effect on testing uptake. All of these effects are significantly larger than the minimum detectable effect sizes that were deemed ex-ante economically meaningful of a 13 percentage point change in rapid test uptake when comparing each intervention arm to the control group. While there are very small differences in effect sizes when comparing intervention arms to each other, they are much smaller than the minimum effects that the study was powered to detect.

Turning now to look at the intervention effects on treatment targeting, the next set of analyses answer research question 2: how do targeted incentives impact the likelihood that patients purchase ACTs with an accompanying test result. The third and fourth columns of Tables 6 and 7 show the marginal effects of unadjusted and adjusted logistic models for the outcome measure, ACT uptake with a confirmatory diagnostic test, among patients who purchased malaria products. In the full sample, 5,871 out of 51,486 (11%) clients who purchased malaria products for their symptoms purchased an ACT with an accompanying test. Unadjusted results in Table 6, column 3 show that there is a 9 percentage point increase in the likelihood that patients purchase an ACT with an accompanying diagnostic test at any of the pharmacies with the incentive interventions, compared to the control group ($p < 0.05$, control group mean = 0.06). This is more than a two-fold increase in appropriate treatment targeting. Looking at each intervention separately, I find that all three incentive approaches have effect sizes of similar magnitudes for this outcome (the effect on patient subsidies is statistically insignificant). Pharmacy incentives (T2) increase the likelihood that a patient purchases ACTs with a diagnostic test by by 7 percentage points ($p < 0.1$), and the hybrid approach increases this likelihood by 7 percentage points ($p < 0.05$) as well. The Wald tests that test for equality across the three intervention arms, and T1 compared with T2 are unable to reject the null hypothesis that there are no differences across the interventions (research questions 3-4, for the first treatment targeting outcome). Table 7, columns 3 and 4 show results of the adjusted analysis on the first treatment targeting outcome (uptake of ACTs with a diagnostic test). These results are consistent with what was found in the unadjusted models - the pooled intervention effect is 7 percentage points ($p < 0.05$), with each intervention separately increasing the likelihood that a patient purchases ACTs with a diagnostic test by between 5 and 7 percentage points, with only the hybrid arm statistically significant at the 10% level. Again, I am unable to reject the null hypothesis that all three incentive approaches, or the supply- compared with the demand-side incentives, yield the same effect on this measure of treatment targeting. These effects are smaller than the minimum detectable effects determined to be economically meaningful ex-ante (12 percentage point change when comparing each intervention to the control group, and between 12-19 percentage point change when comparing intervention arms to each other), but are significant because they represent large increases in treatment targeting relative to a very low baseline in these private sector pharmacy outlets.[24]

Columns 5 and 6 of Tables 6 and 7 look at the second dimension of treatment targeting associated with research question 2: how do incentives impact the likelihood that patients purchase antimalarials without an accompanying test. Out of the 51,486 pharmacy clients who purchased malaria products during the study period, 33,390 (67%) purchased ACTs without a diagnosis. Unadjusted results in Table 6, column 5 show that there is a 17 percentage point decrease in the likelihood that patients purchase an ACT without any diagnostic test at any of the pharmacies with the incentive interventions, compared to the control group ($p < 0.05$, control group mean = 0.81). Looking at each intervention separately, I find that all three incentive approaches have effect sizes of similar magnitudes for this outcome of between 13 and 19 percentage points, with only the hybrid arm statistically significant at the 5% level. The Wald tests that test for equality across the three intervention arms, and T1 compared with T2 are unable to reject the null hypothesis that there are no differences across the interventions (research questions 3-4, for the second treatment targeting outcome). Table 7, columns 5 and 6 show results of the adjusted analysis on the second treatment targeting outcome (uptake of ACTs without any diagnosis). These results are consistent with what was found in the unadjusted models - the pooled intervention effect is a decrease of 20 percentage points ($p < 0.01$), with each intervention separately decreasing the likelihood that a patient purchases ACTs without a diagnosis by between 16 and 22 percentage points. Again, I am unable to reject the null hypothesis that all three incentive approaches, or the supply- compared with the demand-side incentives, yield the same effect on this measure of treatment targeting.[25]

[24] Additionally, the benchmarks used to determine the MDEs were based on pilot data and a review of prior literature, which focused on both the public and private sector. Public sector testing and treatment targeting rates tend to be higher than in private sector pharmacies in this setting, so the ex-ante MDEs reflect this as well.

[25] Appendix tables 20 and 21 show that results are robust to using linear probability models. Appendix table 21 includes coefficients for covariates used in the adjusted regressions, for reference. Appendix table 24 shows an alternative specification which includes all baseline covariates from Table 1.

The results from this analysis suggest that all three interventions have strong effects on encouraging patients who seek malaria-related care to get diagnosed in the pharmacy prior to purchasing treatment. Additionally, the results on the two dimensions of treatment targeting - ACT uptake with a test, and ACT uptake without a test - suggest that the incentive interventions encourage malaria positive patients to get the highest quality antimalarials available to them, and discourage malaria negative patients from unnecessarily purchasing antimalarials. The fact that I do not find any significant differences across intervention arms shows that targeting the incentive to either the patient or provider yields similar effects in this setting.

4.3 Treatment effects on secondary outcomes

Tables 8 and 9 present results from analyses looking at intervention effects on overall malaria patient shares, ACT uptake, and an alternative measure of treatment targeting - ACT uptake with an accompanying test, restricted to patients who purchase ACTs. The first outcome - malaria patient shares - was not pre-specified, but provides an important insight into whether there was meaningful sorting of patients with suspected malaria across intervention pharmacies. The second and third outcomes were pre-specified. Table 10 looks at the effects of pharmacy incentives and the hybrid approach compared to the patient subsidies on malaria positivity rate and alternate measures of treatment targeting that link patient purchase behavior to diagnostic test outcome from malaria tests conducted as part of the intervention.

Columns 1 and 2 of table 8 shows marginal effects from unadjusted logistic regression analysis on the outcome of share of pharmacy patients who purchased malaria products. During the study period, there were 265,610 pharmacy client interactions overall and 51,486 of these resulted in the sale of a malaria test or treatment (19%). I do not find any intervention effects on malaria patient share looking at the pooled intervention effect (column 1) or at each intervention arm separately (column 2). The adjusted models presented in columns 1 and 2 of table 9 yield the same results. This suggests that the interventions did not lead to patient sorting by likelihood of purchasing a malaria test or treatment, and instead the effects found on testing and treatment targeting described in the section above are due to behavior changes in a relatively stable patient pool, at least on this dimension.

Columns 3 and 4 of Tables 8 and 9 shows the marginal effects from unadjusted and adjusted logistic regression models on the intervention effects on overall take up of ACTs, respectively. In the entire sample, 40,261 malaria patients purchased ACTs (78%). Notably, in the control group, 87% of all malaria product sales are ACTs - this is consistent with the availability of low cost, high quality treatment in the private sector due to the AMFm manufacturer level subsidies between 2011-2016, that was described in the background section. Looking at the intervention effects on this outcome, I find a pooled effect of -14 percentage points ($p < 0.01$), and individual treatment arm impacts of between -9 and -15 percentage points in the adjusted models ($T1: -15pp, p < 0.05$, $T2: -9pp, p < 0.1$, $T3: -14pp, p < 0.01$), but no significant effects in the unadjusted model and no statistically significant differences across the treatment arms, or between supply- and demand-side interventions. The fact that interventions reduced the likelihood that patients purchased ACTs is a result of the increase in diagnostic testing, and in particular, the confirmed malaria-negative patients electing to not purchase medication.

Finally, columns 5 and 6 of tables 8 and 9 show an alternative measure of treatment targeting, which restricts the sample to pharmacy clients that purchase ACTs and looks at the likelihood that among that group, patients purchase an ACT with an accompanying test. Overall, 40,256 clients purchase ACTs and 15% of them do so with an accompanying diagnostic test ($N = 5,871$). In the control group, only 7% of patients who purchase ACTs do so with a diagnostic test. I find a 13 percentage point increase in the pooled intervention effect, and between a 9-13 percentage point increase when looking at each intervention arm separately. [26]

All of the analyses presented above are ITT estimates that look at overall intervention effects across all treatment arms. All patients who purchased malaria products were included in these ITT estimates, regardless of whether or not they purchased incentivized products. The next set of analyses, in Table 10, restricts the sample to only malaria patients who received diagnostic testing through the incentivized interventions (opted in to the incentive interventions). This can be thought of as a treatment-on-the-treated analysis, and I am only able to examine differences between treatment arms.[27] There are 8,478 malaria patients that make up this analysis sample - these are patients who sought care at treatment group pharmacies and elected to take a rapid diagnostic test through one of the incentivized channels. I first look at whether there are differences across intervention arms in terms of test positivity rate - the likelihood that a patient who is tested with malaria tests positive. Overall, 2,967 patients tested positive for malaria (35%). This positivity rate is consistent across patients who sought care in T1 pharmacies - those where tests and appropriate treatment were heavily discounted - and T3 pharmacies - those with the hybrid incentive of both patient subsidies and pharmacy incentives. However, the positivity rate in the pharmacy incentive arm (T2) is significantly higher at the 10% level by 14 percentage points, at 49%. This difference in test positivity rate in T2 could mean pharmacists in these sites were more motivated to encourage patients who they perceived as having malaria (perhaps based on a clinical assessment of symptoms) to get tested through the incentive program.

[26]Appendix tables 22 and 23 show that results are robust to linear probability models. Appendix table 23 includes coefficients for covariates used in the adjusted regressions, for reference. Appendix table 25 shows an alternative specification which includes all baseline covariates from Table 1.

[27]The treatment targeting outcomes shown here were pre-specified, but the analysis is more limited than what was pre-specified due to data limitations in the administrative data. I do not observe test result for all tested patients, just those who are tested as part of the incentivized interventions (in T1, T2, and T3).

This could be evidence of providers being incentivized to increase supplier induced demand for patients who they suspected were malaria positive based on clinical presentation.

Columns 2 and 3 of table 10 present treatment targeting outcomes. Among those patients who tested for malaria through the incentive programs, 3,025 tested positive across all three arms (35%). In the patient subsidy group, 99% of the malaria positive patients elected to purchase a subsidized ACT after receiving their test result, which is comparable to the hybrid group (98%). In the pharmacy incentive arm, this reduces to 92% (marginal effect of -7 percentage points, $p < 0.05$). Across all three groups, the vast majority of malaria positive patients then go on to take ACTs, but there does appear to be some reduced effect when looking at the pharmacy incentive only arm. This reduction in ACT uptake when positive in the pharmacy incentive only arm could be due to lack of incentive pass-through to patients (patients face a comparatively high cost of ACTs, so elect to purchase alternative medications or go to the public sector to seek care). This effect is not particularly large, and should be considered as part of the results as a whole.

Next, I look at the differences across intervention arms of malaria negative patients purchasing antimalarials unnecessarily. Among those patients who tested for malaria through the incentive programs, 5,504 tested negative across all three arms (65%). In the patient subsidy group, 1% of the malaria negative patients elected to purchase antimalarials after receiving their test results. I find no difference in the pharmacy incentive only arm, but in the hybrid arm I find that this likelihood increases to 11% (increase of 10 percentage points, $p < 0.1$). This appears to be driven by three outlier pharmacies, and is not indicative of a broader trend.

4.4 Testing price pass-through mechanism

In order to test the hypotheses about incentive pass-through, I use exit survey data from the Standardized Patient (SP) visits. SPs conducted a total of 411 visits across 137 facilities in the study sample, with three different SPs visiting each facility. Half of the SP visits were conducted by women. SPs followed a uniform script for how to present a suspected malaria case in a pharmacy setting: SPs were instructed to complain of fever, headache and joint pains in their opening statement and then provided additional information about their illness episode and health history if the pharmacist followed up with additional questions.

SP visits provided a unique opportunity to assess the implementation fidelity and quality of care of the patient-provider interaction at study pharmacies, and Table 11 presents the results described below that characterize the visits. Very few SPs reported that there were signs present at the pharmacies they visited with details about discounted rapid tests or treatment - pharmacies were encouraged to put up posters with price information and were provided with program-specific materials to display at their sites. So, the fact that almost none of the pharmacies had these materials visible indicates that information was not readily available to patients when they came in for care.[28] Over half of SPs reported that they were offered a rapid test for malaria, with no differences across intervention arms (C: 53%; T1: 55%, 95% CI: (40%,70%); T2: 54%, 95% CI: (37%,71%); T3: 59%, 95% CI: (42%, 76%)). Interestingly, this is higher than the rapid test uptake seen in the administrative data. This could reflect the fact that SPs were instructed to not state that they suspected malaria, but rather presented malaria symptoms and waited for the provider to give a clinical assessment and diagnosis. By contrast, pharmacy patients may have been more direct in stating that they suspected malaria in their initial interaction with the pharmacist, and pharmacists may have been more likely to bypass the step of diagnostic testing if a patient demanded medication or indicated confidence in their malaria status.

Of the SPs that took a rapid test at the pharmacy (N=206), I find significant differences in test positivity rates across intervention arms. In the control group, 39% of SPs reported that the pharmacist told them that their test was positive, with significantly lower test positivity rates in each of the intervention arms (between 16-24%). All SPs were confirmed to be malaria-negative prior to beginning field work, and were tested regularly at high quality labs if any symptoms arose during the data collection period. So, all tests should have come back negative given that the SPs did not have malaria by design. The false positive rate for rapid diagnostic tests is 22%, and the positivity rates found in the intervention arms are consistent with this [33, 34]. The implications of this finding of differential test positivity from the SP data suggests that the interventions allow pharmacists to truthfully report malaria test results to patients without losing potential profits from not selling malaria treatment. In the control group, where pharmacists are not incentivized, they seemingly over-report positive test results in order to sell additional malaria medication.

The fact that test positivity rates were lower in intervention group pharmacies suggests that the interventions may have increased transparency in test result reporting because pharmacists had to upload the result to the digital platform and discuss the result with the patient. Half of SPs who tested in the control group saw their results on the rapid test cassette, while between 63-75% of SPs in intervention arms saw their results on the rapid test cassette.

Very few SPs reported that the pharmacist explicitly offered them a discount on rapid tests and/or treatments - suggesting that the incentivized products may not have been framed as discounted. In the control group sites, SPs paid an average of 100.78 KES ($0.87 USD) for rapid tests, which is consistent with the average market price benchmarks found at baseline. In the patient subsidy arm, SPs paid an average

[28] Because of the clustered design, pharmacies were instructed to not have any broad marketing or demand-generation activities for the incentives program outside their shops.

of 85.71 KES ($0.76 USD), and in the hybrid arm, SPs paid an average of 91.3 KES ($0.79 USD), both of which are significantly different from the amount paid in the control group. By contrast, patients in the pharmacy incentive arm paid an average of 100.77 KES ($0.87 USD) for the rapid test, which is statistically indistinguishable from the amount paid in the control group. On average, SPs paid 136.84 KES ($1.18 USD) for medications with no significant differences across intervention arms. Besides rapid tests and medications, SPs also may have incurred consultation fees or other fees as part of their pharmacy visit. On average, SPs paid 266.67 KES ($2.30 USD) for medications and other fees, and there are no meaningful differences across intervention arms except for when comparing the pharmacy incentives group to the hybrid group and to the control group. SPs in the pharmacy incentives group had higher total payments than either the control group or the hybrid group, but not when compared to the patient subsidy group. Across all intervention arms, SPs waited 7.68 minutes for their test result to be ready, which is less than the recommended guidance of 15 minutes. A full analysis of quality of care is beyond the scope of this paper, but will be analyzed in future work.

The key comparisons that I test to measure incentive pass-through are whether the effects from the pharmacy incentive group (T2) are different from those in the patient subsidy group (T1), and whether the effects from the hybrid group (T2) are different from those in the patient subsidy group (T1). Table 12 shows results from regression analysis of these key comparisons. Column 2 of Table 12 shows that the SPs who purchased malaria products from intervention pharmacies were not informed about any discounts at any higher or lower rate than in the control group. However, I find that SPs who sought care in the patient subsidy arm were marginally more likely to have been informed of a discount than those who sought care in the pharmacy incentive arm (7.93% compared to 2.1%, where the Wald test p-value comparing these two coefficients = 0.086[29]) or than those who sought care in the hybrid arm (7.93% compared to 1.9%, where the Wald test p-value comparing these two coefficients = 0.074). Column 4 of table 12 looks at whether there were any differences in the $log(price)$ that SPs paid for rapid tests at participating study pharmacies. Overall, I find significant differences in purchase prices when comparing each intervention to the control group. SPs report paying 40% less for rapid tests in the patient subsidy arm, compared to in the control group ($p < 0.05$, 95% CI: (8%, 72%)), and SPs in the hybrid group report paying 17% less for rapid tests compared to SPs in the control group ($p < 0.1$, 95% CI: (0%, 35%)). The magnitude of the discount offered in the patient subsidy group is 90% for rapid tests, so there is suggestive evidence here that not all of this is getting passed through to patients. However, because it is possible that patients purchased rapid tests at an intervention

[29]For example, 7.98% is calculated by adding the coefficient on T1 0.0313 to the control group mean of 0.048. The rest of these group means are calculated using the same approach, from estimates in column 2 of Table 12.

pharmacy that were not subsidized, this may reflect that pharmacists did not always enroll patients in the incentive program. Additionally, I find significant differences in purchase price for rapid tests when comparing the pharmacy incentive group to the patient subsidy group - SPs in the patient subsidy group pay significantly less for rapid tests than they do in the pharmacy incentive group. This suggests that incentive pass through is not occurring in the pharmacy incentive arm, but is at least partially in the patient subsidy arm. I do not find any evidence of differential pricing when looking at medication sales.

4.5 Cost-effectiveness results

In order to compare the efficiency of each intervention, I conducted a cost-effectiveness analysis from the perspective of the program implementer (including only Maisha Meds's program costs) and from a societal perspective (including Maisha Meds's cost and costs incurred by the care-seeking patient). I estimated incremental cost-effectiveness ratios (ICERs) in terms of cost per patient obtaining ACTs appropriately, defined as being malaria positive. I used a time horizon equal to the duration of the intervention period (8 months) and included all malaria patients who sought care at study pharmacies for this analysis. All methods, parameter inputs and assumptions are described in detail in the methods section.

4.5.1 Benefits

In the control group, the probability of taking an ACT conditional on being malaria positive is $< 1\%$, in each intervention arm this probability is 12%, 14% and 8% in the patient subsidy group (T1), pharmacy incentives group (T2), and the hybrid group (T3), respectively. The total number of beneficiaries in each arm are 75, 874, 1093, and 626 in the control group, T1, T2, and T3, respectively (assuming a hypothetical cohort of 10000 suspected malaria patients who sought care in each arm). These estimates can be found in the top panel of Table 13.

4.5.2 Costs

In the control group, the total implementation cost is \$0, because there is no programmatic cost of administering any incentive interventions. The costs for the intervention arms are \$2,601.00, \$4,084.00 and \$5,039.00 in T1, T2, and T3 respectively. These cost differences are due to the differential take up of incentivized rapid tests and ACTs in each intervention arm, with the hybrid arm having the largest share of patients purchasing incentivized rapid tests driving most of this difference. These cost estimates can be found in the bottom panel of Table 13.

For the societal perspective, I also include the direct medication costs of over-treating malaria negative patients in each of the intervention arms, and the time costs to patients for seeking malaria care at pharmacies in each of the intervention arms in addition to the program implementation costs. In the control group, the total social costs are $361,459, and the societal costs for the intervention arms are $358,805, $335,149, and $374,615 in T1, T2 and T3 respectively. The cost differences are due to differential take up of incentivized rapid tests and ACTs in each intervention arm and the arm-specific malaria test negativity rate, which is highest in the hybrid arm. These cost estimates can be found in the bottom panel of Table 13.

4.5.3 Results

Table 14 presents the incremental cost of each intervention relative to the next less expensive alternative. From the implementer perspective (Maisha Meds's perspective), the incremental costs are relatively small, since the incentive amounts are modest. The control group (status quo) is the cheapest alternative, and the hybrid arm is the most expensive. From a societal perspective, both patient subsidies and pharmacy incentives are cost-saving interventions relative to the control group because of the lower costs incurred from fewer malaria negative patients being treated unnecessarily and lower time costs of care-seeking due to lower patient volumes. The hybrid arm is the most expensive from a societal perspective, because of the larger time cost to patients seeking care, relative to the control group.

Table 15 presents the incremental benefits and ICERs from the implementer perspective (top panel) and from the societal perspective (bottom panel). Within each panel, I present incremental gains and ICERs relative to the next best alternative and the incremental gains and ICERs for each intervention (patient subsidies, pharmacy incentives, or hybrid) relative to the control group. I highlight the results with respect to the control group here, as that is the most policy-relevant benchmark when deciding amongst these possible intervention approaches. The control group resulted in 75 appropriately targeted ACTs. Patient subsidies resulted in 799 additional appropriately targeted ACTs at a cost of $3.26/patient, pharmacy incentives resulted in an additional 1018 patients treated appropriately at a cost of $4.01/patient, and the hybrid approach resulted in an additional 551 patients treated appropriately at a cost of $9.15/patient (all from Panel A, Table 15).

From a societal perspective, I find that patient subsidies result in an additional 799 patients treated appropriately with ACTs at a cost of -$32.93/patient compared to the control group, which is cost-saving. I find that pharmacy incentives are also cost saving: compared to the control group, this intervention leads to 1018 additional patients treated appropriately with ACTs at a cost of -$2.61/patient. And finally, the hybrid intervention leads to an additional 551 ACTs targeted appropriately compared to the control group, at a cost of $23.88/patient.

5 Discussion and Conclusion

Discussion

This study analyzes the effect of demand- and supply-side incentives on malaria testing and treatment targeting, examining how patients and pharmacists make decisions for malaria case management in Kenya. It provides evidence that both patient subsidies and pharmacy incentives for rapid diagnostic testing and quality-assured malaria treatment (ACT) conditional on being malaria positive have a meaningful impact on rapid test uptake and treatment targeting. I show that the share of suspected malaria patients who get diagnosed with a rapid test prior to receiving treatment increases significantly as a result of all three intervention arms relative to the control group: the patient subsidies, pharmacy incentives, and the hybrid approach. Additionally, I find evidence that incentives are effective at increasing the likelihood that quality-assured treatments are appropriately targeted to patients who have a confirmed malaria diagnosis, when compared to the control group. Again, I find no significant difference in impact when comparing demand- to supply-side incentives. All intervention groups lead to a significant decrease in the share of patients who purchase antimalarials without being diagnosed first, which suggests that the interventions are effective at encouraging testing and that patients adhere to their test result when making subsequent treatment decisions. Despite this, there is still a large share of suspected malaria patients who purchase antimalarials without a test across all intervention arms, suggesting that these incentive interventions are not sufficient to change behavior in all patients.

To understand why there was no significant difference in main intervention effects between demand- and supply-side incentives, I test for incentive pass-through using data from Standardized Patient encounters at all study pharmacies. I find that discounts on rapid tests were passed through to patients in the patient discount and hybrid arms. I find suggestive evidence that providers in the pharmacy incentive arm did not pass through any incentives to patients in the form of reduced prices on rapid tests or quality-assured treatments. This analysis suggests that the mechanism of price pass-through in the pharmacy incentive arm is unlikely to be what drives the main impact evaluation results.

From the implementer perspective, all interventions are relatively low cost in terms of getting appropriate high quality treatment to malaria positive patients. The control group resulted in 75 appropriately targeted ACTs. Patient subsidies resulted in 799 additional appropriately targeted ACTs at a cost of \$3.26/patient, pharmacy incentives resulted in an additional 1018 patients treated appropriately at a cost of \$4.01/patient, and the hybrid approach resulted in an additional 551 patients treated appropriately at a cost of \$9.15/patient. From a societal perspective, the patient subsidy and the pharmacy incentive approaches were cost-saving compared to the control group, while the hybrid arm cost \$24/patient for an additional 551 patients being appropriately treated, so is also relatively low cost. The cost-effectiveness analysis can be extended to be directly comparable with other studies by looking at costs/DALY. 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.

5.1 Limitations

I discuss the limitations of this paper here. First, is the reliance on administrative pharmacy transaction data for the main outcomes. Pharmacists log all sales transactions in a sales tracking and inventory management application, and this database is the most comprehensive source of micro-level pharmacy transaction data to my knowledge. However, these data may be incomplete if pharmacists choose to not log certain transactions in the digital tool. Second, this study can only identify the impacts of the interventions on testing decisions and treatment purchase behavior. Future work should consider examining the results on treatment adherence, and medium and long-term impacts of continued or phased out incentive interventions. Third, this sample is not representative of the rest of Kenya. The pharmacies in this study operate in areas of high malaria burden, represent a sample of sites that are more formalized than other chemists and drug shops that are present throughout the country, and may be more predisposed to adopt new technology or try new innovations given that they adopted the digital sales and inventory management platform. Finally, because the incentive amounts are held fixed across all three intervention arms (at $2 USD), I am not able to look at the additive effect of the patient subsidy and provider incentive in the hybrid group.

The pass-through analysis is based on a relatively small sample of SP visits. Second, the SP case presentation was meant to mirror what a typical suspected-malaria patient interaction looked like, and the SPs were instructed to present generalized symptoms but not state that they suspected malaria in the opening statement. This was done in order to assess the diagnostic ability of pharmacists and obtain information on testing recommendations and proper adherence to protocols. If the majority of patients who seek care at these pharmacies are confident in their malaria status and demand antimalarials without a test, then the SP sample could look different from the average patient population. Future work will look at a variant on this standard case where SPs were more certain in their malaria presentation and demanded antimalarials at the onset of the patient-pharmacist interaction.

The cost-effectiveness analysis is comprehensive, but not without limitations. First, I do not estimate the long-term costs and benefits of the incentive interventions. Second, I do not include the costs of rapid tests and treatments for malaria positive patients in these calculations. Future versions of this analysis should consider these costs as well. Third, my estimates for calculating the probabilities of being malaria positive and malaria negative in the tested and untested samples are based on data collected from a subset of the sample. While the assumptions used in this analysis are not unreasonable, they are strong, and future studies should consider obtaining estimates of test positivity from untested and tested samples of patients at multiple points during the year across all intervention arms. Fourth, I do not include pharmacist time spent counseling patients as part of the costs for several reasons.Fifth, I do not include costs or benefits to patients who test negative for malaria in obtaining further counseling and appropriate care, nor do I include the broader societal costs of over-treating malaria negative patients (quantifying drug resistance potential, for example). These are both important areas of future work when thinking about overall management of febrile illness, and future studies should consider measuring this aspect and incorporating it formally into a cost effectiveness framework. Future analysis should include a sensitivity analysis to some of these key parameters.

Contributions to research and policy

This study adds to the literature on how individualized health information and financial incentives can be combined to change health behavior. Critically, this study builds on several other field experiments that look at the effectiveness of incentives in improving uptake of malaria diagnostic testing and treatment targeting. Cohen, Dupas and Schaner [6] find that subsidizing ACTs leads to significant increases in ACT uptake, but that the increased demand comes at a cost: only about half of the subsidized ACTs are taken by malaria-positive individuals. They find that a rapid test subsidy also increases uptake of testing, but does little to improve testing targeting. Subsequent work by Prudhomme O'Meara and colleagues [12] in Kenya finds that providing free rapid tests increases testing uptake, and providing subsidies for ACTs when patients test positive increases the use of ACTs when malaria positive. I find results on testing uptake that are consistent with both of these studies: incentivizing rapid tests does increase usage significantly. However, I additionally find that diagnosis conditional incentives for ACTs improve treatment targeting. One key difference between this study and those that came before it is that these incentive interventions are entirely administered in pharmacies and by pharamcists, rather than using community health workers to test and refer patients to retail outlets to purchase treatments.

This study contributes to our understanding of how incentives targeted at the demand-side or the supply-side can affect health decision-making by comparing demand- and supply-side incentive approaches directly to each other. The fact that I find no significant differences in testing uptake or treatment targeting between demand- and supply-side approaches suggests that targeting incentives to either patients or providers is effective at improving malaria case management in a pharmacy setting. This is interesting from a policy and implementation perspective, because it provides some evidence that incentive programs can be applied at the level that is most operationally-feasible, with limited impact on overall effectiveness in terms of end-user outcomes. However, the fact that I do not find evidence of incentive pass-through in the pharmacy incentive arm suggests that incentivizing pharmacists may not lead to patients getting discounted tests and treatment, and instead the increased demand may be due to other mechanisms.

Finally, this study provides evidence on how pharmacists make decisions, and provides cost-effectiveness estimates that can be used by decision-makers to compare this intervention to others aimed at improving malaria care. Given that pharmacies play an important role in health care provision in many low- and middle-income countries, understanding pharmacist motivations and how interventions aimed at improving quality of care work in a pharmacy context is a crucial policy relevant question. The fact that I find improvements in malaria case management suggests that pharmacy-level interventions is one promising avenue to improve quality of care.

Conclusion

Demand- and supply-side incentives are both effective at encouraging uptake of rapid diagnostic testing for malaria and improving treatment targeting in a pharmacy setting, which is characterized by low levels of diagnostic testing and high levels of antimalarial overuse. This research demonstrates that incentives targeted to either patients or pharmacists can lead to improvements in malaria case management, and that having pharmacy-level programs aimed at improving malaria care has the potential to improve outcomes.

6 Figures

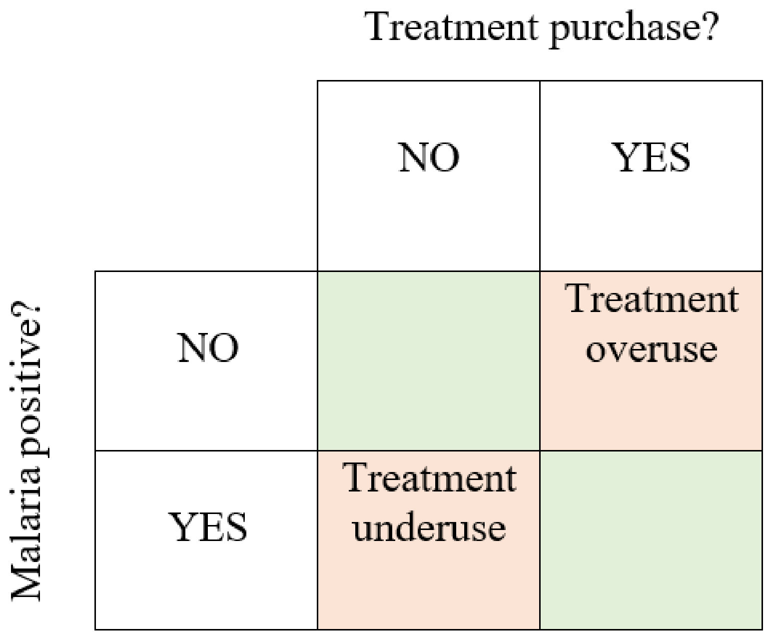

Figure 1: Types of errors (Back: 2)

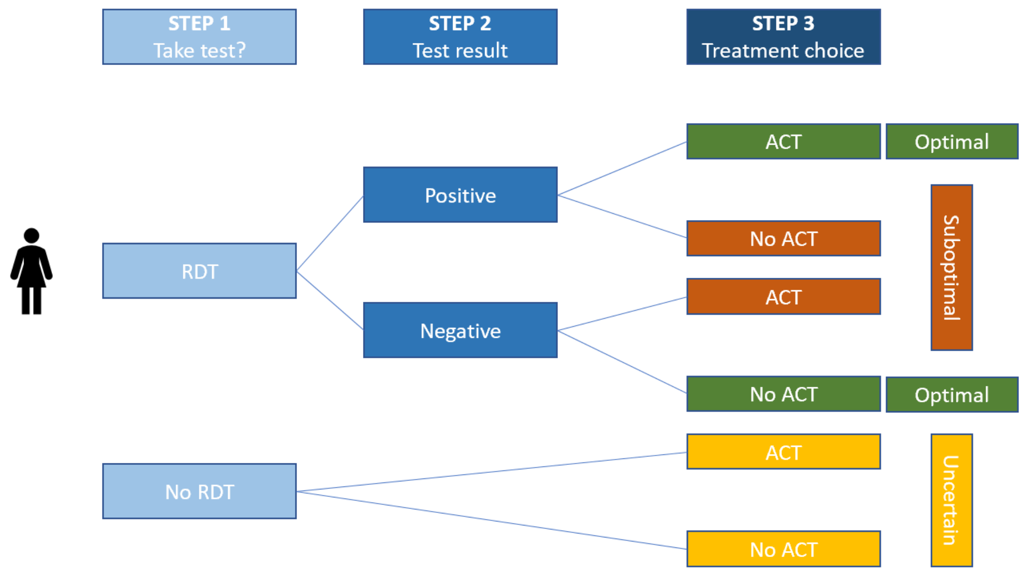

Figure 2: Patient decision to test and treat (Back: 2.1)

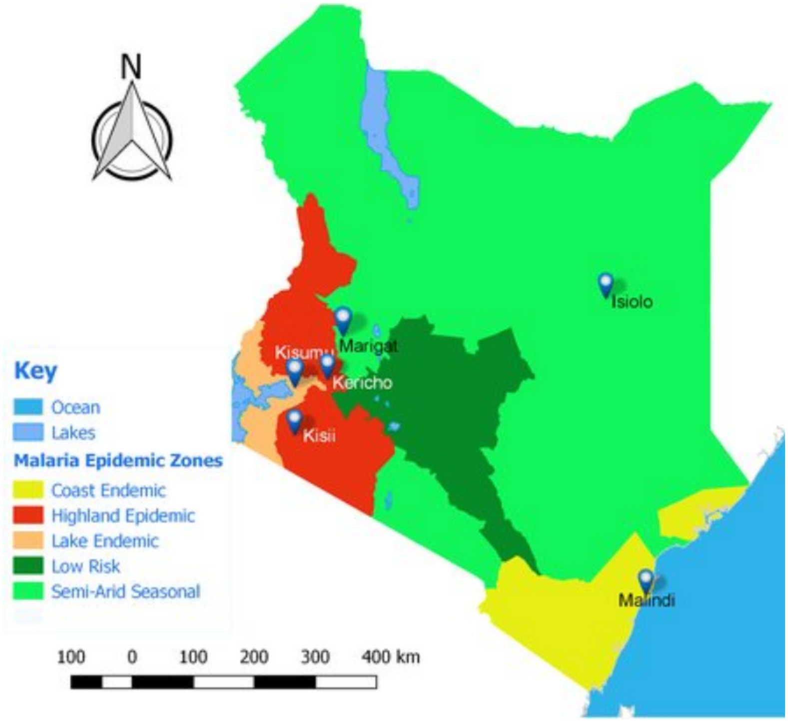

Figure 3: Malaria zones in Kenya, source: [38] (Back: 3.1.1)

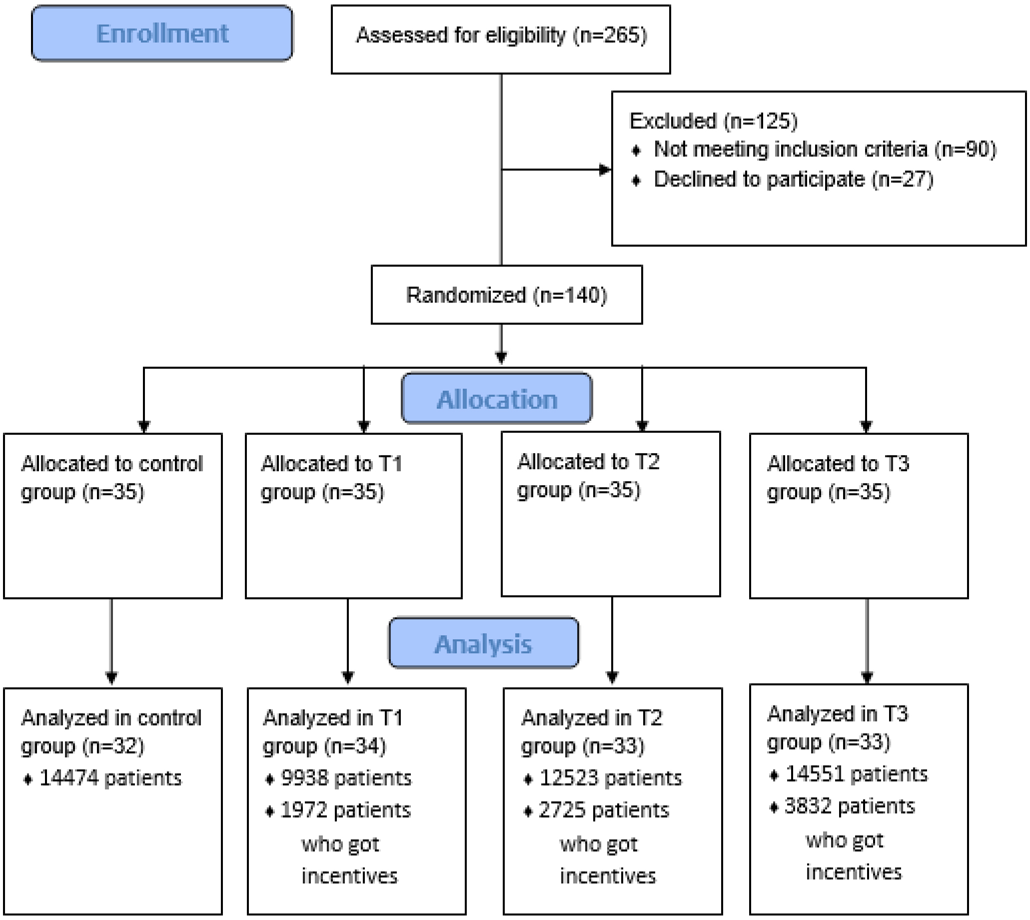

Figure 4: Study flow diagram (Back: 3.1.2)

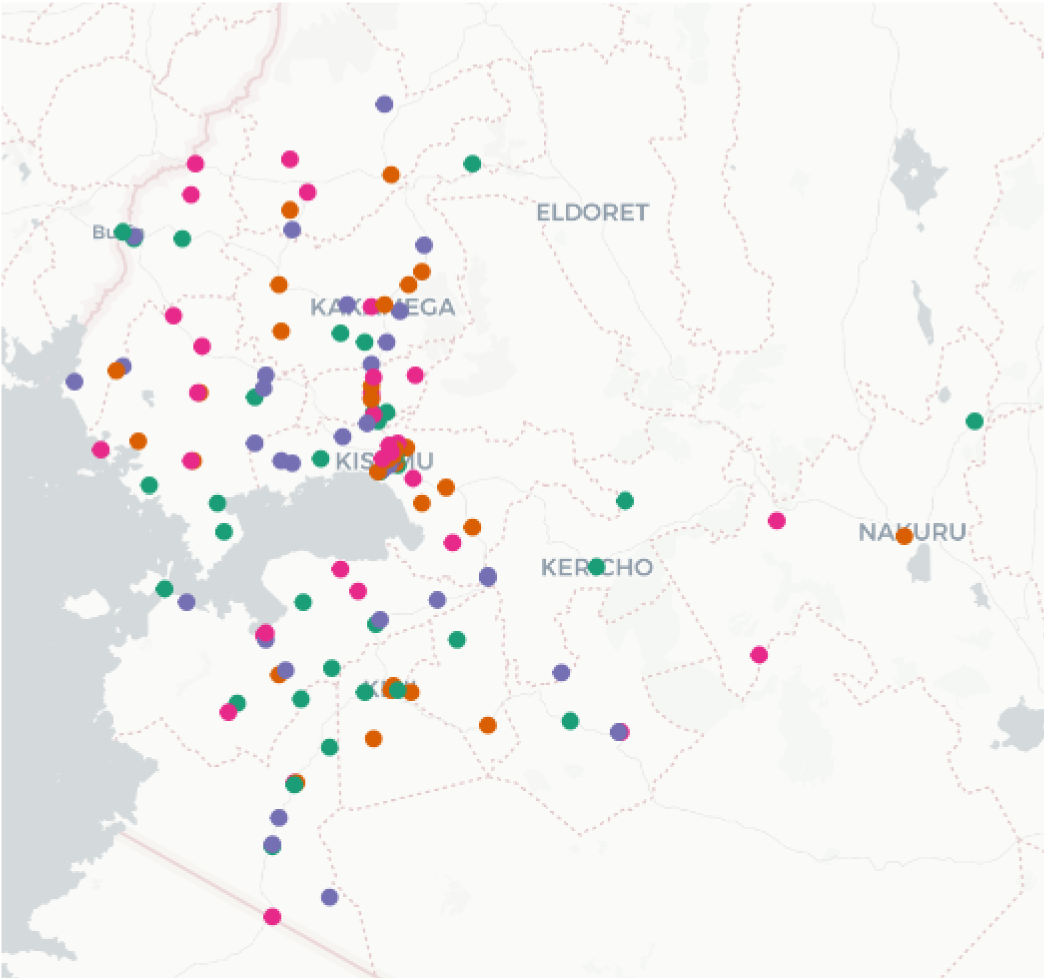

Figure 5: Map of study sites (Back: 3.1.3)

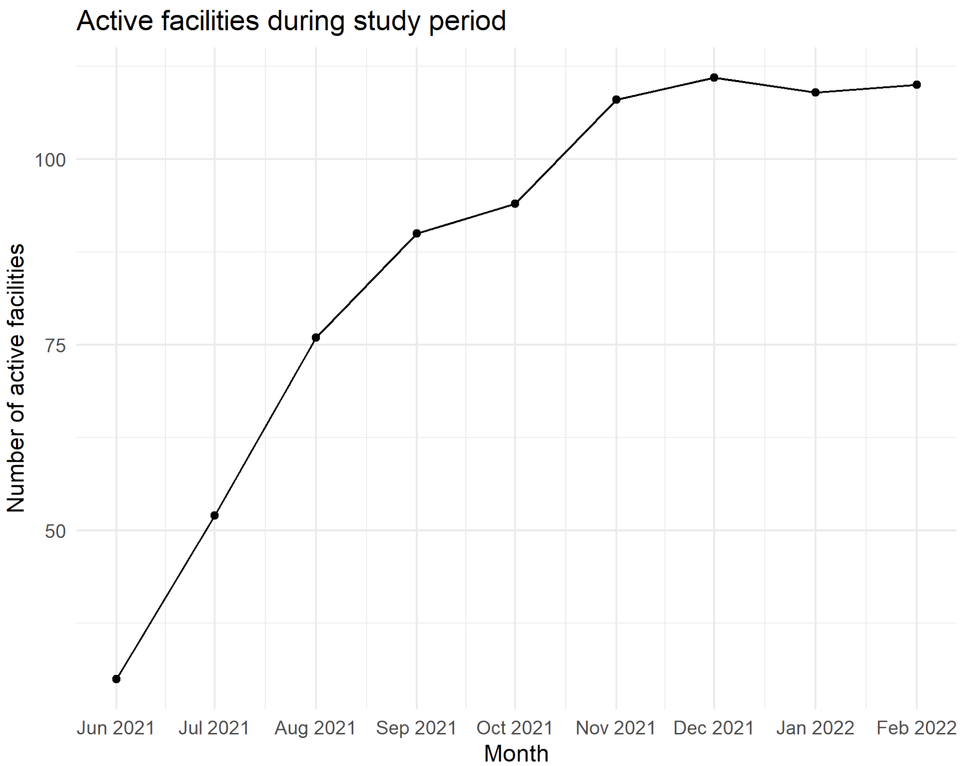

Figure 6: Number of active facilities during study period 4.1)

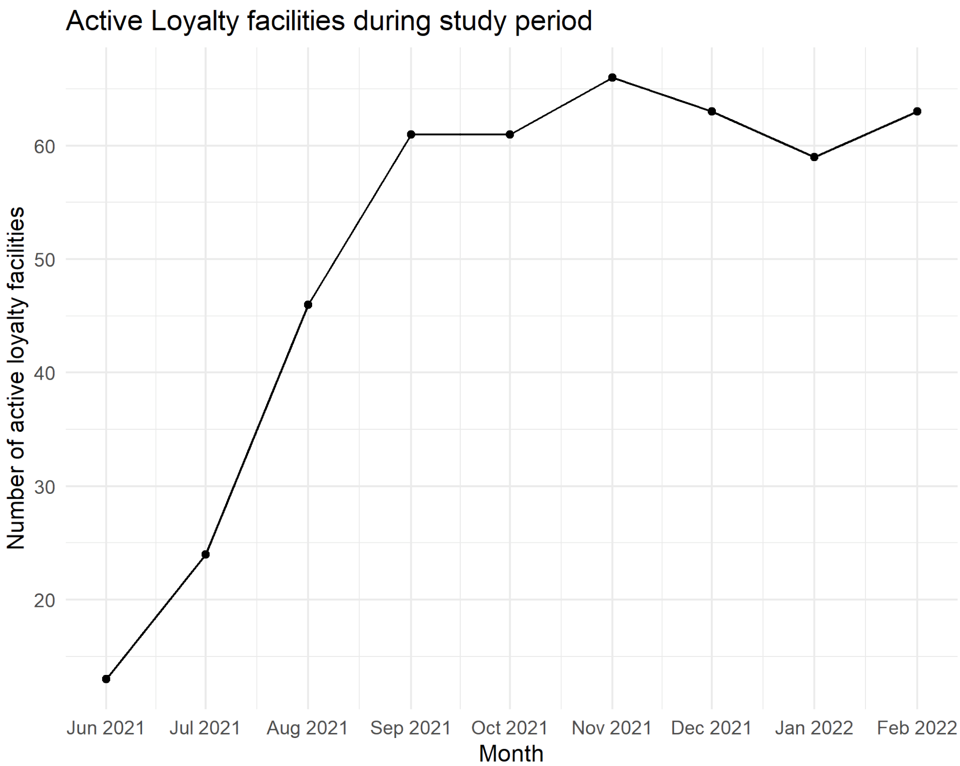

Figure 7: Number of active loyalty facilities during study period 4.1)

7 Tables

Table 1: Comparison group means for power calculations (Back: ??)

	RDT uptake	ACT targeting
Control group	0.088	0.072
Lower bound comparison	0.209	0.132
Midrange comparison	0.234	0.18
Upper bound comparison	0.343	0.22

Table 2: MDEs detectable at 85% power for 140 sites (35/arm), 100 people/site (75 ACT sales/site) (Back: ??)

Table 2. Minimum Detectable Effect Sizes at 85% power

	RDT uptake	ACT targeting
C	0.13	0.12
LB	0.17	0.14
MB	0.18	0.16
UB	0.19	0.16

	Control (status quo)	T1 Patient subsidies	T2 Provider incentives	T3 Hybrid
	PARAMETER INPUTS			
P(tested)	0.082	0.344	0.282	0.29
P(untested)	0.918	0.656	0.718	0.71
P(malaria positive | tested)	0.237	0.354	0.495	0.298
P(malaria positive | untested)	0.106	-0.011	-0.152	0.045
P(malaria positive)	0.343	0.343	0.343	0.343
P(ACT | malaria positive & tested)	0.014	0.996	0.93	0.98
P(ACT | malaria positive & untested)	0.086	-0.01	-0.1	0.03
P(malaria negative)	0.657	0.657	0.657	0.657
Share of patients who purchased ACTs	0.867	0.72	0.7763	0.731
Incentive unit cost (RDT) ($)	$0.00	$0.90	$1.20	$1.10
Share of patients purchasing incentivized RDT	0	0.201	0.279	0.391
Incentive unit cost (ACT) ($)	$0.00	$1.10	$0.80	$0.90
Share of patients purchasing incentivized ACT	0	0.072	0.092	0.082
Average antimalarial treatment unit cost ($), untested	$1.92	$2.12	$1.49	$1.99
Share of untested patients	0.919	0.647	0.723	0.721
P(malaria negative | untested)	-0.106	0.011	0.152	-0.045
Average antimalarial treatment unit cost ($), tested	$4.02	$0.79	$1.98	$1.26
Share of tested patients	0.081	0.353	0.277	0.279
P(malaria negative | tested)	0.763	0.646	0.505	0.702
Time cost of seeking care	$18.04	$17.71	$16.33	$18.39
Hourly wage ($)	$2.00	$2.00	$2.00	$2.00
Number of patients who accessed care	10000	10000	10000	10000

Table 3: Cost Effectiveness Analysis Inputs (Back: 3.4.3)

Variable	(1) Control	(2) T1	(3) T2	(4) T3	(5) T1-T2	(6) T3-T1	(7) T2-T3
Number of months active on digital sales management tool	10.14	9.06	5.37*	8.11	3.69+	-0.94	-2.74
	(10.48)	(9.42)	(8.02)	(10.02)	(0.08)	(0.69)	(0.21)
Below median baseline malaria sales	0.37	0.46	0.37	0.46	0.09	-0.00	-0.09
	(0.49)	(0.51)	(0.49)	(0.51)	(0.47)	(1.00)	(0.47)
Average monthly malaria sales, 2019-2020	64.80	54.97	53.12	43.58+	1.85	-11.39	9.55
	(60.34)	(53.99)	(50.49)	(39.98)	(0.88)	(0.32)	(0.38)
Average monthly quality treatment sales, 2019-2020	52.84	46.89	47.64	38.60	-0.75	-8.29	9.04
	(39.27)	(50.21)	(48.16)	(38.46)	(0.95)	(0.44)	(0.39)
Average monthly rapid test sales, 2019-2020	4.30	7.56	4.61	3.53	2.95	-4.03+	1.08
	(5.34)	(11.35)	(9.17)	(4.06)	(0.24)	(0.05)	(0.53)
Site was in earlier pilot study phase	0.17	0.20	0.17	0.09	0.03	-0.11	0.09
	(0.38)	(0.41)	(0.38)	(0.28)	(0.76)	(0.18)	(0.29)
Site is in an urban area	0.20	0.29	0.40+	0.34	-0.11	0.06	0.06
	(0.41)	(0.46)	(0.50)	(0.48)	(0.32)	(0.61)	(0.63)
Site is in a malaria endemic county	0.89	0.77	0.80	0.91	-0.03	0.14	-0.11
	(0.32)	(0.43)	(0.41)	(0.28)	(0.77)	(0.10)	(0.18)
Site does not have clinical capabilities	0.71	0.83	0.83	0.83	0.00	-0.00	0.00
	(0.46)	(0.38)	(0.38)	(0.38)	(1.00)	(1.00)	(1.00)
Observations	35	35	35	35	70	70	70

Significance stars are from pairwise comparisons with the control group: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$
Differences between treatment arms are in columns 5-7 (p-values in parentheses)
Multinomial logit test for joint orthogonality produces p-value from χ^2-test of 0.46

Table 4: Baseline Balance Between Treatment Arms, Administrative Data (Back: 4.1)

Variable	(1) Control	(2) T1	(3) T2	(4) T3	(5) T1-T2	(6) T3-T1	(7) T2-T3
Percentage of pharmacy staff who are female	0.44	0.35	0.51	0.44	-0.16+	0.09	0.07
	(0.42)	(0.41)	(0.39)	(0.42)	(0.10)	(0.36)	(0.46)
Age of pharmacy owner	37.43	35.37	36.40	35.71	-1.03	0.34	0.69
	(5.28)	(7.74)	(7.14)	(8.08)	(0.57)	(0.86)	(0.71)
Average age of pharmacy staff	29.37	29.37	28.94	29.29	0.43	-0.09	-0.34
	(5.32)	(4.15)	(5.80)	(5.11)	(0.72)	(0.94)	(0.79)
Owner is female	0.36	0.16*	0.17+	0.17+	-0.01	0.01	0.00
	(0.48)	(0.36)	(0.38)	(0.36)	(0.87)	(0.87)	(1.00)
Number of pharmacy staff	1.54	1.54	1.49	1.40	0.06	-0.14	0.09
	(0.51)	(0.56)	(0.51)	(0.50)	(0.66)	(0.26)	(0.48)
Observations	35	35	35	35	70	70	70

Significance stars are from pairwise comparisons with the control group: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$
Differences between treatment arms are in columns 5-7 (p-values in parentheses)
Multinomial logit test for joint orthogonality produces p-value from χ^2-test of 0.46

Table 5: Baseline Balance Between Treatment Arms, Pharmacy Survey Data (Back: 4.1)

Table 6: Impact on Primary Outcomes, Unadjusted Models (Back: 4.2)

	Rapid test uptake		ACT uptake with test		ACT uptake without test	
	(1)	(2)	(3)	(4)	(5)	(6)
Pooled treatment	.275**		.0854*		-.173*	
	(0.065)		(0.039)		(0.085)	
T1		.246*		.0703		-.171
		(0.123)		(0.060)		(0.135)
T2		.198**		.0729+		-.128
		(0.075)		(0.043)		(0.095)
T3		.249**		.0726*		-.191*
		(0.059)		(0.034)		(0.085)
Control mean	0.081	0.081	0.057	0.057	0.809	0.809
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2} \neq \gamma_{T3}$)		0.84		0.99		0.81
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2}$)		0.72		0.97		0.75
N	51441	51441	51486	51486	51441	51441

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
Denominator for all three outcomes is all patients that purchased malaria product during study period
Outcome 1 & 3: 45 obs dropped b/c multicollinearity (strata 11)
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table 7: Impact on Primary Outcomes, Adjusted Models (Back: 4.2)

	Rapid test uptake		ACT uptake with test		ACT uptake without test	
	(1)	(2)	(3)	(4)	(5)	(6)
Pooled treatment	.252**		.0738*		-.197**	
	(0.052)		(0.035)		(0.061)	
T1		.272*		.0742		-.22^{+}
		(0.113)		(0.055)		(0.117)
T2		.196**		.0752		-.16*
		(0.068)		(0.046)		(0.079)
T3		.198**		.0492^{+}		-.181**
		(0.055)		(0.029)		(0.069)
Control mean	0.081	0.081	0.057	0.057	0.809	0.809
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2} \neq \gamma_{T3}$)		0.83		0.83		0.89
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2}$)		0.54		0.99		0.63
N	51441	51441	51486	51486	51441	51441

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
Denominator for all three outcomes is all patients that purchased malaria product during study period
Outcome 1 & 3: 45 obs dropped b/c multicollinearity (strata 11)
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table 8: Impact on Secondary Outcomes, Unadjusted Models (Back: 4.3)

	Antimalarial sales overall		ACT uptake, all malaria product purchases		ACT uptake with test, ACT purchases	
	(1)	(2)	(3)	(4)	(5)	(6)
Pooled treatment	-.0139		-.102		.134*	
	(0.029)		(0.084)		(0.053)	
T1		.0279		-.104		.0996
		(0.032)		(0.092)		(0.091)
T2		-.0167		-.0568		.103^{+}
		(0.034)		(0.080)		(0.059)
T3		-.0304		-.119		.128*
		(0.034)		(0.076)		(0.050)
Control mean	0.197	0.197	0.867	0.867	0.066	0.066
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2} \neq \gamma_{T3}$)		0.14		0.27		0.57
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2}$)		0.12		0.19		0.54
N	265610	265610	51486	51486	40256	40256

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
Outcome 3: 50 obs dropped b/c multicollinearity (strata 11)
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table 9: Impact on Secondary Outcomes, Adjusted Models (Back: 4.3)

	Antimalarial sales overall		ACT uptake, all malaria product purchases		ACT uptake with test, ACT purchases	
	(1)	(2)	(3)	(4)	(5)	(6)
Pooled treatment	-.000792		-.139**		.125**	
	(0.024)		(0.050)		(0.046)	
T1		.0243		-.147*		.123
		(0.025)		(0.073)		(0.100)
T2		-.00859		-.0907+		.112+
		(0.033)		(0.052)		(0.062)
T3		-.00829		-.136**		.093*
		(0.027)		(0.047)		(0.043)
Control mean	0.197	0.197	0.867	0.867	0.066	0.066
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2} \neq \gamma_{T3}$)		0.27		0.64		0.95
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2}$)		0.19		0.46		0.92
N	265610	265610	51486	51486	40256	40256

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
Outcome 3: 50 obs dropped b/c multicollinearity (strata 11)
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table 10: Within intervention arm impacts on test positivity outcomes, Unadjusted Models (Back: 4.3)

	Rapid test positivity rate	ACT uptake with positive test	Antimalarial uptake with negative test
	(1)	(2)	(3)
T2	.141^{+}	-.0655*	.00278
	(0.077)	(0.028)	(0.006)
T3	-.0563	-.016	.104^{+}
	(0.074)	(0.010)	(0.054)
T1 mean	0.354	0.996	0.014
Wald test p-val ($\gamma_{T2} \neq \gamma_{T3}$)	$<$0.001	0.119	0.054
N	8478	2967	5213

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 T2 and T3
Outcome 1 denominator: all patients were tested through incentive interventions
Outcome 2 denominator: all patients who tested positive through incentive interventions
Outcome 3 denominator: all patients who tested negative through incentive interventions
Outcome 1: 51 observations dropped from strata 11, 12 & 15 for multicollinearity
Outcome 2: 58 observations dropped from strata 1, 4, 7 & 8 for multicollinearity
Outcome 3: 291 observations dropped from strata 1, 4, 8, 11, 14 & 15 for multicollinearity
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Variable	(1) Control	(2) T1	(3) T2	(4) T3	(5) T1-T2	(6) T3-T1	(7) T2-T3
SP is female	0.53	0.52	0.51	0.48	0.01	-0.04	0.03
	(0.50)	(0.50)	(0.50)	(0.50)	(0.89)	(0.58)	(0.68)
Sign visible at pharmacy about rapid test availability	0.03	0.03	0.03	0.01	-0.00	-0.02	0.02
	(0.17)	(0.17)	(0.17)	(0.10)	(1.00)	(0.31)	(0.31)
Sign visible at pharmacy at malaria treatment availability	0.03	0.01	0.03	0.08	-0.02	0.07*	-0.05
	(0.17)	(0.10)	(0.17)	(0.27)	(0.31)	(0.02)	(0.12)
SP was offered rapid test	0.53	0.55	0.54	0.59	0.01	0.04	-0.05
	(0.50)	(0.50)	(0.50)	(0.49)	(0.89)	(0.57)	(0.48)
SP was offered microscopy	0.09	0.17+	0.12	0.06	0.05	-0.11*	0.06
	(0.28)	(0.37)	(0.32)	(0.24)	(0.32)	(0.01)	(0.14)
Length of time (minutes) SP waited for test results	7.98	7.43	7.10	8.19	0.33	0.76	-1.09
	(4.89)	(3.38)	(3.55)	(3.97)	(0.63)	(0.30)	(0.14)
Rapid test result was positive	0.39	0.16**	0.21**	0.24*	-0.05	0.08	-0.03
	(0.49)	(0.37)	(0.41)	(0.43)	(0.54)	(0.33)	(0.72)
SP saw test results on rapid test cassette	0.51	0.63	0.75*	0.67	-0.12	0.03	0.08
	(0.50)	(0.49)	(0.44)	(0.48)	(0.21)	(0.72)	(0.35)
SP was explicitly offered a discount at time of purchase	0.05	0.08	0.03	0.03	0.05	-0.05	-0.00
	(0.21)	(0.27)	(0.17)	(0.17)	(0.12)	(0.12)	(1.00)
Number of medications purchased by SP	1.68	1.59	1.84	1.56	-0.25+	-0.03	0.28*
	(0.91)	(1.01)	(0.88)	(1.00)	(0.06)	(0.83)	(0.03)
Amount paid for rapid test (KES)	100.78	85.71*	100.77	91.30+	-15.05*	5.58	9.47*
	(27.70)	(42.57)	(20.37)	(27.06)	(0.02)	(0.42)	(0.04)
Amount paid for medication (KES)	143.52	163.64	100.69	139.31	62.95+	-24.32	-38.63
	(249.13)	(265.40)	(205.12)	(210.78)	(0.06)	(0.47)	(0.19)
Observations	105	102	102	102	204	204	204

Significance stars in columns 1-4 are from pairwise comparisons with the control group
Differences between treatment arms are in columns 5-7 (p-values in parentheses)
+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table 11: SP visit details, descriptive statistics (Back: 4.4)

Table 12: Evidence of incentive pass-through, SP visits (Back: 4.4)

	Discount offered		Price of rapid test (log)		Price of medication (log)	
	(1)	(2)	(3)	(4)	(5)	(6)
Pooled treatment	-.00817		-.183*		-.15	
	(0.027)		(0.082)		(0.160)	
T1		.0313		-.396*		-.169
		(0.037)		(0.162)		(0.228)
T2		-.0269		-.0161		-.272
		(0.033)		(0.071)		(0.167)
T3		-.029		-.17^{+}		-.0121
		(0.031)		(0.093)		(0.193)
Control mean	0.048	0.048	4.600	4.600	5.040	5.040
Test p-val ($T1 \neq T2$)		0.086		0.015		0.606
N	338	338	205	205	251	251

Reporting marginal effects from the control group, from logistic regressions
Standard errors are clustered at the facility level
Wald test comparisons of difference in marginal effects (γ) between treatment arms in Column 2
F test comparisons of difference in marginal effects (β) between treatment arms in Columns 4 & 6
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table 13: Benefits and Costs Estimates (Back: 4.5)

	Control (status quo)	Patient subsidies	Provider incentives	Hybrid
		BENEFITS		
P(ACT | malaria positive)	0.009	0.121	0.141	0.086
Number of patients taking ACTs	8670	7200	7763	7310
Number of beneficiaries	75	874	1093	626
		COSTS		
Total cost of incentives	$0.00	$2,601.00	$4,084.00	$5,039.00
Cost of over-treating malaria negative patients	$618.68	$1,943.89	$4,405.45	$1,815.58
Total time cost to patients seeking care	$360,840.00	$354,260.00	$326,660.00	$367,760.00
Total costs - societal perspective	$361,458.68	$358,804.89	$335,149.45	$374,614.58
Total costs - implementer perspective	$0.00	$2,601.00	$4,084.00	$5,039.00

Table 14: Incremental Costs (Back: 4.5)

	Implementer perspective	
	Costs	Inc. cost
Control (status quo)	$0.00	-
TI - Patient subsidies	$2,601.00	$2,601.00
T2 - Provider incentives	$4,084.00	$1,483.00
T3 - Hybrid	$5,039.00	$955.00
	Societal perspective	
	Costs	Inc. cost
T2 - Provider incentives	$335,149.45	-
T1 - Patient subsidies	$358,804.89	$23,655.44
Control (status quo)	$361,458.68	$2,653.79
T3 - Hybrid	$374,614.58	$13,155.90

Implementer perspective includes only incentive costs. Societal perspective includes incentive costs, costs of overtreating malaria negative patients, and time costs. Incremental cost = incremental cost relative to next most expensive alternative.

Table 15: Incremental Benefits and ICERs (Back: 4.5)

Incremental benefits and ICERs - Implementer perspective

	Number of malaria positive patients treated with ACTs	Incremental appropriately treated	ICER Cost / patient appropriately treated
	Compared to next best alternative		
Control (status quo)	75	-	-
T3 - Hybrid	626	551	$9.15
T1 - Patient subsidies	874	248	-$9.83
T2 - Provider incentives	1093	219	$6.77
	Each intervention compared to the control group		
T1 vs. C	-	799	$3.26
T2 vs. C	-	1018	$4.01
T3 vs. C	-	551	$9.15

Incremental benefits and ICERs - Societal perspective

	Number of malaria positive patients treated with ACTs	Incremental appropriately treated	ICER Cost / patient appropriately treated
	Compared to next best alternative		
Control (status quo)	75	-	-
T3 - Hybrid	626	551	$71.62
T1 - Patient subsidies	874	248	-$63.75
T2 - Provider incentives	1093	219	-$108.02
	Each intervention compared to the control group		
T1 vs. C	-	799	-$32.93
T2 vs. C	-	1018	-$2.61
T3 vs. C	-	551	$23.88

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A Appendix Figures

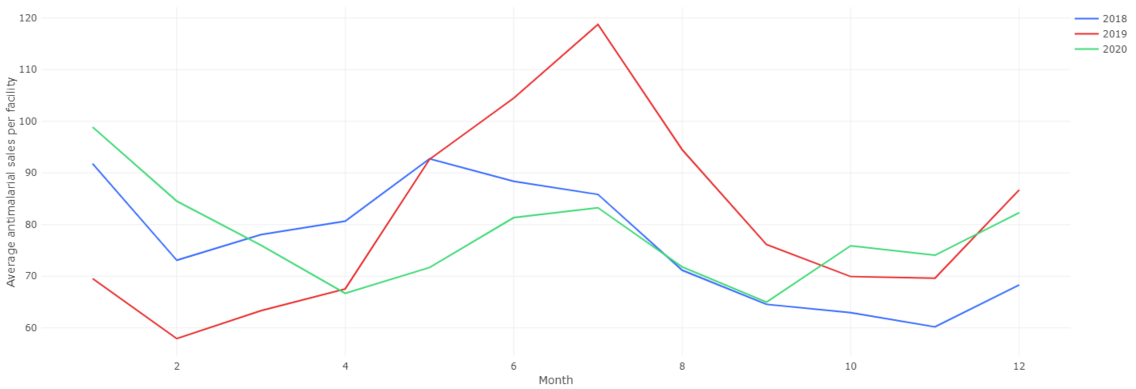

Figure 8: Malaria sales, seasonal trends (Back: 1.1)

B Appendix Tables

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 16: Incentive amount details, by treatment arm (Back: 3.1.2)

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	63.39	66.47	3.08
	(63.56)	(75.45)	(0.83)
Average monthly quality treatment sales, 2019-2020	54.41	61.25	6.84
	(54.26)	(72.61)	(0.59)
Average monthly rapid test sales, 2019-2020	6.48	5.39	-1.08
	(9.93)	(11.26)	(0.62)
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

In sample facilities include those that were randomized to one of the study arms and were on-boarded successfully.

Table 17: Baseline balance between facilities in sample and refusals (Back: 4.1)

Table 18: Primary outcomes regressed on baseline characteristics (Back: 4.1)

	(1) Rapid test uptake	(2) ACT uptake with test	(3) ACT uptake without test
Months on sales management tool	.00143 (0.002)	.00197^{+} (0.001)	-.00104 (0.002)
Below median baseline malaria sales	.194** (0.066)	.0369 (0.034)	-.155* (0.064)
Average monthly malaria sales, 2019-2020	-.000374 (0.001)	-.000687 (0.000)	-.00552* (0.002)
Average monthly ACT sales, 2019-2020	-.00211^{+} (0.001)	-.000573 (0.001)	.00812** (0.003)
Average monthly rapid test sales, 2019-2020	.0157** (0.003)	.0095** (0.002)	-.0119** (0.003)
Site was in earlier pilot study phase	-.00984 (0.052)	-.00811 (0.036)	.0372 (0.055)
Site is in an urban area	.0183 (0.054)	.0105 (0.026)	.0195 (0.054)
Site is in a malaria endemic county	.0729 (0.073)	.0648** (0.024)	-.105 (0.073)
Site does not have clinical capabilities	.673** (0.050)	.224** (0.071)	-.652** (0.046)
% of staff who are female	.147^{+} (0.078)	.0561 (0.039)	-.154* (0.075)
Age of pharmacy owner	.00767** (0.003)	.00261* (0.001)	-.00754* (0.003)
Average age of pharmacy staff	.000397 (0.006)	.000689 (0.002)	-.00217 (0.005)
Female owner	-.202** (0.063)	-.0851* (0.033)	.182** (0.063)
Number of staff	.053 (0.060)	.0383 (0.029)	-.0728 (0.059)
N	51486	51486	51486

Linear probability models for primary outcomes on baseline characteristics
Standard errors are clustered at the facility level
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table 19: Secondary outcomes regressed on baseline characteristics (Back: 4.1)

	(1) Antimalarial uptake overall	(2) ACT uptake overall	(3) ACT uptake w/ test, ACT sales
Months active on sales management tool	.000965 (0.002)	.000923 (0.002)	.00249 (0.002)
Below median baseline malaria sales	-.00931 (0.029)	-.118* (0.051)	.112* (0.053)
Average monthly malaria sales, 2019-2020	-.00297** (0.001)	-.00621** (0.002)	.0000527 (0.001)
Average monthly ACT sales, 2019-2020	.00373** (0.001)	.00754** (0.002)	-.00211+ (0.001)
Average monthly rapid test sales, 2019-2020	.00364* (0.002)	-.00239 (0.002)	.014** (0.003)
Site was in earlier pilot study phase	-.0218 (0.039)	.0291 (0.045)	.000996 (0.050)
Site is in an urban area	-.0373+ (0.022)	.03 (0.040)	.0146 (0.043)
Site is in a malaria endemic county	.114** (0.025)	-.0399 (0.064)	.111* (0.049)
Site is does not have clinical capabilities	.177* (0.080)	-.428** (0.088)	.738** (0.044)
% of staff who are female	-.0224 (0.025)	-.0976+ (0.050)	.115+ (0.061)
Age of pharmacy owner	.00336** (0.001)	-.00493+ (0.003)	.00765* (0.003)
Average age of pharmacy staff	-.00212 (0.002)	-.00148 (0.004)	-.000143 (0.004)
Female owner	.0884* (0.034)	.0974* (0.042)	-.164** (0.054)
Number of staff	.038+ (0.022)	-.0345 (0.041)	.038 (0.047)
N	265610	51486	40261

Linear probability models for secondary outcomes on baseline characteristics
Standard errors are clustered at the facility level
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table 20: Impact on Primary Outcomes, Unadjusted LPM Models (Back: 4.2)

	Rapid test uptake		ACT uptake with test		ACT uptake without test	
	(1)	(2)	(3)	(4)	(5)	(6)
Pooled treatment	.216**		.0691*		-.16*	
	(.052)		(.029)		(.075)	
T1		.234^{+}		.0687		-.167
		(.128)		(.065)		(.136)
T2		.185*		.0703		-.122
		(.082)		(.044)		(.098)
T3		.229**		.0685*		-.182*
		(.060)		(.034)		(.081)
Control mean	.081	.081	.057	.057	.809	.809
F test p-val ($\beta_{T1} \neq \beta_{T2} \neq \beta_{T3}$)		0.882		0.999		0.820
F test p-val ($\beta_{T1} \neq \beta_{T2}$)		0.727		0.982		0.749
N	51486	51486	51486	51486	51486	51486

Standard errors are clustered at the facility level
Controls: Strata FE
F test comparisons of difference (β) between treatment arms
Denominators: all patients that purchased malaria product during study period
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table 21: Impact on Primary Outcomes, Adjusted LPM Models (Back: 4.2)

	Rapid test uptake		ACT uptake with test		ACT uptake without test	
	(1)	(2)	(3)	(4)	(5)	(6)
Pooled treatment	.212**		.0583+		-.188**	
	(.048)		(.030)		(.060)	
Total months active	.00455	.00485	.00182	.00221	-.00642+	-.00642+
	(.003)	(.003)	(.002)	(.002)	(.004)	(.004)
Baseline monthly malaria sales	-.00102*	-.00108+	-.00047	-.000503	.000774	.000808
	(.000)	(.001)	(.000)	(.000)	(.001)	(.001)
Female pharmacy owner	-.0649	-.0692	-.0677*	-.0713*	-.0151	-.0139
	(.058)	(.063)	(.031)	(.034)	(.097)	(.100)
T1		.264*		.0725		-.222+
		(.113)		(.059)		(.119)
T2		.189*		.0673		-.163+
		(.079)		(.048)		(.084)
T3		.194**		.0439		-.182*
		(.061)		(.036)		(.070)
Control mean	.081	.081	.057	.057	.809	.809
F test p-val ($\beta_{T1} \neq \beta_{T2} \neq \beta_{T3}$)		0.824		0.880		0.888
F test p-val ($\beta_{T1} \neq \beta_{T2}$)		0.544		0.937		0.629
N	51486	51486	51486	51486	51486	51486

Standard errors are clustered at the facility level
Controls: months active on platform, baseline malaria sales, female owner, strata FE
F test comparisons of difference (β) between treatment arms
Denominators: all patients that purchased malaria product during study period
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table 22: Impact on Secondary Outcomes, Unadjusted LPM Models (Back: 4.3)

	Antimalarial sales overall		ACT uptake, all malaria product purchases		ACT uptake with test, ACT purchases	
	(1)	(2)	(3)	(4)	(5)	(6)
Pooled treatment	-.0157		-.0905		.108**	
	(.033)		(.068)		(.040)	
T1		.0262		-.0985		.0972
		(.036)		(.090)		(.100)
T2		-.0179		-.0521		.0989
		(.036)		(.079)		(.064)
T3		-.0315		-.114		.12*
		(.037)		(.072)		(.050)
Control mean	.197	.197	.867	.867	.066	.066
F test p-val ($\beta_{T1} \neq \beta_{T2} \neq \beta_{T3}$)		0.160		0.588		0.960
F test p-val ($\beta_{T1} \neq \beta_{T2}$)		0.122		0.550		0.988
N	265610	265610	51486	51486	40261	40261

Standard errors are clustered at the facility level

Controls: Strata FE

F test comparisons of differences (β) between treatment arms

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

Table 23: Impact on Secondary Outcomes, Adjusted LPM Models (Back: 4.3)

	Antimalarial sales overall		ACT uptake, all malaria product purchases		ACT uptake with test, ACT purchases	
	(1)	(2)	(3)	(4)	(5)	(6)
Pooled treatment	-.000747		-.129**		.107**	
	(.026)		(.043)		(.041)	
Total months active	-.000459	-.000606	-.0046	-.00421	.00274	.00299
	(.002)	(.002)	(.003)	(.003)	(.002)	(.003)
Baseline monthly malaria sales	.000443^{+}	.000418^{+}	.000304	.000305	-.000771^{+}	-.000815
	(.000)	(.000)	(.001)	(.001)	(.000)	(.001)
Female pharmacy owner	.0297	.0272	-.0828	-.0853	-.0855	-.0896
	(.046)	(.045)	(.088)	(.089)	(.052)	(.059)
T1		.0263		-.15*		.127
		(.027)		(.071)		(.101)
T2		-.0105		-.0957^{+}		.108
		(.035)		(.054)		(.068)
T3		-.00821		-.138**		.0952^{+}
		(.029)		(.050)		(.050)
Control mean	.197	.197	.867	.867	.066	.066
F test p-val ($\beta_{T1} \neq \beta_{T2} \neq \beta_{T3}$)		0.205		0.653		0.964
F test p-val ($\beta_{T1} \neq \beta_{T2}$)		0.157		0.433		0.856
N	265610	265610	51486	51486	40261	40261

Standard errors are clustered at the facility level

Controls: months active on platform, baseline malaria sales, female owner, strata FE

F test comparisons of difference (β) between treatment arms

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

Table 24: Impact on Primary Outcomes, All baseline covariates (Back: 4.2)

	Rapid test uptake		ACT uptake with test		ACT uptake without test	
	(1)	(2)	(3)	(4)	(5)	(6)
Pooled treatment	.168**		.0231		-.166**	
	(0.039)		(0.026)		(0.040)	
T1		.1*		-.0169		-.0956*
		(0.042)		(0.022)		(0.047)
T2		.184**		.0659^{+}		-.171**
		(0.047)		(0.035)		(0.049)
T3		.184**		.0282		-.191**
		(0.058)		(0.033)		(0.053)
Control mean	0.081	0.081	0.057	0.057	0.809	0.809
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2} \neq \gamma_{T3}$)		0.186		0.018		0.265
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2}$)		0.103		0.006		0.185
N	51441	51441	51486	51486	51441	51441

Reporting marginal effects from the control group, from logistic regressions
Standard errors are clustered at the facility level
Controls: all baseline covariates from Table 1
Wald test comparisons of difference in marginal effects (γ) between treatment arms
Denominator: all patients that purchased malaria product during study period
Outcome 1 & 3: 45 obs dropped b/c multicollinearity (strata 11)
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table 25: Impact on Secondary Outcomes, All baseline covariates (Back: 4.3)

	Antimalarial sales overall		ACT uptake, all malaria product purchases		ACT uptake with test, ACT purchases	
	(1)	(2)	(3)	(4)	(5)	(6)
Pooled treatment	-.016		-.0171		.0541^{+}	
	(0.023)		(0.022)		(0.031)	
T1		-.0201		-.0179		-.00815
		(0.026)		(0.025)		(0.026)
T2		-.0147		-.0142		.105*
		(0.035)		(0.034)		(0.043)
T3		-.0146		-.019		.0722
		(0.028)		(0.028)		(0.044)
Control mean	0.197	0.197	0.175	0.175	0.066	0.066
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2} \neq \gamma_{T3}$)		0.981		0.988		0.010
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2}$)		0.876		0.911		0.007
N	265610	265610	265610	265610	40256	40256

Reporting marginal effects from the control group, from logistic regressions
Standard errors are clustered at the facility level
Controls: all baseline controls from Table 1
Wald test comparisons of difference in marginal effects (γ) between treatment arms
Outcome 3: 50 obs dropped b/c multicollinearity (strata 11)
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table 26: CEA Probability Inputs - sources (Back: 3.4.3)

	SOURCES
P(tested)	Intervention group means from Table 4.3, column 2 for all 4 arms
P(untested)	1 - P(tested)
P(malaria positive | tested)	Control group: administrative data from pharmacies on positivity rates; Treatment group means from Table 4.6, column 1
P(malaria positive | untested)	Control group: lab tech testing random subset of control group patients; Treatment groups: P(malaria positive) from control group (unselected positivity rate); P(positive | tested) from Table 4.6, column 1 P(malaria positive | untested) = P(malaria positive) - P(malaria positive | tested)
P(malaria positive)	P(malaria positive | tested) + P(malaria positive | untested) obtained from lab tech activity in control group
P(ACT | malaria positive & tested)	Control group mean from Table 4.3 column 6 * P(malaria positive | tested); Treatment group means from Table 4.6 column 2
P(ACT | malaria positive & untested)	Intervention group means from Table 4.3 column 6 * P(malaria positive | untested), for all 4 arms
P(malaria negative | untested)	Control group: lab tech testing random subset of control group patients; Treatment groups: P(malaria negative) from control group (unselected positivity rate); P(negative|tested) from Table 4.6, column 1 P(malaria negative | untested) = P(malaria negative) - P(malaria negative | tested)
P(malaria negative | tested)	Control group: administrative data from pharmacies on positivity rates; Treatment group means from Table 4.6 column 1

Table 27: CEA Additional Inputs - sources (Back: 3.4.3)

	SOURCES
Num. patients who purchased ACTs	Intervention group means from Table 4.5 column 4; multiplied by 10000 hypothetical cohort
Incentive unit cost (RDT) ($)	Table B1; transaction completion incentives in T2 & T3 are included
Patients getting incentivized RDTs	Share from Administrative data (positive_rdt); multiplied by 10000 hypothetical cohort
Incentive unit cost (ACT) ($)	Table B1
Patients getting incentivized ACTs	Share from Administrative data (act_purchased); multiplied by 10000 hypothetical cohort
Avg. treatment cost ($), untested	Administrative data (cost_malaria_products if rest_rdt_sales==0)
Num. untested patients	Intervention group means from Table 4.3, column 2; multiplied by 10000 hypothetical cohort
Avg. treatment unit cost ($), tested	Administrative data (cost_malaria_products if rest_rdt_sales==1)
Num. tested patients	Intervention group means from Table 4.3, column 2; multiplied by 10000 hypothetical cohort
Time cost of seeking care	Mean time (mins) spent with provider by treatment arm, from patient survey (s4_a7_prov_treat_min)
Hourly wage ($)	Kenya Continuous Household Survey Program 2020
Num. patients who accessed care	Fixed at 10000 hypothetical cohort across all arms

Table 28: CEA Benefits and Cost Estimates - formulas (Back: 3.4.3)

	FORMULAS
P(ACT | malaria positive)	P(ACT | malaria positive) = P(ACT | malaria positive & tested)P(malaria positive | tested)P(tested) + P(ACT | malaria positive & untested)P(malaria positive | untested)P(untested)
Number of patients taking ACTs	Administrative data (act_sales)
Number of beneficiaries	P(ACT | malaria positive)*Number of beneficiaries
	FORMULAS
Total cost of incentives	(RDT incentive*number of patients getting RDT) +(ACT incentive*number of patients getting incentivized ACT)
Total cost of over-treating malaria negative patients	P(malaria negative | untested)*number of untested patients purchasing antimalarials*cost of antimalarial treatment for untested patients + P(malaria negative | tested)*number of tested patients purchasing antimalarials*cost of antimalarial treatment for tested patients
Total time cost to patients seeking care	Number of malaria patients*average time spent with provider*average hourly wage
Total costs - societal perspective	Total cost of incentives + Total cost of over-treating malaria negative patients + Total time cost to patients seeking care
Total costs - implementer perspective	Total cost of incentives