Using Diagnosis Contingent Incentives to Improve Malaria Treatment*

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Abstract

We examine whether a diagnosis contingent incentive contract structure improves the treatment of malaria, and whether it's best to target those incentives to patients or providers. The contract provides incentives to use rapid tests (RDTs) to diagnose patient malaria status combined with incentives to treat with antimalarial drugs (ACTs) if the patient tests positive but not if negative. Using data from a cluster randomized field experiment with 140 pharmacies in malaria endemic regions of Kenya, we find that both patient subsidies and provider incentives significantly increased RDT testing uptake. Absent incentives, 87% of suspected malaria patients purchase ACTs, of which as many as 66% are doing so unnecessarily because they do not have malaria. The incentives lead to an increase RDT test use by 25 pp, a 7 pp increase in the purchase of ACTs by malaria positive patients, and a 27 pp decline in the purchase of ACTs by malaria negative patients. The contract increases (decreases) ACTs for those who are malaria positive (negative) through both improved diagnostic information and incentives. Diagnosis-contingent contracts are highly cost effective, actually lowering the cost per malaria positive person being treated by reducing the unnecessary treatment of malaria negative patients.

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1 Introduction

Central to the provision of medical care, and a defining feature of health care markets, are the separate but interdependent roles of diagnosis and treatment (Arrow 1963). Patients depend on the judgement of more knowledgeable medical care professionals to diagnose their medical conditions and to recommend appropriate treatments. Appropriate treatment, in turn, depends on an accurate diagnosis, which uses information from laboratory tests, imaging and other assessments that require time, effort and costly technology. That is, diagnosis is a complement to treatment in the production of health care and health. .

Despite the importance of diagnosis, most existing theoretical and empirical models of medical care provider behavior do not specify diagnosis and treatment as separate decisions (see e.g. McGuire 2000 for a review). This leaves a critical gap in understanding supply and demand for health care because patient and provider beliefs about both the need for care and how effectively they can treat a patient depend on the quality of the diagnosis (Chandra, Cutler, and Song 2011).[1] This gap extends to payment and incentive designs that bundle diagnosis and treatment, either explicitly or implicitly, by paying in either some form of fee-for-service (FFS) or fixed payment (e.g. diagnostic related groups or DRGs).

In this paper, we propose and test a novel incentive structure that separately pays for diagnostic effort and for treatment contingent on diagnosis for the case of malaria. Specifically, we use a diagnosis-contingent contract structure that provides incentives to first increase the use rapid diagnostic tests (RDTs) to determine if a patient is malaria positive.[2] Second, the contract provides additional incentives to treat using front-line anti-malarial drugs (Artemisinin Combination Therapies – ACTs) only if the patient tests positive for malaria parasites. The contract encourages appropriate treatment both through generating diagnostic information about illness status (i.e. malaria positive or negative) from the RDT and

1. Novel diagnostic technologies that are both more effective at diagnosis and also match specific individuals to specific treatments — so-called precision medicine — make understanding the diagnostic process of particular interest (Stern, Alexander, and Chandra 2017).

2. RDTs are highly accurate tests that use a finger prick to confirm the presence or absence of malaria parasites in a patient's blood.

through the diagnosis contingent financial incentives for ACT use. This way the incentive contract both encourages appropriate treatment and discourages unnecessary treatment.

We begin by presenting a model of consumer demand for malaria testing, and introduce diagnosis-contingent contracts to demonstrate the mechanisms driving contract design as well as explore the conditions under which we expect such contracts to improve welfare. We show that contracts that reduce the price of ACTs conditional on a positive test result are more cost-effective in boosting testing uptake especially when patients overestimate their probability of being malaria-positive. Those patients are more responsive to the level of diagnosis-contingent ACT discount, inducing greater take-up of ACTs for precisely the population where testing is most valuable. At the same time, because the actual probability of being malaria positive is low, the expected direct costs of the contract are minimal. Finally, our model illustrates how contracts that target the provider's incentives increase uptake. If providers pass through the discounts the results are equivalent to demand. This standard result ignores a key channel: information provision. Provider incentives can induce costly effort to inform patients. Testing contingent contract incentivize providers to counsel patients and to do so accurately by encouraging testing followed by treatment only if a test is positive.

Having developed the theory of diagnosis contingent contracts, we use a cluster randomized field experiment (RCT) to estimate the effect of the diagnosis contingent incentives on malaria testing and treatment decisions. We further investigate whether incentives are more effective when they are given to patients through subsidies (demand-side) or to providers through performance incentives (supply-side), or a combination of the two.

We also examine the mechanisms through which the incentives work by leveraging data from Standardized Patient visits, which allows us to examine behavioral channels of impact without the confounding effects of patient selection. We are, therefore, able to explore whether the effect of the incentives contract on treatment (ACT use) is driven by diagnostic information (i.e. malaria status) versus financial incentives.

Malaria is an important disease to study clinical decision-making 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 (WHO 2021). Despite RDTs being cheap and readily available, less than 10% of patients presenting with malaria symptoms are diagnosed with RDTs or other parasitic tests prior to getting treated.[3] This may explain why large shares of malaria-positive patients go untreated while large shares of malaria-negative patients receive antimalarial medication (Cohen et al. 2013; Cohen, Dupas, and Schaner 2015; O'Meara et al. 2016; Ansah et al. 2010).[4] Low diagnostic testing contributes to a gap between treatment and need; missed diagnoses result in more severe avoidable illness and over-prescription of anti-malaria drugs to malaria-negative patients can lead to heightened drug resistance in the population. In the absence of diagnostic information it is perhaps not surprising that we see large over- and under-treatment for malaria. If providers were aware of their patients' malaria status, treatment could be far better tailored.

We explore these issues in high malaria prevalence counties in Kenya, where over 3.5 million people fall ill with malaria annually. The study population lives near Lake Victoria and on the coast, areas that are most vulnerable to infection (Initiative 2021; Disease Control and Prevention 2018). Over half of malaria patients in Kenya and across East Africa access treatment via pharmacies, often the preferred access point for primary care given pharmacies' convenience and reliable presence even in areas that are under-served by public health care clinics and hospitals (Musuva et al. 2017; Burton et al. 2011).

We randomized 140 pharmacies into either a status quo control group or one of three treatment groups, each with a two-part incentive: (1) patient subsidies for RDT tests and

3. The literature identifies several potential reasons as to why diagnostic testing is low. Patients may not demand tests because of (i) strong prior beliefs about their malaria status – i.e., a low perceived value of information from testing (Maffioli et al. 2019), (ii) the cost of the test is prohibitive (Cohen, Dupas, and Schaner 2015; O'Meara et al. 2018), and (iii) they do not want to wait for the diagnostic test result – i.e., impatience. Moreover, providers may not prescribe a test prior to treatment because (i) they have established practices of symptom-based diagnosis – i.e., established norms and habits (Mbonye et al. 2013), (ii) they are optimizing perceived patient preferences (Lopez, Sautmann, and Schaner 2022), and (iii) they have profit motives (Currie, Lin, and Meng 2014).

4. Over- and under-treatment are ubiquitous worldwide with implications for both health care cost and health outcomes (Das and Hammer 2014; Whitehead, Dahlgren, and Evans 2001).

for the anti-malarial drugs (Artemisinin Combination Therapies – ACTs) conditional on a positive test; (2) pharmacy performance incentives for RDT tests, and for prescribing ACTs conditional on a positive test; and (3) combined incentives (patient subsidies and pharmacy incentives) for RDT and ACTs for confirmed malaria-positive cases. The total value of the incentives was held constant across the three intervention arms. This design allowed us to evaluate the impact of a two-part incentive structure where payouts depend on the full continuum of care (and diagnostic information) as well as to examine the causal effect of targeting that incentive to the patient versus the provider.[5]

We find that both patient subsidies and provider incentives are effective at increasing RDT uptake and at improving targeting treatment to malaria-positive patients. Patient subsidies increase the likelihood that a symptomatic patient takes a RDT test by 27 percentage points over a control group rate of 8 percent.[6] The impact of pharmacy incentives are statistically indistinguishable, increasing the likelihood of RDT uptake by a point estimate of 20 percentage points.

Absent any interventions, 87% of suspected malaria patients purchase ACTs, of which as many as 66% are doing so unnecessarily because they do not have malaria. This represents a high baseline level of medication waste. We find that the incentives lead to an overall decline in ACT usage of 14 percentage points, that incentives increase the likelihood that a patient purchases ACTs combined with a diagnostic test by 7 percentage points, and that incentives lead to large declines (16-22 percentage points) in the likelihood that patients purchase malaria treatment without a diagnostic test.

The increase in ACT purchase is concentrated among those that find out that they are malaria positive from the RDT, while the reduction in ACT use is driven by patients who

5. Prior literature has studied the impact of demand-side subsidies, but not provider incentives, on malaria care, finding them to be effective at improving testing but not at improving test result adherence (Cohen, Dupas, and Schaner 2015; O'Meara et al. 2016; O'Meara et al. 2018). To our knowledge, at the time of writing, one other ongoing study compares patient and provider incentives in pharmacy-settings (Visser et al. 2024).

6. This result is consistent with what has been found in prior literature on consumer subsidies for for RDTs and other health goods.(Dupas 2014; Cohen, Dupas, and Schaner 2015)

find out that they they are malaria-negative from the RDT and elect to forego unnecessary antimalarial purchases. However, the reduction in ACT purchase among those that get tested and find out they are malaria negative is larger in the treatment groups but not in the control group. This suggests that the reduction in ACT purchase is driven by a combination of patient specific diagnosis information and incentives. We also find that diagnosis contingent contracts improve efficiency even for those untested. We see a reduction in ACT purchase among those that do not get tested in the treatment groups, driven by improved information provision from providers who are incentivized not to push a "diagnosis" of malaria positive without confirmation from a test.

We find that patient subsidies result in significantly lower RDT prices (43%) but none of the provider incentives were passed through to clients in terms of lower prices. Instead, provider incentives were associated with pharmacists giving more explanation of RDT results and counseling on treatment based on the test results. Both contracts have the same impact on ultimate demand for both testing and ACT treatments but financial incentives seemed to work through an information and advice pathway when targeted to providers whereas demand subsidies induced more patients to purchase RDTs that provided accurate illness status information leading to more appropriate use of RDTs and ACTs. Both patient and provider incentives led to fewer instances of providers telling patients who chose not to test that they were malaria positive and should purchase ACTs.

The diagnosis contingent incentive contracts are extremely cost-effective, largely due to the fact that they led to substantial reductions in malaria-negative patients taking unnecessary antimalarial drugs. The outcome of interest is the marginal cost of an additional malaria positive patient appropriately treated with ACTs. Costs not only include the direct cost of treatment but also the costs of over-treating malaria negative patients and the time costs for patient seeking care. We find that the patient subsidy and the provider performance incentive interventions are significantly cost-saving, relative to the control group. This means that the diagnosis contingent contracts are actually cost-saving.

This paper relates to a number of different literatures. We directly contribute to work on performance-based financing mechanisms that reward providers for both quantity and quality of health services delivered by paying for key outputs (Basinga et al. 2011; Gertler, Giovagnoli, and Martinez 2014; Ahmed et al. 2023; Yip et al. 2014; Peabody et al. 2014; Witter et al. 2012; Miller and Babiarz 2013). 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. Additionally, the literature on performance incentives focuses on the price effects, but ignores mechanisms through which incentives operate. Our study provides evidence on behavioral channels through which provider performance incentives may impact quality.

Second, we contribute to these two strands of literature by innovating in how health financing contracts are structured. Conditional cash transfers for preventive health visits, for example, incentivize health care utilization by lowering the cost of care to patients. In the US, insurance products that have modest copays or deductibles operate in the same way - by lowering the price patients pay. These examples, as well as other demand-side incentives for healthcare, highlight how these financing models typically operate - they reimburse a flat rate for services used through lower prices/copays (Arrow 1963; Pauly 1980; McGuire 2000; Cutler and Zeckhauser 2000).

Performance pay models that reward providers either directly through bonuses tied to services provided (see e.g.Basinga et al. 2011; Gertler, Giovagnoli, and Martinez 2014; Ahmed et al. 2023; Yip et al. 2014; Peabody et al. 2014; Witter et al. 2012; Miller and Babiarz 2013) for examples) or indirectly through capitation have a similar structure. Payments are typically made based on services provided, and do not explicitly take into account diagnostic information when setting copays for various services.[7] This fits in with other literatures on performance pay based on outcomes, including applications for medical doctors and teachers (Campbell et al. 2009; Prendergast 1999; Podgursky and Springer 2007).Financial incen-

7. Though, of course, we acknowledge that patients with different medical needs will pay different amounts for their care because services needed will be diagnosis-dependent.

tives are well-established tools used around the world to promote a wide range of health behaviors. Typically, demand and supply side incentives are studied independently, in this paper we compare them directly and put the two literatures in conversation with each other. 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 affect demand elasticity (Dupas and Miguel 2017; Dow, White, and Bertozzi 2016; Dupas 2014; Cohen, Dupas, and Schaner 2015; O'Meara et al. 2016).

Third, it adds to the literature on how personalized 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 (Meredith et al. 2013; Ma et al. 2014; Dupas 2011). 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 (Gong 2015). 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 (Cohen, Dupas, and Schaner 2015; O'Meara et al. 2018). 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. We test the extent to which appropriate malaria treatment use is a result of information (RDT result) or an added financial incentive (for ACT), contributing to the long literature on the role of information and information asymmetries in health decision-making (Arrow 1963).

Finally, we contribute to the broader literature studying how incentives targeted at the demand-side or the supply-side can affect prices and demand (e.g. Busse, Silva-Risso, and

Zettelmeyer 2006). We also contribute to this conversation in the health economics literature.

2 Model

In this section we develop a simple model to elucidate the rationale for our diagnosis-contingent contract design. We begin with a model of patient demand for RDT testing. Using the derived demand we characterize our diagnosis contingent contract and explore the impact on demand and outcomes. We demonstrate the efficiency of a diagnosis contingent contract relative to direct subsides (e.g. for RDTs alone). We then turn to the joint decision process between the patient and the provider and explore the role of diagnosis contingent contracts in information provision and treatment decisions.

2.1 Patient demand for RDTs

We allow for two sequential choices, presented in figure 1. First, the patient decides whether or not to to test for malaria. Second, the patient must decide whether to purchase an antimalarial medication. Patient demand depends on malaria status $M \in \{m, m'\}$ with probability of malaria $P(m)$, or some other health condition causing the symptoms with probability $P(m')$, such that $P(m) = 1 - P(m')$. The value of treatment (receiving an ACT) depends on true health status.

The patient's realized utility depends on their true malaria status $M \in \{m, m'\}$ and whether they consume an antimalarial. If the patient is malaria positive and left untreated, they receive a disutility $-d_m$. Similarly, if the patient is malaria negative, the patient has disutility $d_{m'}$ from the non-malaria condition. Receiving unnecessary malaria treatment leads to a disutility represented by $-d_w$, in addition to the unnecessary expenditure on treatment. For instance, side effects from antimalarials, the true underlying condition staying untreated for longer, and malaria resistance concerns can affect the value of d_w.

Patients choose whether to buy an ACT and pay price p_a to receive malaria treatment.

We write expected utility with and without ACT purchase as:

$$\mathbb{E}(U) = \begin{cases} \text{ACT} & -p_a - P(m')(d_{m'} + d_w) \\ \text{No ACT} & -P(m')d_{m'} - P(m)d_m \end{cases} \tag{1}$$

Without the purchase of an RDT, true malaria status M is uncertain. If the patient buys an ACT, they will avoid the dis-utility of having untreated malaria, but they will have a probability $P(m')$ of incurring a dis-utility for inappropriate treatment. If the patient does not buy the ACT, the patient has probability $P(m)$ of incurring disutility d_m from having untreated malaria

We first assume that $\mathbb{E}(U(\text{ACT})) > \mathbb{E}(U(\text{No ACT}))$. That is, for the observed patients, consuming ACT will always be optimal under uncertainty. This assumption implies that the disutility d_m is sufficiently large that a patient with malaria-presenting symptoms will always chose to receive an anti-malarial treatment when their status M is uncertain.[8] Therefore, the patient's value of not testing is given by:

$$\mathbb{E}(U(\text{No RDT})) = -p_a - P(m')(d_{m'} + d_w) \tag{2}$$

Patients, on the other hand, have the option to purchase an RDT test for price p_r which will inform them on their status M, that is, $P(m|\text{RDT positive}) = 1$ and $P(m|\text{RDT negative}) = 0$. We assume that buying an ACT and an RDT is feasible for the patients (i.e. $p_a + p_r <$ B for the patient's healthcare budget B).

We further assume that if a patient knows they are malaria positive after incurring cost p_r, then the patient will purchase an ACT. This follows from the assumptions that patient will purchase an ACT under uncertainty and that they can afford both a RDT and ACT. Both assumptions are consitent with what we observed in the data.[9]. Moreover, since RDTs are assumed to remove the patient's uncertainty on their malaria status, if an RDT is negative,

8. This assumption holds in our sample, since we only observe patients that make a Malaria purchase – that is, if a patient does not purchase an RDT, they purchase an ACT.

9. The probability of buying an ACT if the patient tested positive is $> 95\%$

the patient should not purchase an antimalarial.[10]

We can now write the patient's value of purchasing an RDT as:

$$\mathbb{E}(U(\text{RDT})) = -p_r - P(m)p_a - P(m')d_{m'} \tag{3}$$

Because patients purchase an ACT after buying an RDT if they test positive, the patient's value of RDTs depends on the price of both products. RDTs do, however, guarantee that the patient will not incur disutility d_w, and allows for the possibility of avoiding unnecessary expenditures on antimalarial treatments.

Combining 2 and 3 together, the patient buys an RDT if and only if:

$$\begin{gathered} \mathbb{E}(U(\text{RDT})) > \mathbb{E}(U(\text{No RDT})) \\ \iff -p_r - P(m)p_a - P(m')d_{m'} > -p_a - P(m')(d_{m'} + d_w) \\ \iff \boldsymbol{P(m')(p_a + d_w) > p_r} \end{gathered} \tag{4}$$

Equation 4 describes the patient's optimality condition for the purchase of RDTs. The decision to purchase an RDT depends on 4 values key values: the patient's beliefs about their malaria status, the patient's perceived disutility of incorrectly receiving malaria treatment, and the prices of both RDTs and ACTs. We express demand for RDTs as:

$$D(p_a, p_r) = P(P(m'|i)(p_a + d_{w,i}) - p_r > 0) \tag{5}$$

where $P(m'|i)(p_a + d_{w,i})$ is a random variable reflecting individual $i's$ beliefs about malaria risk and disutility from receiving unnecessary ACT treatment when they are malaria negative. Moreover, since the demand is a cumulative density function, this expression yields simple comparative statistics. Demand is increasing on the price of the ACT and the pa-

10. Again, this is consistent with our data since the probability of buying an ACT after testing negative is 5%.

tient's beliefs about $P(m')$ and d_w, and decreasing on the price of the RDT:

$$\begin{aligned}
\frac{\partial D(p_a, p_r)}{\partial p_a} &> 0 \\
\frac{\partial D(p_a, p_r)}{\partial p_r} &< 0 \\
\frac{\partial D(p_a, p_r)}{\partial P(m'|i)} &> 0 \\
\frac{\partial D(p_a, p_r)}{\partial d_{w,i}} &> 0
\end{aligned}$$

2.2 Patient diagnosis contingent contracts

Our model of demand for diagnosis in hand we turn to contract design. A diagnosis contingent contract simply reduces the price of the ACT that the patient pays conditional on the patient testing and the outcome of that test. We express an ACT with discounted price as $p^*_{a|r} = (1 - \delta_a)p_a < p_a$ available only if the patient purchases an RDT through the contract's program. Parameter δ_a describes the relative size of the discount. The patient can continue to purchase the ACT without an RDT at market price p_a. We extend this by reducing the cost of the RDT test by δ percent. The discounted price for RDTs is given by $p^*_r = (1 - \delta_r)p_r < p_r$ for discount rate δ_r and market price p_r. Under a diagnosis contingent contract, the patient's optimality condition becomes:

$$\begin{aligned}
& p_a - P(m)p^*_{a|r} + P(m')d_w > p^*_r \\
\iff\ & p_a - P(m)(1 - \delta_a)p_a + P(m')d_w > p^*_r \\
\iff\ & (1 - P(m)(1 - \delta_a))p_a + P(m')d_w > (1 - \delta_r)p_r
\end{aligned} \tag{6}$$

When $\delta_r = \delta_a = 0$, this condition is identical to equation 4. When $\delta_r \in [0, 1]$ and

$\delta_a \in [0,1]$, the comparative statistics implied by equation 4 continue to hold. However, patient demand for RDTs is now increasing on the discounts δ_r and δ_a:

$$\frac{\partial D(p_a, p_r, \delta_a, \delta_r)}{\partial \delta_r} > 0$$
$$\frac{\partial D(p_a, p_r, \delta_a, \delta_r)}{\partial \delta_a} > 0$$

Diagnostic contingent contract structures that target prices paid by patients for both testing and treatment based on the outcome of the test, increase demand for malaria testing and preventing unnecessary treatments. When deciding whether to test, patients not only care about the price of the RDT, but also about the cost of treatment. Our proposed contract increases demand for testing through changes in the price of the both products. A lower price for RDTs increases demand for testing. Second, a conditional discount on ACTs indirectly increasing the value that testing by making the expected cost of testing positive lower.

Patient diagnostic contingent contracts lead to more patients receiving optimal treatments, as summarized in figure 1. Contracts improve welfare when beliefs are unbiased about d_w and $P(m')$. Gains are even large when patients underestimate the true value of these parameters since the subsidies will correct under-demand for RCTs (akin to "internalities" (Baicker, Mullainathan, and Schwartzstein 2015)). Baseline demand is consistnet with systematic underestimates for the value of testing based on the large gap between demand for RDTs and clinical guidelines.

An important additional benefit of diagnosis contigent contracts is the reduction in unnecessary malaria treatments for those who are not sick. This directly improves patients' utility by avoiding d_w from receiving unnecessary malaria treatments. The social value is potentially far larger. Unnecessary malaria treatments have the potential to increase malarial resistance in the community imposing an important externality due to under testing.

Such externalities imply a gap between the private value of d_w and social value of preventing malaria resistance.

2.3 Cost-effectiveness of patient diagnosis contingent contracts

A key question is why introducing a diagnosis contingent discount on ACT prices would be preferred over direct RDT price reductions? There are several advantages to conditional discounts on ACTs. Depending on the parameter values for the patient demand, diagnosis contingent discounts on ACTs can prove more cost-effective than direct discounts on RDT prices when the policy goal is to increase RDT uptake. Moreover, alternative policy goals also make the use of diagnosis contingent contracts more attractive.

If the main policy goal is increasing the uptake of RDTs, ACTs price-reductions can be more cost-effective than RDT discounts. To see why this is the case, consider the marginal effect on patient demand from changes to discounted prices for ACTs and RDTs, holding the market price constant. For exposition purposes, assume that all patients have the same perceived probability of being malaria positive $P(m|i) = A$. This leads to:

$$\frac{\partial D(p_a, p_r, p_r^*, p_{a|r}^*)}{\partial p_r^*} = -D'(p_a, p_r, p_r^*, p_{a|r}^*) \tag{7}$$

$$\frac{\partial D(p_a, p_r, p_r^*, p_{a|r}^*)}{\partial p_{a|r}^*} = -P(m|i)D'(p_a, p_r, p_r^*, p_{a|r}^*) = -A * D'(p_a, p_r, p_r^*, p_{a|r}^*) \tag{8}$$

As observed in equations 7 and 8, patients are more responsive to changes in the price of the RDT than changes in the price of the ACT by a factor of $1/A \in [1, \text{inf})$. However, the real cost of the program is influenced by the true probability that a given patient is malaria positive — $P(m)$. For an RDT discount, the per patient program cost will be on expectation $(p_r - p_r^*)D(p_a, p_r, p_r^*, p_{a|r}^*)$, while for a conditional ACT discount the expected cost per patient will be $P(m)(p_a - p_{a|r}^*)D(p_a, p_r, p_r^*, p_{a|r}^*)$. This is because the program only pays for the ACT subsidy when a patient is malaria positive. Hence, if the true probability of being malaria

positive is sufficiently low relative to the patients' beliefs, conditional ACT subsidies can be more cost-effective. To see why, note that as the belief $A \to 1$, the marginal effect of a conditional ACT discount approaches the marginal effect of RDT discounts. However, as $P(m) \to 0$, the expected per patient cost of the conditional ACT discount will approach zero.

In addition to the cost-effectiveness argument presented above, diagnosis contingent contracts for ACTs can be an attractive policy tool for social planners that want to increase the affordability of malaria treatments while avoiding unnecessary medical expenditures. This is particularly relevant in LMICs where the status quo often includes unconditional subsidies for ACTs. Transitioning from unconditional ACT discounts to diagnosis contingent contracts can prove a useful strategy to achieve these policy objectives.

2.4 The role of the provider's counseling on RDT demand

Providers counsel malaria suspect patients on the value of testing. For simplicity, assume that providers can signal the value of testing to the patient through $\theta \in \{0, 1\}$, whether the provider recommends to be tested or not. Patients update their beliefs about the true value of $P(m)$ and d_w based on the provider's counseling, with functions given by $d_{w,i}(\theta)$ and $P(m|\theta, i)$.

If a provider recommends a test $\theta = 1$, it is likely that the patient will interpret this as a signal that either $P(m')$ or d_w are high, or equivalently, that the value of testing is high. If this is the case, then the provider's recommendation to test increases the demand for RDTs:

$$D(p_a, p_r|\theta = 1) - D(p_a, p_r|\theta = 0) > 0 \tag{9}$$

Provider's motivations to recommend testing are potentially twofold. On one hand, providers care about the patient's welfare and the potential for increasing malaria resistance

with unecessary treatments. On the other hand, providers might care about their financial incentives. For concreteness, let the provider's decision to recommend testing be given by:

$$\theta = 1\{W(d_w, P(m), p_r, p_a, \delta_r, \delta_a) + \lambda f(\mathfrak{m}, t) > e_d\} \tag{10}$$

Such that $W(d_w, P(m), p_r, p_a, \delta_r, \delta_a)$ is a function that represents the provider's internalized patient's welfare and concerns about malaria resistance in their community, and $f(\mathfrak{m}, t)$ represents the provider's financial incentives to recommend testing. The provider's financial incentives are a function of the vector of markups $\mathfrak{m}$ for all the malaria products sold in the pharmacy and a vector of any incentives included in the diagnosis contingent contracts (t). In particular:

$$\begin{aligned} f(\mathfrak{m}, t) &= \mathbb{E}[\pi|\theta = 1] - \mathbb{E}[\pi|\theta = 0] \\ &= \Sigma_k(\mathfrak{m}_k + t_k)(P[k|\theta = 1] - P[k|\theta = 0]) \end{aligned} \tag{11}$$

Such that π are the provider's expected profits from a malaria counseling interaction with the patient. These profits depend on the probability of patients deciding to buy product $k \in \{r, a, a|r\}$ conditional on their advice to the patient. The provider's incentives include direct transfers to the provider from the diagnosis contingent contract, and the markups of the pharmacy for the sale of the distinct available products.

2.5 Provider diagnosis contingent contracts

Provider diagnosis contingent contracts change the financial incentive structure from the sale of the malaria products, encouraging providers to recommend testing. In particular, these contracts increase $\mathfrak{m}_k + t_k$ for $k \in \{r, a|r\}$. Since θ is likely to be positively correlated with the patient's beliefs about the value of testing, the probabilities that patients buy an RDT (r) or an ACT conditional on an RDT sale ($a|r$) should both be increasing on θ. In other

words, $P[r|\theta = 1] - P[r|\theta = 0] > 0$ and $P[\{a|r\}|\theta = 1] - P[\{a|r\}|\theta = 0] > 0$. Therefore, provider diagnostic contingent contracts increase the provider's incentives to recommend testing $f(\mathfrak{m}, t)$, and thus, potentially increase demand for testing by the patient.

If the patient underestimates the true value of testing, contracts that target provider incentives to recommend RDTs should be welfare improving to the patient when prices are held constant. Moreover, similar to the patient diagnostic contingent contracts, we expect the provider counterparts to increase social welfare far and beyond the patient's welfare if the private value of testing does not fully incorporate the social cost of preventing malaria resistance in the community.

3 Experimental Design

The study randomized 140 pharmacies into 4 groups – 3 intervention groups and a control group. The three treatment arms are (Appendix Table C1):

1. Patient subsidy group (T1): Clients who seek care for suspected malaria cases at these pharmacies pay a subsidized price for RDTs (90% subsidy, a 10 Kes copay) and a subsidized price for ACTs (80% subsidy, a 30 Kes copay) conditional on a confirmed positive malaria diagnosis. The prices are advertised in large posters in prominent spots in the pharmacy.

2. Pharmacy incentive group (T2): Pharmacy owners receive an incentive to sell RDTs (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. Pharmacies are free to set prices charged to patients.

3. Combined group (T3): Clients are eligible for discounted rapid tests (60% subsidy, a 40 Kes copay) and discounted ACTs 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. Pharmacies are free to set prices charged to patients.

The of the total value of the incentive was held fixed at 200 Kes (~$2 USD in 2021 exchange rates) across all treatment arms.[11]

The pharmacies participating in the study are existing users of Maisha Meds's digital sales management platform. Maisha Meds is a Kisumu-based healthcare social enterprise that provides sales and inventory management support to small pharmacies and clinics throughout Kenya. The platform records all pharmacy transactions and product stock. The incentive interventions were integrated into Maisha Meds's digital platform and managed centrally by the Maisha team. Subsidy and incentive amounts were 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.

Pharmacy staff received training on the importance of diagnostic testing (all arms), proper RDT administration, and use of the malaria case management tool. Stocks of RDTs and ACTs were provided on consignment through Maisha Meds in the intervention arms, while in the control group they managed their own stock.

3.1 Sample Enrollment

The sample consists of for-profit pharmacies and the clients that present with malaria symptoms located in the thirteen counties in the malaria endemic and epidemic areas of Kenya's western regions. These pharmacies manage their own stock and sales of diagnostic tests and medications. They set their own prices and sell at market prices.

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

To be eligible to participate in the study, pharmacies needed to be part of the Maisha Meds network and active users of the Maisha Meds digital sales and inventory management platform. Additionally, they had to be licensed pharmacies that were registered with Kenya's Pharmacy and Poisons Board. They also had to be willing to be randomized to one of the study arms, 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.

All eligible pharmacies were mapped. Those located at least 0.5 km from other potential study participants were invited to participate.[12] Using these criteria 175 pharmacies were identify as eligible and were invited to participate in the study, of which 140 accepted.[13] These 140 pharmacies were randomly assigned to one of the four arms in waves, stratified on average monthly malaria product sales volumes (above/below median), urban/rural, and location of pharmacy in lake endemic county. Figure 4 shows the geographic span of the experiment across the target regions in Kenya and the final selection of pharmacies.

3.2 Data

See Appendix Table C2 for study timeline and a description of the primary sources of data. The study was initially planned to begin in June 2020, but was delayed due to COVID-19. The pharmacy onboarding, patient exit survey, standardized patient visits, and control group testing activities were all done in person following appropriate COVID-19 precautions.[14] The pharmacy baseline surveys were conducted over the phone.

We use the following data sources for analysis:

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

13. Appendix Table C3 reports balance on baseline variables between pharmacies accepted (in sample) and those that declined (refusals). Facilities that declined to participate had been using the digital sales platform for longer than facilities in the sample frame. No other meaningful imbalances were found.

14. 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.

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 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.[15]

   (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.

3. *Standardized Patient Survey:* We employ standardized mystery patients (SP) to measure the appropriateness of the care delivered using the same clinical case scenario. We trained individuals (SPs) to present an identical standardized illness case scenario as real walk-in clients to providers. During encounters with providers, SPs portrayed real patients presenting a standardized, pre-scripted acute adult malaria case. The SPs were confirmed to be malaria-negative based on malaria microscopy tests administered by a reliable, high-quality laboratory before and after the month of field work. SPs and field work supervisors also monitored any potential symptoms throughout field work;

15. Prices observed in the data are retail prices set by pharmacists in the digital tool.

all were otherwise healthy. By using trained SPs portraying the same illness case to generate the care data, we avoid bias from selection on patient illness type and severity that is inherent in care data collected using other common methods such as patient exit interviews, direct clinical observation, or health records (Peabody et al. 2000).

4. *Patient exit survey data:* survey with a random sample of 1654 eligible adult pharmacy clients across all study sites (12.6 clients/site).[16] 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.

5. *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.

6. *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.

16. 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.

3.3 Estimation Methods

All primary analyses are conducted at the patient level.[17] For all binary outcomes, we report marginal treatment effects from adjusted logistic regression models using the following specification:

$$Pr(Y_{ip}) = expit(\beta_0 + \beta_1 T_{1ip} + \beta_2 T_{2ip} + \beta_3 T_{3ip} + \lambda_s + \boldsymbol{X}_p + \epsilon_{ip}) \tag{12}$$

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. We include variables that had significant imbalance with the control group at the $\leq 10\%$ level at baseline (Table 1) as covariates in this adjusted model ($\boldsymbol{X_p}$), as specified in the pre-analysis plan. The β terms represent the log-odds of the treatment effect of each intervention relative to the control group, as percentage point changes. However, we report all results in terms of marginal treatment effects and p-values from Wald tests comparing the marginal treatment effect coefficients of the interventions to each other. Results of unadjusted models (excluding $\boldsymbol{X_p}$) are consistent with findings from the adjusted models, and can be made available upon request.

4 Sample Balance Across Study Arms

Table 1 reports the experimental balance checks at baseline and shows that randomization was balanced across a large set of pre-specified covariates. Out of 84 tests conducted, 8 are significant at the ≤ 10 percent level. When we conduct a joint test for orthogonality using a multinomial logit model with treatment assignment as the categorical outcome, we find that the χ^2-test produces a p-value of 0.46. This suggests that these covariates are not jointly

17. The analyses specified in this section were pre-registered in a pre-analysis plan (AEARCTR-0004705). We discuss any deviations from the pre-analysis plan where relevant.

predictive of group assignment. In the adjusted models, we control for covariates that were unbalanced at baseline from comparisons with the control group.

5 Results

5.1 RDT Use

Table 2 reports the estimated effects of the incentive interventions on RDT use. As a reference point, only 8% of patients who sought care for malaria-related symptoms in control group pharmacies purchased a rapid diagnostic test prior to obtaining treatment, which is consistent with trends found across the full pharmacy sample prior to the start of the experiment (Appendix Figure C1) as well as with other existing research on rapid diagnostic test use in pharmacy settings across East Africa.[18] Overall, the incentives increased RDT use substantially. Patients who sought care in treatment pharmacies across all intervention arms were 25 percentage points more likely to purchase a diagnostic test (column 1, Table 2). Looking at each incentive arm separately in column 2 we find large and statistically significant effects in all three arms; patient discounts resulted in a 27 percentage point increase in RDT uptake, while both the pharmacy incentives and the combination of patient discounts and pharmacy incentives resulted in a 20 percentage point increase. However, the differences across arms were not statistically significant from each other. Pharmacy-administered incentives to either patients or providers lead to more people being tested for malaria prior to receiving treatment.

5.2 ACT Use

Tables 3 and 4 present results of incentives on overall ACT use and by test result, respectively. The estimated impacts of the incentives on overall ACT use are reported in Table 3. Despite

18. For example, in Cohen et al. 2013; Cohen, Dupas, and Schaner 2015; O'Meara et al. 2016; Ansah et al. 2010.

very limited diagnostic testing in the control group, the vast majority of these patients who sought care for suspected malaria purchase ACTs (87%). Based on a malaria positivity rate of 34% derived from the random testing exercise done in the control group, 66% of these individuals purchase ACTs unnecessarily, suggesting potentially large levels of medication waste. Overall we find that the incentives caused a statistically significant decrease of 14 percentage points in ACT purchase (column 1), and between 9-15 percentage point decline when looking at each incentive intervention separately (column 2). Again, the three arms are not statistically distinguishable from each other.

This overall decline in ACT use is ambiguous without analyzing ACT use with and without an accompanying diagnostic test. The rest of Table 3 answers the question: how did changes in testing drive the the overall effect of the incentives on ACT uptake? First, only 6% of the 87% of the patients who purchased ACTs in the control groups also used an RDT. However, as reported earlier, the incentive interventions increased testing substantially so that patients learned their malaria status with certainty and got access to discounted ACTs only if they tested positive. Columns 3 and 4 in Table 3 report the estimated treatment effects when the dependent variable is redefined as ACT use combined with an RDT test. Across the three intervention arms, we find a 7 percentage point increase in the share of patients who purchased ACTs with an accompanying diagnostic test and is consistent with the overall positive effect of the incentives on RDT testing reported in Table 2. Many of these newly tested patients learn that they are malaria positive, triggering discounts for ACT purchase.

Columns 5 and 6 in Table 3 report the estimates when the dependent variable is ACT use without an RDT. We find an average treatment effect of a 20 percentage point reduction in ACT uptake without a diagnostic test, with the patient discount group having a -22 percentage point treatment effect, the pharmacy incentive group having a -16 percentage point treatment effect, and the combination of the patient discounts and pharmacy incentives having a -18 percentage point treatment effect (again, no statistically significant difference

across the three arms). This decrease is driven by the reduction in the number of patients not getting tested and not getting access to the discounted ACT prices.

How much of the effects on ACT uptake are due to the information provided by a diagnostic test vs. the ACT subsidy? Table 4 reports the results of a mediation analysis asks how much of the treatment effect is mediated by testing and the results of those tests. Since we do not have individual level test outcome data for the full sample but do have test positivity and negativity rates at the pharmacy level, we aggregate the data and conduct the analyses at the pharmacy level. Column 1 reports test positivity rates by arm - for the control group, test positivity was obtained by an independent random testing exercise (unconditional test results) and administrative pharmacy data, and in the treatment groups positivity rates are directly observed for individuals who opt into taking an incentivized test. We find differences in test positivity by treatment arm, suggesting that there is patient selection into incentivized care that is different by arm. As a result, these analyses are a descriptive decomposition of the treatment effects and not causal estimates. However, they provide insights into how much of the treatment effect is explained (a) information about one's own malaria status, and (b) an ACT subsidy/incentive on ACT uptake outcomes. Columns 2 and 3 of Table 4 reproduce the main effects on ACT uptake, but do so at the pharmacy-level. Incentive interventions reduce the share of ACTs sold by 24 percentage points on average (column 1), with patient discounts reducing by 27 percentage points and pharmacy incentives reducing by 22 percentage points (column 2), consistent with the individual-level findings.

The rest of the table explores how much of this overall effect is due to information provided by the RDT versus the conditional financial incentive on ACTs. In columns 4 and 5, we see that conditional on the information provided by the diagnostic test (positivity and negativity rates), the overall impact on ACT shares is diminished to about a 6 percentage point decrease overall, and to 4 and 8 percentage point decrease for patient and provider incentives, respectively. Test negativity rate has a large negative impact on ACT sales, on average across treatment groups. Columns 6 and 7 interact positivity and negativity rates

with treatment status, to explore how information affects outcomes by treatment arm. We find that the effect of information on ACT shares is entirely driven by the information effect in pharmacies that received incentive interventions (either patient or provider). So, the information provided by RDTs as a result of the incentive interventions is *only being acted upon* for ACT uptake if the information is combined with ACT price incentives.

5.3 Mechanisms

We investigate mechanisms using data from an audit study that employs standardized patients (SP) to measure the content of the care visit using the same clinical case scenario, with results in Tables 5-7.[19]. SPs have an advantage over client exit surveys or administrative transaction data in our setting as they avoid bias from selection on patient malaria status (real or perceived). Given that we find differential test positivity rates in the sample of patients who opt into treatment (TOT) by arm, this suggests that there are different behavioral mechanisms by arm that explain the overall results. The SP data, by capturing data on an unselected patient sample - where the only variation is by the experimental design - allows us for cleaner identification of mechanisms.

We trained individuals (SPs) to present an identical standardized illness case scenario as real walk-in clients to providers. 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. SPs conducted a total of 411 visits across 137 facilities in the study sample, with three different SPs visiting each facility. SP visits provided a unique opportunity to assess the implementation fidelity and quality of care of the patient-provider interaction at study pharmacies.

19. SPs have been used to measure quality of care extensively. For example see: Peabody et al. 2000; Das et al. 2012; Das et al. 2016; Mohanan et al. 2015; Kwan et al. 2018; Kwan et al. 2019; Kwan 2022; Das et al. 2022; Boone et al. 2023

Table 5 reports intervention effects on patient prices, as reported in exit surveys. We find an 18% price reduction for RDTs, across all intervention arms (column 1). In column 2 of Table 5, we see that this price pass-through is driven by the patient discount arm, but not in the supply-side incentive arms. The discount was reflected in a 43% price reduction for patients when the incentive was administered as a consumer subsidy (which implies a price elasticity of demand of 7.86).[20] This suggests that in the patient discount arm, the increase in testing uptake and improvements in treatment targeting can be explained by reduced patient prices on rapid diagnostic tests. As evidenced by columns 3-6 of this table, we do not find any effects on pharmacists offering an RDT to SPs (or clients, as reported through patient exit surveys) as a result of the interventions. Pharmacists in the control group suggest an RDT to their clients 53%-63% of the time - much higher rates than what we find in RDT purchases in the control group from the transaction data. This suggests that absent any interventions, there is a gap between translating suggestion (know) to action (do).

But, this price mechanism does not appear to explain why we find similar effects in the two supply-side incentive arms. Tables 6 and 7 present results on the pharmacist-patient interaction using data collected from the SP exit surveys. Table 6 discusses results from the SP sample on diagnostic testing. We find no impact on the likelihood that the SP took a malaria diagnostic test (Columns 1 and 2), but the likelihood that SPs who went to control group pharmacies took a test was already quite high (55%). This is much higher than the full sample, and is likely due to the fact that the SPs were instructed to present generalized symptoms and ask the pharmacist for their recommendation, rather than begin by demanding antimalarials, which is also common practice in these settings. Twenty percent of the SP sample reported receiving a positive malaria test result, with no differences across arms (Columns 3 and 4). All SPs were confirmed malaria-negative prior to beginning field work and monitored during the data collection activity, so these were either true false positives or provider mis-reports. Given that RDTs have a 94% specificity, the majority of these

20. Using 338% change in quantity, calculated from point estimates in Table 2

appear to be mis-reported positive results (Mfuh et al. 2019; Wanja et al. 2016). When we restrict the sample to the SPs who were tested with an RDT (columns 5 and 6), we find a 36% test positivity rate (false positives) in the control group, and noisy evidence that the interventions reduced this. Estimates are large (6-11 percentage point decreases in test positivity) and noisy (so, are not statistically significant), but suggest that pharmacists may have been less likely to mis-report malaria positive tests in the intervention arms.

Table 7 shows results from SP data on pharmacist advice and counseling behavior. We first look at whether the SP reported that the pharmacist advised that they had malaria at some point during their visit and suggested that they purchase an ACT, based on either their symptom presentation or a diagnostic test result (columns 1-3). We first look at each intervention separately, then compare the patient incentives to the pooled provider arms, and then look at the pooled (any) intervention. In the control group, 46% of SPs reported that their provider told them that they had malaria and advised an ACT (regardless of whether or not this was confirmed with a diagnostic test). Incentive interventions reduce this likelihood by 11 percentage points on average, and by 5-15 percentage points when looking at each arm separately. This reduction is primarily driven by the untested group of SPs, suggesting that providers in intervention arms are less likely to engage in moral hazard by advising the patient that they suspect malaria. We then look at SP reports of whether the pharmacist comprehensively explained their test result and treatment regimens, a measure of quality of counseling (columns 4-6). We find that only 31% of SPs in control group sites report receiving comprehensive information about tests and/or treatment options, and the incentives significantly improve this (11 percentage points, on average, from column 6). When looking at the patient and provider incentives separately, we see that the improvements in counseling are driven entirely by the provider-side incentives (columns 4 and 5). When pharmacists are incentivized directly, they are 15 percentage points more likely than control group pharmacists to clearly explain treatment options to SPs (pooling the provider arms, as in column 5, and looking at them separately, as in column 4). This suggests that when

incentivized directly, pharmacists do change their behavior and provide more comprehensive counseling on testing and treatment options to suspected malaria patients.

Taken together, these results suggest that the information/counseling channel, rather than a price pass-through, is likely to explain the supply-side treatment effects we find in Tables 2 - 4.

6 Cost-effectiveness

Finally, we develop a cost-effectiveness framework to quantify the societal costs of over-treatment with antimalarials and benefits of appropriate malaria treatment targeting from an implementer and limited societal perspective.[21] The framework that we 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.

In order to analyze the efficiency of each intervention, we conduct a cost-effectiveness analysis where the measure of interest is the incremental cost per additional patient who is appropriately treated with ACTs (so, is malaria positive). We use standard formulas to calculate the ratio of the change in benefits to the change in costs across each intervention arm compared to the status quo. Benefits are defined as the change in patients taking ACTs appropriately (patients must be malaria positive), and costs are defined as the sum of the incentive costs, patient out of pocket costs for tests and treatment, patient time costs of care-seeking, and the direct costs of over-treating malaria negative patients.

The final cost-benefit ratio formula used is below:

$$\frac{Beneficiaries_t - Beneficiaries_c}{TotalCost_t - TotalCost_c}$$

where $t \in (1, 2, 3)$ denotes each treatment arm and c denotes the control group (status quo). The term *Beneficiaries* represents the total number of patients who take ACTs ap-

21. For details on the different perspectives one can take in a CEA, see Kim et al. 2020 as an example.

propriately, and the term *Total Cost* represents the intervention costs, patient out of pocket costs for tests and treatments, patient time costs of care-seeking, and the direct costs of over-treating malaria negative patients. We estimate incremental cost-effectiveness ratios (ICERs) from the perspective of the program implementer (including only program costs) and from a limited societal perspective (including program costs, costs incurred by patients for tests and treatments, time costs of care-seeking, and direct costs of over-treating malaria negative individuals). Full details on the cost-effectiveness analysis, including all formulas, assumptions, and data sources for each parameter, can be found in Appendix 7.

Table 8 presents the incremental benefits and ICERs from the implementer perspective (top panel) and from the limited societal perspective (bottom panel). Within each panel, we present incremental gains and ICERs for each intervention (patient subsidies, pharmacy incentives, or combined) relative to the control group, as this is the most policy-relevant benchmark when deciding amongst these possible intervention approaches. The control group resulted in 73 appropriately targeted ACTs. Patient subsidies resulted in 180 additional appropriately targeted ACTs at a cost of $14.30/patient, pharmacy incentives resulted in an additional 258 patients treated appropriately at a cost of $12.87/patient, and the combined approach resulted in an additional 165 patients treated appropriately at a cost of $22.72/patient (all from Panel A, Table 8).

From a limited societal perspective (Panel B, Table 8), we find that patient subsidies result in an additional 180 patients treated appropriately with ACTs at a cost of -$57.60/patient compared to the control group, which is cost-saving. We find that pharmacy incentives are even more cost saving: compared to the control group, this intervention leads to 258 additional patients treated appropriately with ACTs at a cost of -$142.14/patient. And finally, the combined intervention leads to an additional 165 ACTs targeted appropriately compared to the control group, at a cost of $35.92/patient. These cost-effectiveness estimates may understate the true benefits of these interventions because they do not incorporate the benefits incurred by malaria negative patients foregoing unnecessary antimalarials,

and thus not contributing to increased likelihood of drug-resistant mosquito strains, which are a social cost.

7 Discussion

This paper examines the effects of a novel diagnosis-contingent contract structure to improve malaria case management in a cluster-randomized control trial in Kenya. The experimental treatments provided financial incentives to patients, pharmacists, or both for RDTs and ACTs conditional on testing positive for malaria and were implemented in private sector pharmacies in thirteen malaria-prone counties. By tying financial incentives for treatment to diagnostic outcome, we propose a flexible innovation in how payment contracts for health services could be structured to emphasize quality of care rather than service volume.

This paper contributes to the literature on performance-based financing mechanisms by examining the behavioral channels through which provider incentives impact healthcare quality. It also innovates in health financing contracts, proposing differential payment structures based on diagnostic information. Additionally, it explores how combination of personalized health information and financial incentives influence health behavior. And finally, it adds to our understanding of how incentives targeted at the demand-side or the supply-side can affect decision-making.

We find encouraging results of the demand- and supply-side incentives on both testing and treatment targeting. Overall, the incentives interventions increased RDT use substantially in a setting with very low baseline testing levels. On average, patients who sought care in treatment pharmacies were 25 percentage points more likely to receive a formal malaria diagnosis prior to purchasing treatment for suspected malaria. This represents a more than 300% increase over the control group. Incentive interventions were also effective encouraging appropriate use of antimalarials. We find an overall 14 percentage point decrease in the use of ACTs as a result of the treatment, and this is due to malaria negative patients opting

out of purchasing unnecessary antimalarials. For patients who test positive, we find that they are appropriately nudged to take ACTs, consistent with their diagnostic test result. Interestingly, we find statistically indistinguishable effects of the demand-side and supply-side treatment arms, suggesting that incentives yield similar outcomes whether they are provided directly to patients or they are provided to pharmacists.

We explore mechanisms through which the incentive interventions worked in order to contextualize the main findings. We find that the patient subsidies for RDTs resulted in significantly lower prices being paid by patients (43% reduction in price). However, we find no evidence of pass-through of the RDT incentive in either of the two supply-side arms, and no evidence of price pass-through on ACT prices in any of the three treatment arms. Instead, we find evidence that in the supply-side incentive arms, pharmacists explained diagnosis and treatment options more comprehensively to their patients. Improved, individualized health information appears to be the channel through which the supply-side incentives resulted in the overall changes in RDT and ACT use seen in the main results. In sum, the demand subsidies induced more patients to purchase RDTs that provided accurate illness status information, which led to more appropriate use of ACTs. And, the supply-side incentives led pharmacists to provide more detailed diagnosis counseling and treatment recommendations, yielding similar overall effects on malaria case management.

Finally, we find that the diagnosis contingent incentive contracts are extremely cost-effective, largely due to the fact that they led to large reductions in malaria-negative patients taking unnecessary antimalarial drugs. Taken together, our results imply that diagnosis-contingent contracts may have the potential to reduce medical waste and curb spending while better targeting health care resources to areas of proven need.

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Tables

Table 1: Baseline Balance Between Treatment Arms (Back: 4)

	T1	T2	T3	Control mean
Total months active	-1.45	-4.15*	-0.743	10.143
	(1.98)	(2.00)	(1.98)	
Average monthly malaria sales	-6.29	-9.81	-12.8	64.797
	(11.37)	(11.50)	(11.34)	
Average monthly ACT sales	-1.97	-3.55	-7.67	52.845
	(9.98)	(10.10)	(9.96)	
Average monthly RDT sales	2.68	1.1	-0.12	4.304
	(1.79)	(1.81)	(1.78)	
Pharmacy only, no clinic capability	0.105	0.059	0.116	0.714
	(0.09)	(0.10)	(0.09)	
Pct. Female staff	-0.0927	0.0475	-0.00763	0.443
	(0.10)	(0.10)	(0.10)	
Pharmacy owner age	-1.51	-0.356	-1.42	37.429
	(1.79)	(1.81)	(1.79)	
Average age of staff	0.342	0.0757	0.198	29.371
	(1.29)	(1.31)	(1.29)	
Female owner	-.203*	-.214*	-.175+	0.357
	(0.10)	(0.10)	(0.10)	
Number of staff	-0.0347	-0.0433	-0.115	1.543
	(0.12)	(0.12)	(0.12)	
Below median malaria sales	0.0857	-2.59E-16	0.0857	0.371
	(0.12)	(0.12)	(0.12)	
Participated in earlier study phase	0.0286	5.75E-17	-0.0857	0.171
	(0.09)	(0.09)	(0.09)	
Urban	0.0857	.2+	0.143	0.2
	(0.11)	(0.11)	(0.11)	
Malaria endemic area location	-0.114	-0.0857	0.0286	0.886
	(0.09)	(0.09)	(0.09)	

Multinomial logit test for joint orthogonality produces p-value from chi-squared test of 0.46. Linear regression and linear probability models with strata fixed effects, except for in regressions of baseline outcomes used for stratification. + p<0.1, * p<0.05, ** p<0.01

Table 2: Impact on rapid test uptake, adjusted logistic regression (Back: 5.1)

	Rapid test uptake	
	(1)	(2)
Pooled treatment	.25**	
	(0.051)	
Patient discount		.267*
(γ_{T1})		(0.106)
Pharmacy incentive		.194**
(γ_{T2})		(0.065)
Patient discount and		.201**
pharmacy incentive (γ_{T3})		(0.054)
Control mean	0.081	0.081
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2} \neq \gamma_{T3}$)		0.827
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2}$)		0.540
Wald test p-val ($\gamma_{T1} \neq \gamma_{T3}$)		0.606
Wald test p-val ($\gamma_{T2} \neq \gamma_{T3}$)		0.940
N	51441	51441

Standard errors are clustered at the facility level
Controls: months active on platform, baseline malaria sales,
female owner, strata and calendar month FE
Wald test comparisons of difference in marginal effects (γ)
between treatment arms
Denominator is all patients that purchased malaria product during study period
45 obs dropped b/c multicollinearity (strata 11)
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table 3: Impact on ACT uptake and treatment targeting, adjusted logistic regression (Back: 5.2)

	ACT uptake		ACT uptake with test		ACT uptake without test	
	(1)	(2)	(3)	(4)	(5)	(6)
Pooled treatment	-.139**		.0748*		-.197**	
	(0.049)		(0.034)		(0.060)	
Patient discount		-.145*		.072		-.218*
(γ_{T1})		(0.069)		(0.050)		(0.110)
Pharmacy incentive		-.0892^{+}		.0769^{+}		-.161*
(γ_{T2})		(0.050)		(0.045)		(0.075)
Patient discount and		-.136**		.0511^{+}		-.183**
pharmacy incentive (γ_{T3})		(0.047)		(0.029)		(0.068)
Control mean	0.867	0.867	0.057	0.057	0.809	0.809
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2} \neq \gamma_{T3}$)		.602		0.839		0.881
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2}$)		0.433		0.938		0.629
Wald test p-val ($\gamma_{T1} \neq \gamma_{T3}$)		0.904		0.710		0.782
Wald test p-val ($\gamma_{T2} \neq \gamma_{T3}$)		0.394		0.587		0.802
N	51486	51486	51486	51486	51441	51441

Standard errors are clustered at the facility level

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

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

Denominator is all patients that purchased malaria product during study period

Outcome 3: 45 obs dropped b/c multicollinearity (strata 11)

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

Table 4: Impact on ACT sales by test result, pharmacy-level analysis (Back: 5.2)

	Test positivity rate	Share of ACTs sold					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
T1	.113*						
	(0.054)						
T2	.272**						
	(0.054)						
T3	.123*						
	(0.054)						
Pooled treatment		-.235**		-.0643+		.0349	
		(0.054)		(0.038)		(0.047)	
Patient discount			-.27**		-.0377		.0643
			(0.064)		(0.045)		(0.065)
Pharmacy incentives (pooled)			-.217**		-.0762+		.0245
			(0.057)		(0.039)		(0.050)
Positivity rate, tested				-.108	-.0948	.0917	.0986
				(0.091)	(0.092)	(0.733)	(0.736)
Negativity rate, tested				-.756**	-.77**	-.101	-.102
				(0.058)	(0.059)	(0.189)	(0.190)
Pooled × Positivity						-.192	
						(0.736)	
Pooled × Negativity						-.711**	
						(0.196)	
Patient × Positivity							-.166
							(0.760)
Pharmacy × Positivity							-.186
							(0.741)
Patient × Negativity							-.72**
							(0.212)
Pharmacy × Negativity							-.732**
							(0.202)
Control mean	0.072	0.891	0.891	0.891	0.891	0.891	0.891
N	138	132	132	132	132	132	132

Controls: months active on platform, baseline malaria sales, female owner
Wald test comparisons were conducted of difference in marginal effects (γ) between provider & patient arms
Cols 2-7: No significant differences were found between patient and provider arms,
so the p-values for these tests have been omitted from the table.
Analysis at the pharmacy level, outcome in Col 1 is pharmacy-level test positivity rate
outcome in Cols 2-7 is share of ACTs purchased by patients over the study period
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table 5: Evidence of incentive pass-through, RDT decision (Back: 5.3)

	Log Price of rapid test		SP offered RDT		Patient offered RDT, exit surveys	
	(1)	(2)	(3)	(4)	(5)	(6)
Pooled treatment	-.181*		.0203		.047	
	(0.088)		(0.069)		(0.061)	
T1		-.427*		.0205		.0445
		(0.174)		(0.078)		(0.069)
T2		-.0273		-.00128		.0628
		(0.094)		(0.088)		(0.072)
T3		-.0895		.0429		.0254
		(0.095)		(0.086)		(0.077)
Control group mean	2.880	48.952	0.533	0.533	0.628	0.628
Test p-val ($\gamma_{T1} \neq \gamma_{T2} \neq \gamma_{T3}$)		0.089		0.884		0.873
Test p-val ($\gamma_{T1} \neq \gamma_{T2}$)		0.028		0.791		0.768
Test p-val ($\gamma_{T1} \neq \gamma_{T3}$)		0.066		0.781		0.787
Test p-val ($\gamma_{T2} \neq \gamma_{T3}$)		0.548		0.620		0.607
N	137	137	411	411	1654	1635

Standard errors in parentheses

Col 1 & 2: Facility-level analysis; Col 3 & 4: SP-visit-level analysis;

Col 5 & 6: Patient exit survey analysis

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

Col 1 & 2 covariates: facility-level indicators for whether any SP was offered RDT, missing price info

Col 3 & 4 covariates: SP fixed effects

Col 5 & 6 covariates: Enumerator fixed effects

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

Table 6: Evidence from SP visits on diagnostic testing (Back: 5.3)

	SP took malaria test		Positive malaria test result full sample		Positive malaria test result tested sample	
	(1)	(2)	(3)	(4)	(5)	(6)
Pooled treatment	.0309		-.0272		-.0765	
	(0.072)		(0.054)		(0.081)	
T1		.0578		-.0382		-.105
		(0.083)		(0.065)		(0.101)
T2		.0394		-.0113		-.0621
		(0.089)		(0.068)		(0.102)
T3		-.00565		-.0326		-.0644
		(0.089)		(0.070)		(0.109)
Control mean	0.552	0.552	0.200	0.200	0.362	0.362
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2} \neq \gamma_{T3}$)		0.738		0.922		0.901
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2}$)		0.824		0.699		0.686
Wald test p-val ($\gamma_{T1} \neq \gamma_{T3}$)		0.441		0.936		0.716
Wald test p-val ($\gamma_{T2} \neq \gamma_{T3}$)		0.612		0.768		0.984
N	411	411	411	411	238	238

Standard errors are clustered at the facility level

SP fixed effects are included in all models

F test comparisons of difference in effects between treatment arms

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

Table 7: Evidence from SP visits on provider advice and counseling (Back: 5.3)

	Provider Advised That Patient is Malaria Positive			Provider Counseled Patient on Treatment		
	(1)	(2)	(3)	(4)	(5)	(6)
T1	-.145^{+}			.0353		
	(0.085)			(0.069)		
T2	-.0456			.162*		
	(0.082)			(0.069)		
T3	-.145^{+}			.135^{+}		
	(0.085)			(0.069)		
T1		-.145^{+}			.0354	
		(0.084)			(0.069)	
Pooled T2 & T3		-.0943			.149*	
		(0.073)			(0.059)	
Pooled treatment			-.111			.111*
			(0.069)			(0.056)
Control mean	0.457	0.457	0.457	0.314	0.314	0.314
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2} \neq \gamma_{T3}$)	0.381			0.169		
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2}$)	0.229			0.074		
Wald test p-val ($\gamma_{T1} \neq \gamma_{T3}$)	0.997			0.157		
Wald test p-val ($\gamma_{T2} \neq \gamma_{T3}$)	0.236			0.695		
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2\&T3}$)		0.488			0.064	
N	411	411	411	411	411	411

Standard errors are clustered at the facility level

SP fixed effects are included in all models

F test comparisons of difference in effects between treatment arms

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

Table 8: Incremental Benefits and ICERs (Back: 6)

Incremental benefits and ICERs, compared to control group

	Number of malaria positive patients treated with ACTs*	Incremental appropriately treated	ICER Cost / patient appropriately treated
		Implementer perspective	
T1 vs. C	238	180	$14.30
T2 vs. C	253	258	$12.87
T3 vs. C	331	165	$22.72
		Societal perspective	
T1 vs. C	238	180	-$57.60
T2 vs. C	253	258	-$142.14
T3 vs. C	331	165	$35.92

*73 malaria positive patients treated with ACTs in the control group

Figures

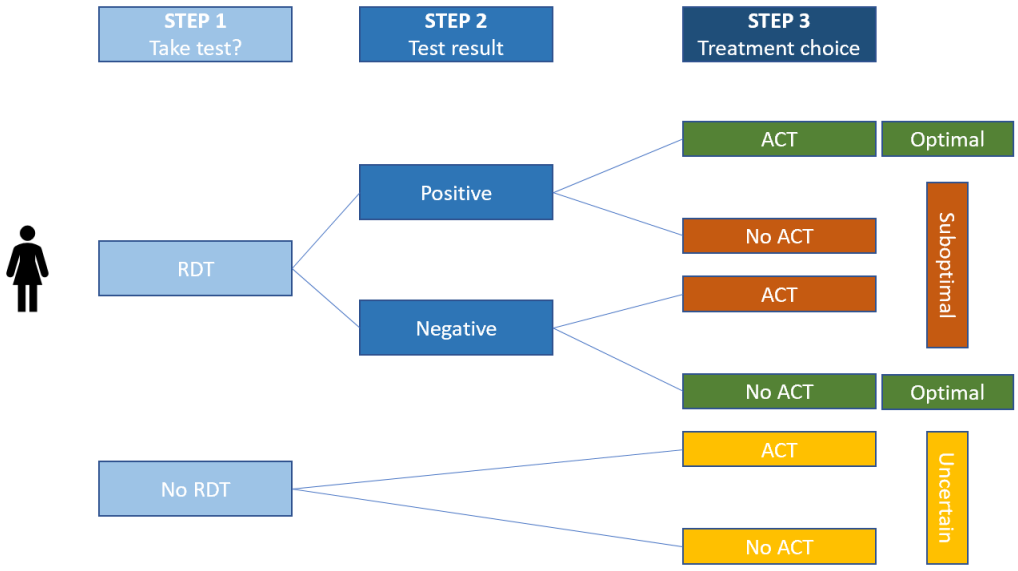

Figure 1: Patient decision to test and treat (Back: 2)

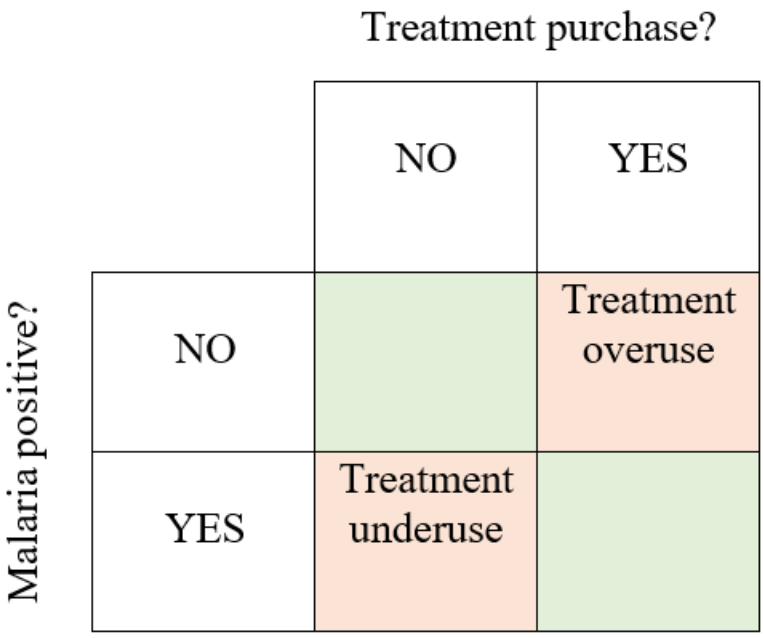

Figure 2: Types of errors (Back: 2)

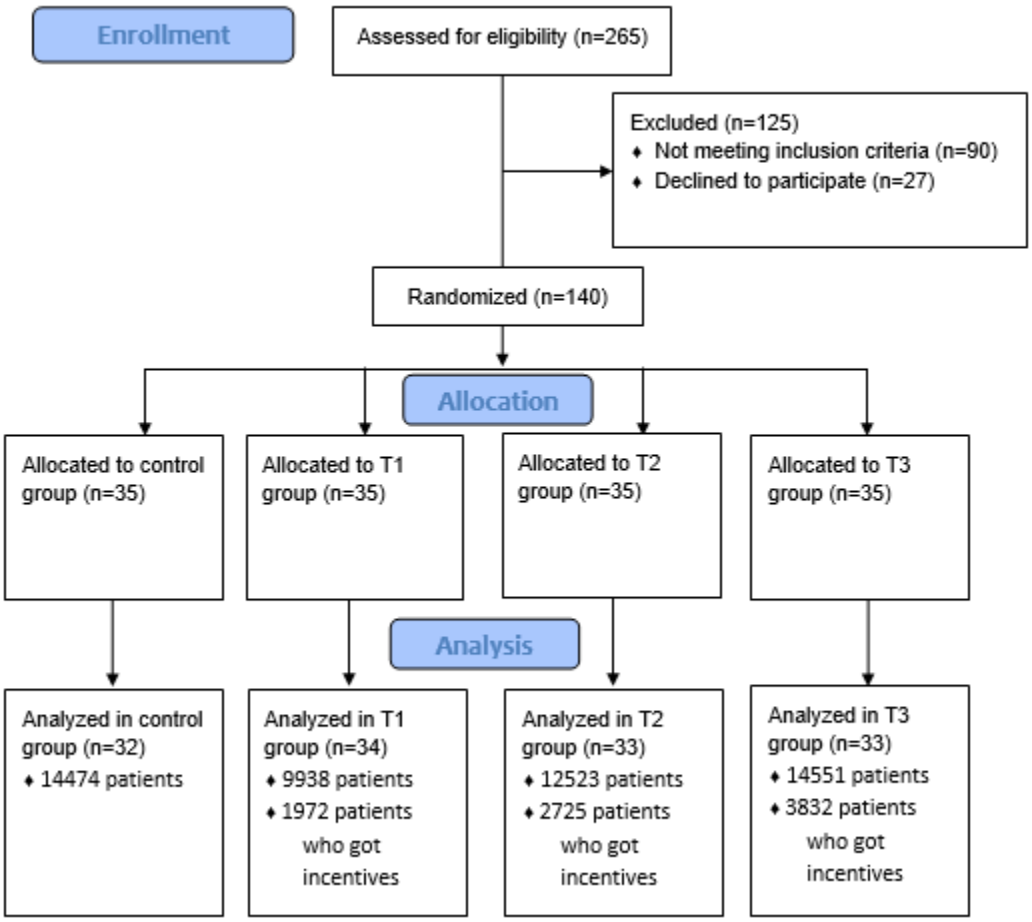

Figure 3: Study flow diagram (Back: 3)

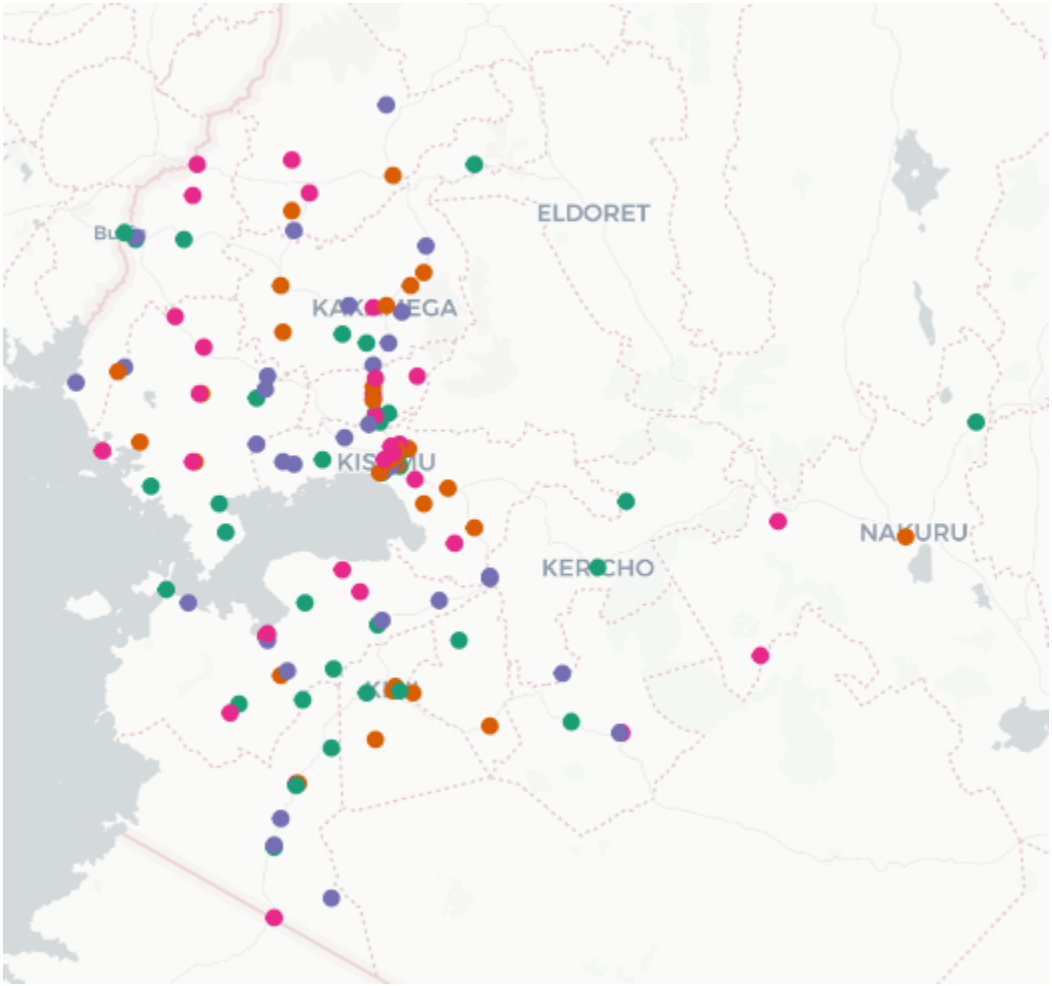

Figure 4: Map of study sites (Back: 3.1)

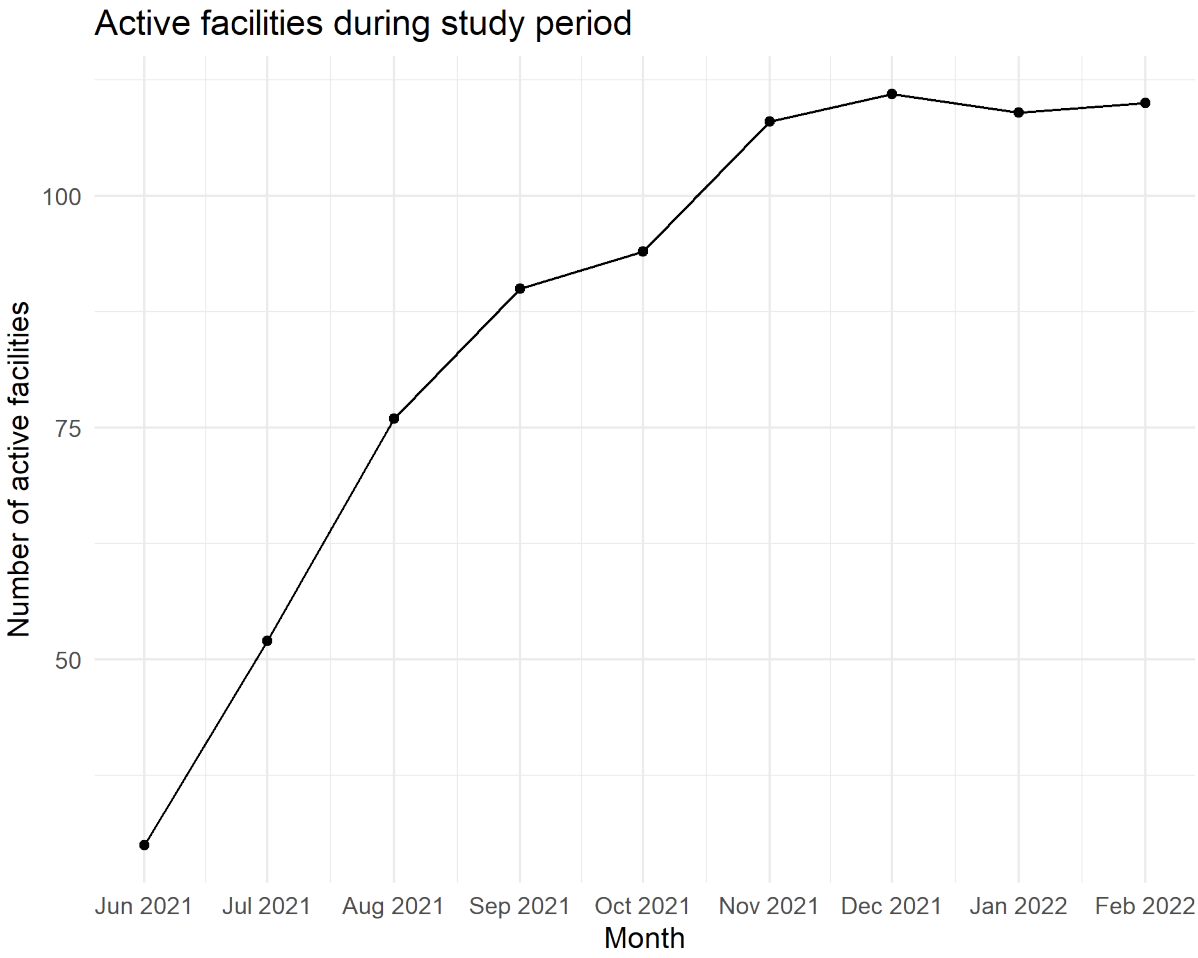

Figure 5: Active facilities during study period (all transactions) (Back: 4)

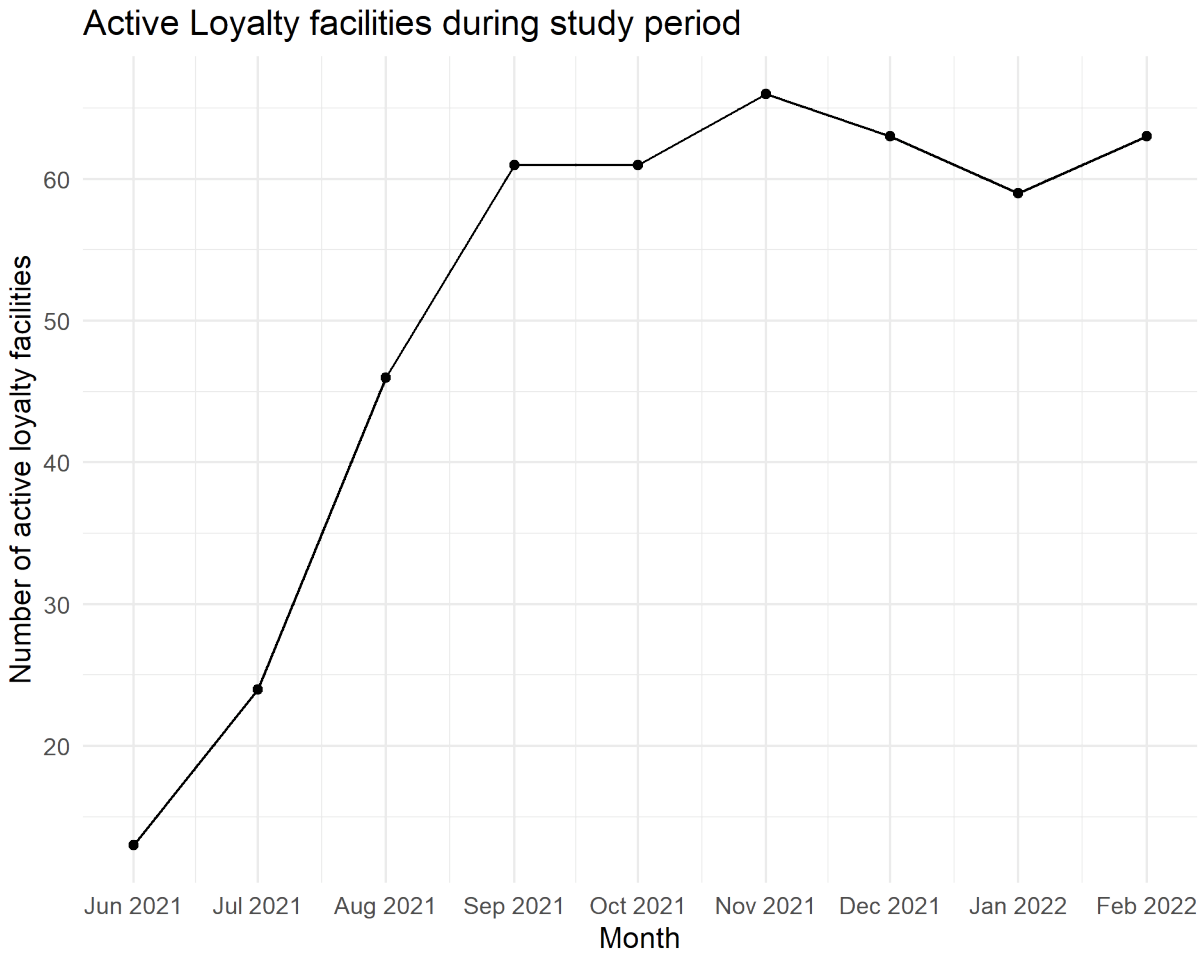

Figure 6: Active treatment facilities during study period (incentivized transactions) (Back: 4)

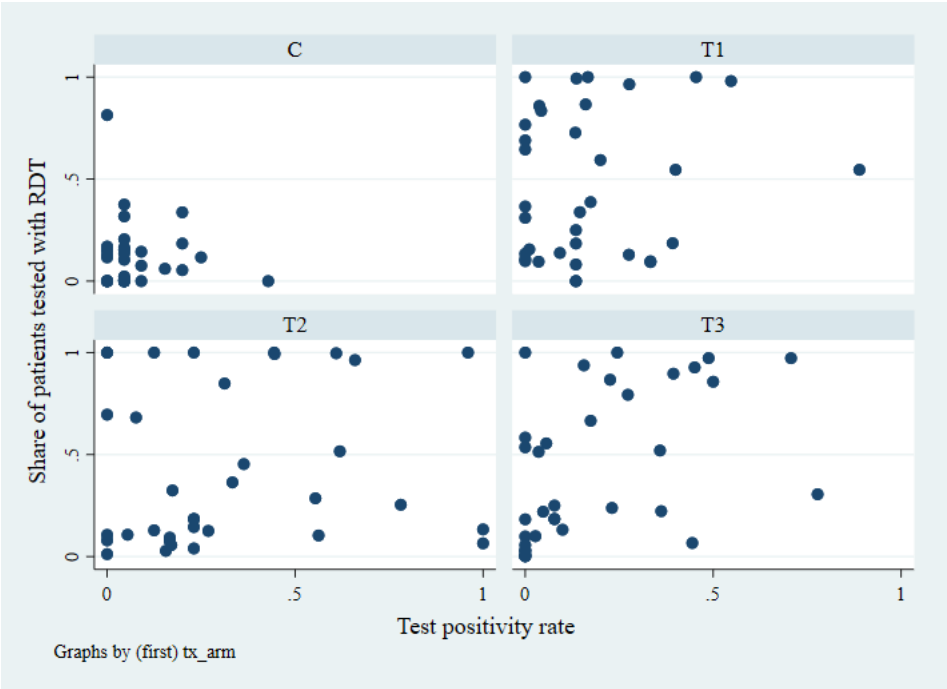

Figure 7: Test positivity rates by share tested, facility-level by treatment arm

Appendix A. Cost-effectiveness Analysis Supplement

7.1 CEA Methods

Benefits are measured as patients who take ACTs appropriately, 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, we 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,[22] 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, for patients accessing incentivized tests and treatments. In the control group, this probability is estimated using the control group mean from column 4 of Table 3 (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. $Pr(tested)$ is directly estimated from the administrative data in all four arms, and is the treatment arm specific mean in column 2 of Table 2. $Pr(ACT|positive\&untested)$ is estimated for all four arms and is the treatment group specific means of $Pr(ACT|untested)$ from column 6 in Table 3 multiplied by $Pr(positive|untested)$. $Pr(positive|untested)$ is estimated in the control group using data collected from the lab tech

22. Obtained from intervention group specific means from Table 3, column 2.

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 overall malaria positivity rate (without selection into testing), and is obtained from the control group testing data ($Pr(positive|tested) + Pr(positive|untested)$), and $Pr(positive|tested)$ is directly obtained from the administrative transaction data for patients who purchased incentivized tests.

The inputs needed to calculate the number of beneficiaries in each intervention arm can be found in Appendix Table A1. We 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 A2 and A3. For details on formulas used to calculate the benefits estimates, please see Appendix Table A4.

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, we 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, we 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 we have estimates for each of these out of pocket costs for each of my intervention arms. Then we also observe the number of untested patients and number of tested patients in each treatment arm, again from the administrative data. We 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. We 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 overall malaria negativity rate (without selection into testing), 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 administrative transaction data from patients who purchased incentivized tests.

Finally, we 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. We 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 Appendix Table A1. For details on the sources of each parameter input for the costs, please see Appendix Tables A2 and A3. For details on formulas used to calculate the cost estimates, please see Appendix Table A4.

7.2 CEA Results

Below are details on calculating the benefits and costs that informed the final ICERs presented in the main text.

7.2.1 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 3.5%, 4.3% and 3.3% in the patient subsidy group (T1), pharmacy incentives group (T2), and the combined group (T3), respectively. The total number of beneficiaries in each arm are 73, 253, 331, and 238 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 A5.

7.2.2 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 $2,574.00, $3,320.00 and $3,748.00 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 combined 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 A5.

For the limited societal perspective, we 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 $374,594, and the societal costs for the intervention arms are $364,226, $337,921, and $380,520 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, the arm-specific malaria test negativity rate, which is highest in the combined arm, and the share of malaria negative patients who purchase antimalarials unnecessarily. These cost estimates can be found in the bottom panel of Table A5.

Table A6 presents the incremental cost of each intervention relative to the next less expensive alternative. From the implementer perspective, the incremental costs are relatively small, since the incentive amounts are modest. The control group (status quo) is the cheapest alternative, and the combined arm is the most expensive. From a limited 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 combined arm is the most expensive from a limited societal perspective, because of the larger time cost to patients seeking care, relative to the control group.

7.3 CEA Tables

Table A1: Cost Effectiveness Analysis Inputs

	Control (status quo)	T1 Patient subsidies	T2 Provider incentives	T3 Hybrid
	PARAMETER INPUTS			
P(tested)	0.081	0.348	0.275	0.282
P(untested)	0.919	0.652	0.725	0.718
P(malaria positive | tested)	0.021	0.1	0.16	0.12
P(malaria positive | untested)	0.106	0.027	-0.033	0.007
P(malaria positive)	0.127	0.127	0.127	0.127
P(ACT | malaria positive & tested)	0.001	0.996	0.956	0.962
P(ACT | malaria positive & untested)	0.086	0.02	-0.02	0
P(malaria negative)	0.873	0.873	0.873	0.873
Share of patients who purchased ACTs	0.867	0.722	0.7778	0.731
Incentive unit cost (RDT) ($)	$0.00	$0.90	$1.20	$1.10
Share of patients purchasing incentivized RDT	0	0.198	0.218	0.263
Incentive unit cost (ACT) ($)	$0.00	$1.10	$0.80	$0.90
Share of patients purchasing incentivized ACT	0	0.072	0.088	0.095
Average antimalarial treatment unit cost ($), untested	$1.92	$2.12	$1.49	$1.99
Share of untested patients (P(untested))	0.919	0.652	0.725	0.718
P(malaria negative | untested)	0.76	0.451	0.592	0.549
Average antimalarial treatment unit cost ($), tested	$4.02	$0.79	$1.98	$1.26
Share of tested patients (P(tested))	0.081	0.348	0.275	0.282
P(malaria negative | tested)	0.113	0.422	0.281	0.324
Time cost of seeking care (minutes)	18.04	17.71	16.33	18.39
Hourly wage ($)	$2.00	$2.00	$2.00	$2.00
Number of patients who accessed care	10000	10000	10000	10000

Table A2: CEA Probability Inputs - sources

	SOURCES
P(tested)	Intervention group means from Table 4, column 2 for all 4 arms
P(untested)	1 - P(tested)
P(malaria positive | tested)	Control group: administrative data from pharmacies on positivity rates (8 sites), positivity rates from random testing activity multiplied by share tested (19 sites); Treatment groups: test positivity rates from administrative transaction data of patients accessing tests through interventions.
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), obtained from administrative transaction data as described above, 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, applied to all groups (base malaria positivity rate)
P(ACT | malaria positive & tested)	Control group mean from Table 5 column 4 * P(malaria positive | tested); Treatment group means from administrative transaction data for patients accessing incentivized tests and treatments
P(ACT | malaria positive & untested)	Group means from Table 5 column 6 * P(malaria positive | untested), for all 4 arms
P(malaria negative | untested)	Calculated directly from P(malaria negative) - P(malaria negative | tested) for all groups
P(malaria negative | tested)	Control group: lab tech testing random subset of control group patients; Treatment groups: P(malaria negative) from control group (unselected negativity rate); P(negative | tested), obtained from administrative transaction data as described above, P(malaria negative | untested) = P(malaria negative) - P(malaria negative | tested)

Table A3: CEA Additional Inputs - sources

	SOURCES
Number of patients who purchased ACTs	Intervention group means from Table 5 column 2; multiplied by 10000 hypothetical cohort
Incentive unit cost (RDT) ($)	Table C1; transaction completion incentives in T2 & T3 are included
Number of patients purchasing incentivized RDTs	Share from Administrative data (positive_rdt); multiplied by 10000 hypothetical cohort
Incentive unit cost (ACT) ($)	Table C1
Number of patients purchasing incentivized ACTs	Share from Administrative data (act_purchased); multiplied by 10000 hypothetical cohort
Average antimalarial treatment unit cost ($), untested	Administrative data (cost_malaria_products if rest_rdt_sales==0)
Number of untested patients	Inervention group means from Table 4, column 2; multiplied by 10000 hypothetical cohort
Average antimalarial treatment unit cost ($), tested	Administrative data (cost_malaria_products if rest_rdt_sales==1)
Number of tested patients	Inervention group means from Table 4, 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
Number of patients who accessed care	Fixed at 10000 hypothetical cohort across all arms

Table A4: CEA Benefits and Cost Estimates - formulas

	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

Table A5: Benefits and Costs Estimates

	Control (status quo)	Patient subsidies	Provider incentives	Hybrid
		BENEFITS		
P(ACT | malaria positive)	0.008	0.035	0.043	0.033
Number of patients taking ACTs	8670	7220	7778	7310
Number of beneficiaries	73	253	331	238
		COSTS		
Total cost of incentives	$0.00	$2,574.00	$3,320.00	$3,748.00
Cost of over-treating malaria negative patients	$13,753.75	$7,392.35	$7,941.13	$9,012.15
Total time cost to patients seeking care	$360,840.00	$354,260.00	$326,660.00	$367,760.00
Total costs - societal perspective	$374,593.75	$364,226.35	$337,921.13	$380,520.15
Total costs - implementer perspective	$0.00	$2,574.00	$3,320.00	$3,748.00

Table A6: Incremental Costs

	Implementer perspective	
	Costs	Inc. cost
Control (status quo)	$0.00	-
TI - Patient subsidies	$2,574.00	$2,574.00
T2 - Provider incentives	$3,320.00	$746.00
T3 - Hybrid	$3,748.00	$428.00
	Societal perspective	
	Costs	Inc. cost
T2 - Provider incentives	$337,921.13	-
T1 - Patient subsidies	$364,226.35	$26,305.22
Control (status quo)	$374,593.75	$10,367.40
T3 - Hybrid	$380,520.15	$5,926.40

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.

Appendix B. Pharmacy-level test positivity rates detail

Table 4 presents pharmacy-level results on treatment purchase conditional on test positivity rates at the site-level. In the intervention arms (T1, T2, T3), test positivity for the tested sample is observed directly from transaction records for patients that tested for malaria using the incentivized rapid tests. In the control group, we do not observe test positivity for individual patients. In the transaction data for all sites, we do observe whether clients purchased a rapid test and what their treatment choice was. From administrative aggregate testing records provided by a subset of control group sites that keep records on malaria positivity rates, we know that 24% of tests came back positive between January - February 2022. We use this test positivity rate, combined with the test positivity rate obtained from an independent random testing exercise of a subset of patients seeking care in control group sites, impute test positivity rates absent any incentives for the control group. We follow a parallel process to obtain pharmacy-level test negativity rates (with the third, omitted, group being the untested sample).

Appendix C. Supplementary Tables and Figures

7.4 Appendix Tables and Figures

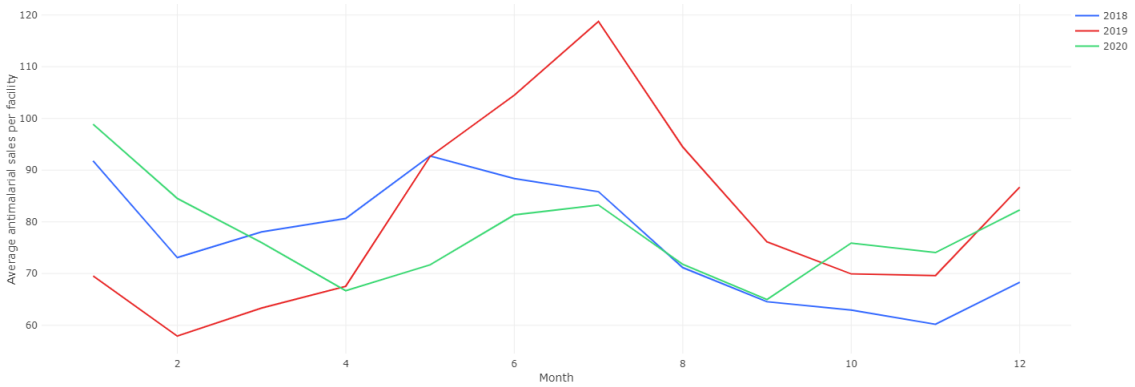

Figure C1: Malaria sales, seasonal trends (Back: 5.1)

Table C1: Incentive amount details, by treatment arm (Back: 3)

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 C2: Study timeline (Back: 3.2)

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**: implementation team monitors intervention implementation 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
Dec '21-Feb '22	**Standardized patient visits**: 412 mystery shopper visits by enumerators presenting as suspected malaria patients, 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

Table C3: Baseline balance between facilities in sample and refusals (Back: 4)

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 C4: Primary outcomes regressed on baseline characteristics (Back: 4)

	(1)	(2)	(3)
	Rapid test uptake	ACT uptake with test	ACT uptake without test
Months	.00143	.00197^{+}	-.00104
on sales management tool	(0.002)	(0.001)	(0.002)
Below median baseline	.194**	.0369	-.155*
malaria sales	(0.066)	(0.034)	(0.064)
Average monthly	-.000374	-.000687	-.00552*
malaria sales, 2019-2020	(0.001)	(0.000)	(0.002)
Average monthly	-.00211^{+}	-.000573	.00812**
ACT sales, 2019-2020	(0.001)	(0.001)	(0.003)
Average monthly	.0157**	.0095**	-.0119**
rapid test sales, 2019-2020	(0.003)	(0.002)	(0.003)
Site was in earlier	-.00984	-.00811	.0372
pilot study phase	(0.052)	(0.036)	(0.055)
Site is in an urban	.0183	.0105	.0195
area	(0.054)	(0.026)	(0.054)
Site is in a malaria	.0729	.0648**	-.105
endemic county	(0.073)	(0.024)	(0.073)
Site does not have	.673**	.224**	-.652**
clinical capabilities	(0.050)	(0.071)	(0.046)
% of staff	.147^{+}	.0561	-.154*
who are female	(0.078)	(0.039)	(0.075)
Age of pharmacy	.00767**	.00261*	-.00754*
owner	(0.003)	(0.001)	(0.003)
Average age of	.000397	.000689	-.00217
pharmacy staff	(0.006)	(0.002)	(0.005)
Female owner	-.202**	-.0851*	.182**
	(0.063)	(0.033)	(0.063)
Number of staff	.053	.0383	-.0728
	(0.060)	(0.029)	(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 C5: Secondary outcomes regressed on baseline characteristics (Back: 4)

	(1) Antimalarial uptake overall	(2) ACT uptake overall	(3) ACT uptake w/ test, ACT sales
Months active on sales management tool	.000965	.000923	.00249
	(0.002)	(0.002)	(0.002)
Below median baseline malaria sales	-.00931	-.118*	.112*
	(0.029)	(0.051)	(0.053)
Average monthly malaria sales, 2019-2020	-.00297**	-.00621**	.0000527
	(0.001)	(0.002)	(0.001)
Average monthly ACT sales, 2019-2020	.00373**	.00754**	-.00211+
	(0.001)	(0.002)	(0.001)
Average monthly rapid test sales, 2019-2020	.00364*	-.00239	.014**
	(0.002)	(0.002)	(0.003)
Site was in earlier pilot study phase	-.0218	.0291	.000996
	(0.039)	(0.045)	(0.050)
Site is in an urban area	-.0373+	.03	.0146
	(0.022)	(0.040)	(0.043)
Site is in a malaria endemic county	.114**	-.0399	.111*
	(0.025)	(0.064)	(0.049)
Site is does not have clinical capabilities	.177*	-.428**	.738**
	(0.080)	(0.088)	(0.044)
% of staff who are female	-.0224	-.0976+	.115+
	(0.025)	(0.050)	(0.061)
Age of pharmacy owner	.00336**	-.00493+	.00765*
	(0.001)	(0.003)	(0.003)
Average age of pharmacy staff	-.00212	-.00148	-.000143
	(0.002)	(0.004)	(0.004)
Female owner	.0884*	.0974*	-.164**
	(0.034)	(0.042)	(0.054)
Number of staff	.038+	-.0345	.038
	(0.022)	(0.041)	(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 C6: Impact on antimalarial uptake, adjusted logistic regression models (Back: 5.2)

	Antimalarial sales overall		Non-ACT sales overall	
	(1)	(2)	(3)	(4)
Pooled treatment	.000427		-.000289	
	(0.023)		(0.003)	
Patient discount		.0259		-.00157
(γ_{T1})		(0.025)		(0.005)
Pharmacy incentive		-.00873		-.00444
(γ_{T2})		(0.032)		(0.004)
Patient discount and		-.00655		.00308
pharmacy incentive (γ_{T3})		(0.027)		(0.005)
Control mean	0.197	0.197	0.022	0.022
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2} \neq \gamma_{T3}$)		0.231		0.426
Wald test p-val ($\gamma_{T1} \neq \gamma_{T2}$)		0.156		0.544
Wald test p-val ($\gamma_{T1} \neq \gamma_{T3}$)		0.202		0.471
Wald test p-val ($\gamma_{T2} \neq \gamma_{T3}$)		0.943		0.200
N	265610	265610	258765	258765

Standard errors are clustered at the facility level

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

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

Denominator is all patients that purchased malaria product during study period

Outcome 3: 45 obs dropped b/c multicollinearity (strata 11)

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