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No. 8976

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DEVELOPMENT ECONOMICS

Centre for Economic Policy Research

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Available online at: www.cepr.org/pubs/dps/DP8976.asp

ISSN 0265-8003

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Discussion Paper No. 8976
May 2012

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CEPR Discussion Paper No. 8976

May 2012

ABSTRACT

Do Public Health Interventions Crowd Out Private Health Investments? Malaria Control Policies in Eritrea*

It is often argued that engaging in indoor residual spraying (IRS) in areas with high coverage of mosquito bed nets may discourage net ownership and use. This is just a case of a public program inducing perverse incentives. We analyze new data from a randomized control trial conducted in Eritrea which surprisingly shows the opposite: IRS encouraged net acquisition and use. Our evidence points to the role of imperfect information. The introduction of IRS may have made the problem of malaria more salient, leading to a change in beliefs about its importance and to an increase in private health investments.

JEL Classification: I10
Keywords: crowding-out, development, health and malaria

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*We would like to thank officials at the Eritrean Ministry of Health and the National Malaria Control Program, and the data collection team at the Orotta School of Medicine in Asmara. Without them this research would not have been possible. This research was funded by a World Bank grant (as part of the HAMSET II project) through the Project Management Unit of the Eritrea Ministry of Health, and a World Bank grant from DIME. We thank Pascaline Dupas, Costas Meghir, Petra Todd, and seminar participants at UCL, EDePo at IFS, the AEL Conference 2011, the 10th Arnoldshain Seminar, the World Bank, PSE and the NEUDC Conference 2011 for helpful comments. Carneiro gratefully acknowledges the financial support from the Economic and Social Research Council for the ESRC Centre for Microdata Methods and Practice (grant reference RES-589-28-0001), the support of the European Research Council through ERC-2009-StG-240910-ROMETA and Orazio Attanasio's ERC-2009 Advanced Grant 249612 "Exiting Long Run Poverty: The Determinants of Asset Accumulation in Developing Countries", and the hospitality of the World Bank Research Group.

Submitted 10 May 2012

Abstract

It is often argued that engaging in indoor residual spraying (IRS) in areas with high coverage of mosquito bed nets may discourage net ownership and use. This is just a case of a public program inducing perverse incentives. We analyze new data from a randomized control trial conducted in Eritrea which surprisingly shows the opposite: IRS encouraged net acquisition and use. Our evidence points to the role of imperfect information. The introduction of IRS may have made the problem of malaria more salient, leading to a change in beliefs about its importance and to an increase in private health investments.

JEL codes: D12, D83, H42, I12.

Keywords: Malaria, Bed nets, Indoor residual spray, Information, Beliefs, Behavior.

Most public programs induce behavioral responses in their target population. These responses are often perverse, making these programs less effective than what was originally intended. For example, the success of public health programs is limited by (among other things) the extent to which they crowd out private health investments. This is a central concern in the design of public interventions across a variety of areas, in rich and poor countries alike. In the particular case of malaria control programs, such as indoor residual spraying (IRS),[1] the introduction of IRS could have a negative impact on the acceptability of insecticide treated mosquito bed nets (ITN), possibly inducing individuals to stop using them (see, e.g., Lengeler (2011)).

In the standard model, the amount of crowding-out depends on the degree of substitutability between private and public investments. However, outside the scope of this simple model are situations where the introduction of a program conveys new information about the returns to private health investments. For example, the introduction of a new health program in a community can be perceived by its members

Grant 249612 "Exiting Long Run Poverty: The Determinants of Asset Accumulation in Developing Countries", and the hospitality of the World Bank Research Group.

[1] IRS consists in spraying the interior walls of dwellings with insecticide to kill resting mosquitoes.

as an indication that (the government knows that) a particular health problem has become more serious in the community, inducing a change in the beliefs about the returns to private health investments (i.e., a program may have an implicit information component even when it does not include an explicit information campaign). In this context, the standard crowd-out intuition breaks down, and an increase in public health investments can lead to an increase in private health investments even when they are substitutes.

Although this is a fairly sensible point, and potentially relevant for most education and health programs in developing countries, it is absent from the discussion on the behavioral responses to such programs. This paper presents experimental evidence from Eritrea that an IRS campaign led to increases in ITN ownership and use. Our analysis suggests that the introduction of IRS may have made the problem of malaria more salient in treatment villages, leading to a change in beliefs about the importance of the disease in these areas, which resulted in an increase in private health investments.

The data used in our study come from an experimental evaluation of the impact of IRS in the most malarious region of Eritrea (Gash Barka), organized by the Government of Eritrea. Fifty-eight (58) villages were randomly assigned to treatment and 58 villages were randomly assigned to control. Between June–July 2009, before the start of the malaria season, households in treatment villages were visited by government workers carrying IRS equipment and were offered free IRS. Households in control villages did not receive publicly provided IRS and IRS is not privately provided in the market. A household survey and rapid diagnostic tests (RDT) were administered during the malaria season that followed (October, 2009).

Our data show that IRS had no detectable impact on (the already very low levels of) malaria parasite infection prevalence (Keating, Locatelli, Gebremichael, Ghebremeskel, Mufunda, Mihreteab, Berhane, and Carneiro (2011)). However, it led to higher ownership and use of ITNs. In addition, households in treatment villages are more aware of (and concerned with) malaria than in control villages. In particular, they are more likely to mention mosquitoes as a malaria vector and to mention children as one of the groups most affected by malaria.

A large literature debates the extent to which a variety of public programs discourages (or crowds-out) private investments in those goods or services which are provided by the public sector. Three examples (among many) are Peltzman (1973), who discusses the case of higher education in the US, Cutler and Gruber (1996), who study health insurance in the US, and Das, Dercon, Habyarimana, Krishnan, Muralidharan, and Sundararaman (2011), who analyze education subsidies in Zambia and India. Examples of the importance of crowding-out effects for health programs in developing countries are much less common in the literature, perhaps because of lack of data. A recent survey of the literature barely mentions this issue (Dupas (2011)).

The standard presumption in these papers is that there is substitutability between private and public expenditures, say, in health, and that individuals have perfect information about the returns to their health investments. There is however increasing evidence that decision making by the poor is greatly affected by limited information (e.g., Bertrand, Mullainathan, and Shafir (2006), Banerjee and Duflo (2011) and Dupas (2011)). This means that health programs have the potential to simultaneously deliver health services and induce changes in beliefs about the returns to health investments in the populations they serve, which could even lead to a reversal of potential crowding-out effects.

Beyond the literature on crowding-out effects of public programs, it is also important to mention how our study fits into the literature on malaria control programs, and on information and health in developing countries. We contribute to the understanding of ITN use, which is the main tool available to households to prevent infection. Several studies have investigated ways to promote acquisition and usage of ITNs in malarious villages and attention has been focused on the comparison between free-distribution and cost-sharing programs. One central paper on this topic is that by Cohen and Dupas (2010), who provide evidence in support of free distribution.

Providing information about the returns from using a technology can also be an effective way to promote both take-up and use. Dupas (2011) reviews several studies that show how the provision of information can effectively influence people's health-seeking behavior, when they are not already fully informed about the health situation they face, when the source of information is credible and when they are able to process this new information.

In a study of HIV in Malawi, De Paula, Shapira, and Todd (2011) highlight that policies may affect people's behavior if they are able to change their beliefs. They do not find strong evidence that HIV testing consistently affects people's beliefs about their own HIV status (see also Delavande and Kohler (2009)). They also show that downward revisions in beliefs about HIV status increase risky behavior, while the opposite occurs with upward revisions.

Borrowing from the literature in marketing and psychology, Dupas (2009) analyzes how the framing of information on the benefits of ITN use affects ownership and use of ITNs. She compares two cases: one which stresses the financial gains from a reduction in missed work and another highlighting the health gains from avoiding malaria. Using data from a randomized control trial (RCT) from Kenya, Dupas finds that neither take-up nor usage are affected by how benefits are framed in a marketing campaign. As a possible explanation, she proposes that the stakes are high and that liquidity constraints are probably the main barrier to investments in malaria prevention.

This aspect is further investigated by Tarozzi, Mahajan, Blackburn, Kopf, Krishnan, and Yoong (2011), who conducted a RCT in India, to estimate the effectiveness of micro-loans in promoting ITN ownership and use, to reduce malaria prevalence. Their intervention was effective in promoting ITN ownership and use, but had no impact on malaria prevalence. Tarozzi et al. (2011) rule out that the intervention caused any "perverse" behavioral response. In other words, their results showed no reduction in any pre-existing anti-malaria behavior. If anything, such behaviors actually increased in treated groups. The authors do not explain this phenomenon, but it is possible that the mechanism that we emphasize in our paper is also at work in theirs.

The remainder of the paper is organized as follows. In Section 1 we briefly describe the study area and the status quo in malaria eradication. In Section 2 we describe our dataset and we introduce our model in Section 3. We present and discuss our estimates in Section 4. Section 5 concludes.

1 IRS in Eritrea and the Intervention

Eritrea has an estimated population of 3.6 million. Malaria dramatically declined in the country over the past decade, from a national peak of 260,000 clinical cases diagnosed in 1998 to just under 26,000 cases in 2008. More than half of all diagnosed malaria cases and over 60 percent of all related deaths in the country come from Gash Barka Zone (2007, 2008), where this study was conducted.[2]

Malaria is transmitted, mainly at night, from infected to healthy people, by female *Anopheles* mosquitoes. Three main technologies are currently used to reduce transmission: ITNs, larval habitat management (LHM) and IRS. ITNs must be hung over the bed at night to protect sleeping individuals from infectious mosquito bites; LHM includes activities such as draining stagnant water, to destroy the habitat of mosquitoes; IRS consists in spraying the inside walls of dwellings with insecticide to kill resting mosquitoes.

The costs of IRS are borne by the Government, which is in charge of conducting spray campaigns. In contrast, ITNs must first be acquired by individuals and then set up above the bed. Sleeping under a net is perceived as unpleasant, especially in warm weather. ITNs also need regular re-impregnation, if they are not coated with long lasting insecticide. LHM campaigns are carried out by the Government with the active involvement of local populations.

Eritrea has been successful in greatly reducing malaria prevalence,[3] however elimination has not yet been achieved. Complete malaria eradication is therefore a priority in Eritrea. Accordingly, the National Malaria Control Program (NMCP) is

[2]We present maps of the study area in Figures 1 and 2 in Appendix 4.

[3]See Figure 4 in Appendix 4.

currently developing strategies to reduce the infection rate to zero.

IRS is an expensive intervention, although generally perceived as effective. Nevertheless, there are no studies of the added benefit of IRS in low-transmission settings over and above ITN use, effective case management, and LHM. As such, the NMCP conducted an evaluation of the impact of IRS in the context of the existing control program (which promotes LHM and ITN use), with the support of the World Bank. The results of this evaluation are presented in Keating et al. (2011).

A two-arm cluster-randomized community-controlled trial, post-test only design was used to evaluate the impact of IRS on malaria infection prevalence. Effectiveness was measured as a single difference between treatment and control groups.

One hundred and sixteen (116) villages in Gash Barka (perceived as especially malarious) were selected for the study. Fifty-eight (58) villages were randomly assigned to the treatment group and 58 villages were randomly assigned to serve as the control group. A geographic buffer was used to insure that treatment and control villages were at least 5 km apart.[4] The NMCP verified the distance between treatment and control villages, and villages that were too close (less than 5 km apart) to another were replaced by the closest village, at least 5 km apart. In addition, further replacements were made in a few cases where the originally chosen village had moved and could not be found or reached. Again, the closest eligible village was chosen as a replacement. This procedure is discussed in more detail in the next section and village replacements are documented in detail in Appendix 5.

The intervention involved the control of adult mosquito populations using IRS with the insecticide dichlorodiphenyltrichloroethane (DDT), which is recommended by the Eritrean NMCP. In each intervention village, dwellings were sprayed according to the manufacturer's recommended guidelines. The spraying targeted all households to ensure a minimum coverage of 80 percent, as recommended by the World Health Organization's (WHO). Spraying was done during the months of June–July 2009. Treatment and control villages received similar levels of ITNs,

[4]The 5 km threshold was set to ensure that control villages could not benefit from the intervention conducted in treatment villages.

LHM and case management, per existing NMCP guidelines and policy. Further details on the study design and intervention are available in Keating et al. (2011).

2 Data

A household survey was conducted in October 2009 (a baseline survey was not collected because of budgetary constraints). This corresponds to the period just after the peak of the malaria season. Only one person per household was interviewed and the response rate was high at 94.23 percent, yielding a total sample size of 1,617 households (corresponding to 7,895 individuals), of which 809 lived in treatment villages and 808 resided in control villages. All present and consenting household members were tested for malaria using Carestart® RDTs.[5] Microscopy was used to validate positive RDT results. Appendix 2 provides a detailed description of the data and of all the variables used in this paper.

Tables 1 and 2 present means and standard deviations for variables which are essentially pre-determined, and mean differences in these variables between the treatment and the control groups.[6] Table 1 shows individual variables and table 2 shows household variables. The characteristics of treatment and control villages are balanced with one exception: the Tigre tribe is over represented in the treatment group. We take this into account in our analysis by including in all regressions an indicator variable which takes a value equal to 1 if household i belongs to the Tigre tribe, and 0 otherwise (the exclusion of this variable does not affect our results).

These tables also show joint tests that check the balance of several variables

[5]A total of 5,502 people were tested with RDT. 1,120 people were absent at the time of the survey and they could not be tested. In addition, 651 people refused testing. Among those tested, 13 individuals tested positive in the control group and 17 tested positive in the treatment group. The difference between the share of positive RDTs in the two groups is 0.001 (st. err. = 0.003) and not significant (see Keating et al. (2011)). Malaria prevalence was very low in the area under investigation. More details are presented in Section 3.1 of Appendix 3.

[6]Even though some of these variables could potentially respond to the intervention, it is highly unlikely that any response took place between the time of the intervention (June–July, 2009) and the time of the survey (October, 2009).

simultaneously. We consider three different sets of variables: those available for the whole sample, those available for respondents only, and those available only at the household level. To conduct the test we run probit regressions of treatment assignment on the variables in each group, and we test whether the coefficients in the regressions are jointly equal to zero. To be precise, let $Treatment_i$ denote an indicator that takes value 1 if household i belongs to a treatment village, and 0 otherwise. Let X_i be a vector of variables in each group. Then we estimate:

$$\Pr\left(T_i = 1|X\right) = \Phi\left(X\beta\right)$$

where Φ is the cumulative density function of the standard normal, and we test whether $\beta = 0$ (where β is the vector of coefficients associated with each variable). Standard errors are clustered at the level of the community. We do not reject the null hypothesis for any of the three groups of variables, which means that we do not reject that these variables are jointly equal in the treatment and control groups. This provides additional evidence that randomization was effective in achieving balance in the characteristics of treatment and control villages.[7]

Half the population in our sample consists of females, as shown in table 1. Almost all household members usually live in the house visited by the interviewer. The population is quite young, with the average age only at 22, and the average age of respondents is about 42. Average levels of education in our sample are low: only 19 percent of respondents ever attended school and 76 percent of them attended only primary school. The proportion of literate respondents is equally low, at 19 percent. Almost all respondents are muslim and married.

Table 2 shows that average household size in the sample is between 4 and 5, with more than half of household members being below 18 years of age. Respondents

[7]As mentioned above, the list we originally used to randomly assign villages to treatment or control included 116 villages. Some names were changed at the time of the intervention or when the data collection was conducted, and some villages had to be replaced because they were not found. A very detailed analysis of this issue is presented in Appendix 5, along with robustness checks. Our analysis makes us confident that randomization was indeed effective.

living in these villages are very poor: only 43 percent of them has access to drinking water from a public tab, 6 percent has a toilet, 25 percent owns a radio, 95 percent uses firewood as the main source of fuel and the average number of rooms per house is well below 2.

Table 3 shows that there was high but not perfect compliance with treatment. Our data shows that 6 percent of households living in control villages reported having their dwelling sprayed in the 5 months prior to the survey.[8] This spraying was not done by the government. Most likely, households used simple insecticide sprays purchased from local shops, which have low effectiveness when compared to IRS.[9] Also, 25 percent of households in treatment villages reported not receiving IRS. This may have occurred because all household members were absent at the time of the intervention, or because the residents did not authorize spraying inside their home.[10]

Throughout the paper we report simple comparisons between treatment and control communities. Given that compliance with spraying was not perfect, one may think of also reporting instrumental variable estimates of the impact of the program on various outcomes, where each household's participation in spraying is instrumented by the community level treatment indicator. Estimates are reported in tables 12–16 in Section 4 of Appendix 3. We notice that these estimates are very similar to those presented in tables 5–9 in the paper. The reason why we focus on

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[8]This is roughly the period of time between treatment and the interviews, allowing for some recall error.

[9]Respondents were asked whether anyone had sprayed the interior walls of their dwelling against mosquitoes, at any time over the previous 12 months. NMCP records report that no IRS campaigns was conducted in control villages over the 12 months to the survey. We can also exclude that some other organization conducted an IRS campaign in the region. So, because the question did not specify "with DDT" or "by spraying teams", these respondents may have plausibly answered yes if they had engaged in personal spraying with commercially bought insect repellant to coat their walls. The effect of such sprays is very limited compared to that of DDT.

[10]Participation was voluntary, so some households may have not allowed IRS in their homes. In addition, there may have been lack of sufficient insecticide to treat all houses, and some dwellings maybe have been located very far from the center of the village so they were not reached by the IRS campaign. As we mentioned above, spraying targets all households to guarantee that at least 80% of the village is covered (WHO guidelines), so some degree of imperfect compliance was expected.

the community level treatment variable in the main text is that the intervention is likely to affect the beliefs and behaviors of all residents in the community. Given that spraying was so widespread in each community it will be visible to everyone, not only to those who actually received spraying.

3 Theoretical Framework

To guide our empirical analysis we present a simple model of behavioral response to the introduction of IRS under perfect and imperfect information about the probability of malaria infection. The proofs of the results presented in this section are reported in Appendix 1.

There are N identical workers, indexed by $i = 1, 2, \ldots, N$, and each worker has the same time endowment, $time_i = T, \forall i$. All individuals work (labor supply is inelastic) at wage w, which is exogenously determined. Mosquitoes are infected with malaria, and malaria affects the time endowment of worker i by reducing the time available to him from T to $T - t, t > 0$. The probability that an infected mosquito finds a worker i is $\pi_i \equiv \pi \geq 0, \forall i$. Mosquitoes bite and infect all the workers they find, unless workers use some malaria preventive technology.

For simplicity, there are only two available technologies to protect workers from malaria: ITNs and IRS. In the following, we refer to ITNs and IRS as Φ and Ψ respectively. Technology Φ, ITNs, is available to all workers, and it can protect them from infected mosquitoes with probability $p^{\Phi} \in (0, 1)$, preventing a reduction in their time endowment. Adoption of Φ causes disutility to the worker ($d_i > 0$),[11] so some workers may decide not to use it. Technology Ψ, IRS, can protect them from infectious bites with probability $p^{\Psi} \in (0, 1)$, preventing a reduction in time endowment. Use of Ψ does not entail any disutility for workers. Therefore, all workers will choose to use Ψ, if it is made available to them.

[11] Disutility may arise from a variety of factors that negatively impact ITN users, including: the need to hang the net over the bed every night; sleeping closer to other household members to fit more people inside a net; a reduction in ventilation during the hours of sleep; and allergic reactions caused by contact with the insecticide on the ITN.

Suppose technology Φ is available to all who want it. Technology Ψ may be introduced on top of Φ in an attempt to grant workers additional protection from malaria and allow them to work as much as possible. We assume that using two technologies jointly offers more protection than using either alone:[12]

Assumption 1. $\max(p^{\Phi}, p^{\Psi}) < p^{\Phi \cup \Psi}$

where $p^{\Phi \cup \Psi}$ is the probability that a worker is protected from infectious bites, if he uses both technologies. Workers are risk neutral, with utility function $U_i = Y_i - \phi_i d_i$, where ϕ_i is a dummy variable equal to 1 if worker i chooses to use Φ and 0 otherwise, and d_i represents an idiosyncratic disutility incurred when using technology Φ. Each worker chooses whether to use Φ, to maximize his own expected utility:

$$\phi_i^* \in \arg\max_{\phi_i \in \{0,1\}} E(U_i|\Psi) \tag{1}$$

In this simple model, we do not account for any externalities which may arise from others' use of ITNs. Even though they are potentially important, our main point can be made without mentioning them.[13]

3.1 Perfect Information

Under perfect information, all workers know that the probability of infectious bites, π, is $\overline{\pi} > 0$. If Ψ is not introduced, the expected time endowment for each worker is:

[12] This seems a reasonable assumption, in light of the evidence presented in Kleinschmidt, Schwabe, Shiva, Segura, Sima, Mabunda, and Coleman (2009) that combined use of IRS and ITNs reduces the probability of malaria infection more than use of either technology alone. They show that the protective efficacy of either technology is unaffected by the use of the other.

[13] A discussion is presented in Section 5 of Appendix 1.

$$
\begin{aligned}
E(time_i) &= (1-\overline{\pi})T + \overline{\pi}\left\{(1-\phi_i)(T-t) + \phi_i[(p^{\Phi}T + (1-p^{\Phi})(T-t)]\right\} \\
&= T - \overline{\pi}t\left(1-\phi_i p^{\Phi}\right) \qquad (2)
\end{aligned}
$$

where ϕ_i is an indicator variable which takes value 1 if the individual uses an ITN and takes value 0 otherwise. If no mosquitoes find and infect worker i,[14] he will have full time endowment T irrespective of his use of Φ. If a mosquito finds him (with probability $\overline{\pi}$) and if he does not sleep under an ITN, he will lose time endowment t, and will be left with $T-t$. ITN use would grant him protection with probability p^{Φ}, preventing him from losing t.

Worker i will use technology Φ if its use can increase his expected utility relatively to the case in which he does not use it. This happens if the expected gains from ITN use compensate the disutility incurred from its use:

$$
\begin{aligned}
\phi_i^* = 1 \quad &\Leftrightarrow \quad E(U_i|\phi_i = 1) > E(U_i|\phi_i = 0) \\
&\Leftrightarrow \quad w\left(T - \overline{\pi}t + \overline{\pi}p^{\Phi}t\right) - d_i > w\left(T - \overline{\pi}t\right) \\
&\Leftrightarrow \quad w\overline{\pi}tp^{\Phi} > d_i \qquad (3)
\end{aligned}
$$

The provider of preventive technologies, i.e., the government, may decide to provide Ψ. In that case:

$$
\begin{aligned}
E(time_i|\Psi = 0) \quad &= \quad T - \overline{\pi}t\left(1-\phi_i p^{\Phi}\right) \qquad (4) \\
E(time_i|\Psi = 1) \quad &= \quad T - \overline{\pi}t[1 - (p^{\Psi})^{1-\phi_i}(p^{\Phi\cup\Psi})^{\phi_i}] \qquad (5) \\
\text{If } \Psi = 0, \text{ then } \phi_i^* = 1 \quad &\Leftrightarrow \quad w\overline{\pi}tp^{\Phi} > d_i \qquad (6) \\
\text{If } \Psi = 1, \text{ then } \phi_i^* = 1 \quad &\Leftrightarrow \quad w\overline{\pi}t(p^{\Phi\cup\Psi} - p^{\Psi}) > d_i \qquad (7)
\end{aligned}
$$

[14] We assume that mosquitoes infect with certainty all workers they find.

Expressions (4) and (6) are identical to (2) and (3). Equation (5) shows how the probability of infection is affected by the introduction of Ψ. Condition (7) shows that, once spraying campaigns have been rolled out, workers will choose to sleep under an ITN if the *additional* expected gains from its use can compensate for the associated disutility.

We are interested in understanding how the introduction of IRS affects average ITN use. Let $\theta^{\Phi} \equiv E(\phi_i^*|\Psi = 0)$ be the average use of Φ when Ψ is not introduced, and let $\theta^{\Psi} \equiv E(\phi_i^*|\Psi = 1)$ represent the same measure if Ψ is made available. The difference in average ITN use is governed by the relationship between conditions (6) and (7). This comparison requires an rn v assumption on the degree of complementarity between Φ and Ψ. It is reasonable to start by assuming that Φ and Ψ are substitutes, i.e., $p^{\Phi\cup\Psi} \leq p^{\Phi} + p^{\Psi}$. In this scenario (with perfect information) we show in Appendix 1 that the average use of Φ cannot increase following the introduction of Ψ: $\theta^{\Psi} \leq \theta^{\Phi}$ (this is because no worker who does not use Φ in the absence of Ψ, would start using Φ in the presence of Ψ). It is plausible, but less natural, that the two technologies are complements instead, i.e., $p^{\Phi\cup\Psi} \geq p^{\Phi} + p^{\Psi}$. In this case the opposite is true: $\theta^{\Psi} \geq \theta^{\Phi}$.

3.2 Imperfect Information

In a more realistic setting, workers do not know the true value of π. Suppose that π can only take one of two values: 0 or $\overline{\pi} > 0$.[15] Each worker i is endowed with a prior $P_i(\pi = \overline{\pi})$ (and $P_i(\pi = 0) = 1 - P_i(\pi = \overline{\pi})$) drawn from a $Uniform(0, 1)$. Workers believe that the provider of Ψ has perfect knowledge about π. The mapping between the government's decision to spray and π is not deterministic, i.e., the government does not always spray when π is high (say, because of resource constraints), and it may spray in some cases where π is zero (say, either because of different information, or as a preventive measure). Our assumption is that indi-

[15]This formulation simplifies the structure of the problem, still capturing its essence, and it seems suitable to study the very low transmission environment under investigation.

viduals believe that the probability that the government sprays when the true risk of infection is 0 cannot exceed the probability that it does so when malaria poses a threat:[16]

Assumption 2. $\Pr(\Psi = 1|\pi = \overline{\pi}) \geq \Pr(\Psi = 1|\pi = 0)$.

Workers update their beliefs using Bayes' rule after observing the realization of Ψ. We can compute expressions (8) and (9) for the expected time endowment, which are analogous to (4) and (5):

$$E(time_i|\Psi = 0) = T - P_i(\pi = \overline{\pi}|\Psi = 0)\overline{\pi}t(1 - \phi_i p^{\Phi}) \quad (8)$$
$$E(time_i|\Psi = 1) = T - P_i(\pi = \overline{\pi}|\Psi = 1)\overline{\pi}t[1 - (p^{\Psi})^{1-\phi_i}(p^{\Phi\cup\Psi})^{\phi_i}] \quad (9)$$

Expression (8) is identical to (4), except for the fact that the posterior probability of infection is now multiplying the expected time savings, and similarly for equation (9). We can use these two equations to obtain conditions (10) and (11) for ITN use, depending on the availability of Ψ:

$$\text{If } \Psi = 0 \text{ then } \phi_i^* = 1 \quad \Leftrightarrow \quad P_i(\pi = \overline{\pi}|\Psi = 0)w\overline{\pi}t\, p^{\Phi} > d_i \quad (10)$$
$$\text{If } \Psi = 1 \text{ then } \phi_i^* = 1 \quad \Leftrightarrow \quad P_i(\pi = \overline{\pi}|\Psi = 1)w\overline{\pi}t(p^{\Phi\cup\Psi} - p^{\Psi}) > d_i \quad (11)$$

Again, we want to understand how the introduction of IRS may affect average ITN use, and we can do this by comparing conditions (10) and (11). As before, the relationship between $(p^{\Phi\cup\Psi} - p^{\Psi})$ and p^{Φ} depends on whether the two technologies are substitutes or complements, but now the expected gains from ITN use also depend on the posterior probabilities of infection. Assumption 2 implies that $P_i(\pi = \overline{\pi}|\Psi = 1) \geq P_i(\pi = \overline{\pi}|\Psi = 0)$. Therefore, with imperfect information, θ^{Ψ}

[16] People are aware that the government has successfully managed to drastically reduce malaria in recent years, and so they understand that it is committed to fight the disease. This makes the government "credible".

could be either larger or smaller than θ^{Φ}, even when Φ and Ψ are substitutes. This is in contrast with the analogous result for the perfect information case, for which the result was unambiguous.

Finally, if agents perceive Φ and Ψ to be complements, it is easy to show that the average use of ITNs may either remain unchanged or increase with the introduction of Ψ, as in the previous scenario. Table 4 summarizes the predictions of the model, under either assumption.

4 Data Analysis

4.1 Basic Results

In this section we analyze the impact of the IRS campaign on a set of behavioral and socio-economic outcomes. In particular, we look at the effect of spraying (1) on the level of information and awareness of malaria among the people of Gash Barka, (2) on the ownership and use of mosquito bed nets, as well as (3) on their intra-household allocation. The impact of IRS on the prevalence of malaria was found to be zero in our earlier work (see Keating et al. (2011); see also tables 1 and 2 in Appendix 3).

In tables 5–8 we compare treatment and control villages across a variety of dimensions (information and knowledge of malaria, ownership and use of mosquito bed nets, participation in LHM, and behaviors conducive to malaria eradication other than LHM). The first two columns of each table present means and standard deviations for each variable, for treatment and control villages. The remaining columns report differences (and corresponding standard errors) between treatment and control villages using three different specifications (which, given our experimental design, we interpret as the impact of the program). The first specification does not account for any control variables, and therefore corresponds to a simple difference in means between the two sets of villages. The second and third specifications include, respectively, a very simple set of control variables (dummy indicating whether an individual belongs to the Tigre tribe,[17] a dummy indicating Muslim religion, and dummies for subzone of residence), and a more complete set of control variables which includes all the variables we analyzed in the randomization checks (which we call X_{other} in the equations below).[18] We estimate the program impact using least squares regression (12) of Y on a treatment indicator ($Treatment$, in the equation below) and control variables when Y is a continuous variable, or using probit model (13) when Y is binary (marginal effects are presented in this case):

$$\begin{aligned} Y &= \alpha + \beta\, Treatment + \gamma_1\, Tigre\, Tribe \\ &\quad + \gamma_2\, Muslim + \gamma'\, Subzones + X_{other}\lambda + \epsilon \qquad (12) \\ \Pr(Y=1) &= \Phi(\alpha + \beta\, Treatment + \gamma_1\, Tigre\, Tribe \\ &\quad + \gamma_2\, Muslim + \gamma'\, Subzones + X_{other}\lambda) \qquad (13) \end{aligned}$$

where Φ is the cumulative density function of the standard normal. Standard errors are clustered at the village level. Across tables, our estimates are almost identical for models with different controls (columns 3–5). Much of our discussion will focus on the specification with basic regressors.

Table 5 shows that, in spite of the fairly low levels of parasite prevalence in the region,[19] malaria is still (correctly) perceived as a problem in the community by a large majority of the population, both in treatment and control villages. However, we notice that more than 25 percent of respondents report that malaria is not a problem in their community (despite the fact that our survey was conducted in the most malarious villages in Eritrea).[20] There is also widespread knowledge that

[17] This is the main tribe in Gash Barka and it is over-represented in treatment villages.

[18] School enrolment is excluded because it is recorded only for children in school age.

[19] Keating et al. (2011) document a prevalence rate below 1 percent (October, 2009).

[20] The Global Malaria Action Plan of the Roll Back Malaria initiative (available at http://www.rbm.who.int/gmap/) explains that the situation whereby villagers lose interest in malaria and in prevention, in areas where malaria has been dramatically reduced by successful control efforts, is referred to as "malaria fatigue", and that it can lead the public to reduce use of the available preventive and treatment measures. So this issue must be addressed properly and in a timely fashion.

mosquitoes are an important transmission vector. Even though almost everyone agrees that children are especially at risk from malaria, only about a third of respondents believe that pregnant women suffer greatly from having malaria. Finally, about half of the respondents were aware of information campaigns conducted during the 6 months prior to the interview, concerning ITNs, early seeking behavior (seeking timely treatment and proper diagnostic of malaria symptoms) and environmental management.

Table 5 also presents the estimated effect of the IRS campaign on information and knowledge about malaria. Our estimates suggest that treatment increased knowledge that mosquitoes are a vector by about 3 percent, and awareness that children are especially at risk from malaria by almost 7 percent. On average, respondents did not become more worried that malaria was a problem in their community, nor that women are particularly vulnerable to malaria. We test and reject that these four variables are jointly equal in treatment and control villages. These results show there is more concern with malaria transmission in treatment than in control villages, suggesting that the provision of IRS led individuals to update their beliefs about the importance of malaria in their communities. In particular, the increased concern with the impact on malaria on children, paired with an increased awareness that mosquitoes are the transmission vector for the disease, may have changed the expected returns to malaria prevention behaviors such as ITN use.

It is also useful to notice that respondents in treatment villages did not receive significantly more information on ITNs, early seeking behavior and environmental management over the previous 6 months, than those in the control group. These variables are not statistically different in treatment and control variables, either when we look at them individually or jointly. This suggests that any changes in information and knowledge are a direct consequence of the IRS campaign.

Table 6 reports information on ownership and use of bed nets. In this Section we draw a distinction between "ITNs" and "nets": we restrict the former definition to consist only of those nets that were properly treated with insecticide at the time of the survey,[21] while we use the latter term to additionally include those nets that had not been properly re-treated. On average, there were about 1.58 nets and 1.28 ITNs per household in the control group villages. Furthermore, an average of 1.16 nets per household were used the previous night and 0.736 nets were left unused. These figures are slightly higher in the treatment villages. A comparison of ownership figures for any nets versus ITNs suggests that the vast majority of owned bed nets were properly insecticide treated at the time of the survey.[22] About 40 percent of all household members in control villages reportedly slept under a net (net use) the night before the survey.

In table 6 we also present the estimated program effects on ownership and use of bed nets.[23] Households living in treated villages own 0.214 more nets and 0.176 more ITNs than households from control villages. The number of nets used the night before the survey was 0.186 higher in treated villages, but there was no discernible difference in the number of unused nets between treatment and control. We jointly test and reject (at the 10 percent level of significance) that there is no differ-

[21] We include in the definition of "ITNs": all Long Lasting Insecticide treated Nets (LLINs), which were distributed in the area starting from 2006 and whose insecticide is effective for 3–5 years; all ITNs acquired in the 3 years prior to the survey (which are most likely LLINs, since the government distributed exclusively LLINs since 2006); and all ITNs that were re-treated in the 12 months before the survey, in accordance with NMCP guidelines.

[22] We do not study explicitly households' participation in net re-impregnation activities because LLINs have progressively replaced traditional ITNs since the NMCP discontinued distribution of the latter in favor of the former in 2006. An additional reason for omitting an analysis of re-impregnation behavior is that we include in the definition of LLINs also all ITNs acquired in the 3 years before the survey (as we explained in footnote 21) and LLINs need not be re-impregnated.

[23] An interesting question is whether households can (and do) acquire new bed nets if they want to do so, or whether supply is determined solely by free distribution campaigns that provide the same number of nets to every household. To shed some light on this point, we use an asset index (described in Section 3.2 of Appendix 3) to compare statistics on ownership of bed nets by wealth quintile. Focusing on the control group (i.e., in the absence of the intervention), we see that net ownership increases with wealth, so that households in the top quintile own a number of nets (2.17) which is about double that of households in the lowest quintile (1.24). The same can said about ITNs: ownership increases progressively over wealth quintiles, from 0.99 to 1.59 ITNs per household. This is suggestive that ownership of bed nets is not exogenously determined by free distribution campaigns. To the contrary, wealthier households can and do obtain a larger number of nets. They may do so, e.g., by purchasing nets from a local market or from poorer households, or they may possibly exploit their bargaining power to obtain more free nets during distribution campaigns.

ence in these four variables between treatment and control villages. The proportion of individuals reported to have used a net is higher in treatment than in control villages but the difference is not statistically strong (this variable is not included in the joint test because it is an individual rather than a household variable). These results show a clear difference in net ownership and use between treatment and control villages.

Our results are consistent with the model we developed in Section 3. In response to the introduction of IRS in a community, its inhabitants experience an increase in awareness and concern about malaria (especially about the danger of mosquito bites), which affects their ownership and use of ITNs. As far as we know, this mechanism has not been discussed before in the literature, although it could be important in many settings. By introducing a program in a community, be it a health, education, or other type of program, a government potentially provides information about its knowledge of the problem addressed by the program, or it just makes the problem more salient in the minds of community members. When individuals have imperfect information and face uncertainty about the importance of the particular problem at hand, such revelation of information may lead individuals to update their beliefs and, as a result, change their behaviors. These changes in behaviors are generally not expected by those designing the program. This section shows that they can be quite important.

In addition to using bed nets, individuals can engage in other preventive behaviors to reduce the risk of malaria infection. For example, they can keep any cattle away from home, cover any stored water and participate in environmental management campaigns, among others. Table 7 focuses on participation in LHM campaigns and it shows that participation is fairly low across a variety of measures, as pointed out in Keating et al. (2011). Table 8, which includes the full range of mentioned ways how respondents try to avoid mosquito bites, shows that households engage in a wide variety of malaria prevention behaviors other than ITN use and LHM.

Tables 7 and 8 also report estimates of the impact of IRS on those behaviors.

We do not find evidence that IRS crowded-out private investment in any of those behaviors.[24] If anything, the IRS campaign had a positive effect, especially on the proportion of households who keep their livestock away from their dwelling, which increased by as much as 6.76 percent.[25]

4.2 Intra-Household Allocation of Bed Nets

We also checked whether IRS affected net use among some demographic groups and how this changed the intra-household allocation of nets. To do so, we divided the population into six mutually exclusive categories (children under 5 years of age, school age youths (5–20 years old), employed adult (>20 years old) men and women, and unemployed adult men and women) and we analyzed how the intervention affected net use in each of the groups.

Table 3 in Appendix 3 shows that, in the absence of IRS, net usage varies greatly by age, gender and employment status: children under 5 are the most likely to sleep under a bed net (50 percent), followed by unemployed and employed women in working age (44 and 40 percent), school age youths (36 percent) and finally by employed and unemployed adult men (27 and 24 percent). No significant gender differences were observed among children under five or among young people. Among employed adults, women are much more likely to sleep under a bed net (+13 percent) and the same is true among the unemployed (+20 percent).

We estimate the impact of the intervention on the intra-household allocation

[24] Standard errors are rather small in tables 7 and 8, so we would have been able to detect a negative impact of IRS on these sets of behaviors, had there been any. In addition, most coefficients have a positive sign (particularly so in table 7), whereas a negative sign would hint to the presence of crowd-out. The joint test in table 7 omit the third variable in the table (number of household members who participated in LHM) because it is just the sum of the three subsequent variables in the table. Similarly, in table 8 the first two variables are omitted from the joint test only because the sample of non-missing answers for these variables is much smaller than for the remaining variables in the table.

[25] As mentioned above, instrumental variables estimates of the impact of IRS on all these outcomes (where household participation in IRS is instrumented by the village treatment assignment) are presented and discussed in Section 4 of Appendix 3. Our main conclusions are essentially unchanged.

of bed nets using probit regression (13), letting Y be a dummy for net use, and restricting the sample in turn to each socio-demographic category. Estimates are presented in table 9 (which shows marginal effects). For each socio-demographic group, the first two columns of table 9 present average bed net use in treatment and control villages, with standard deviations in parentheses. The remaining three columns present the impact of the intervention on the intra-household allocation of bed nets, with the same sets of controls used in tables 5–8 .

Table 9 shows that treatment increased bed net use especially among workers, and we can see in particular that 8 percent more male workers chose to sleep under a bed net; the estimated increase among female workers is not robust to different specifications (and it is not statistically significant from zero in our favorite specification). We notice, importantly, that the use of bed nets did not decline (estimated coefficients are positive but non significant) among children under five, who are among the most vulnerable to malaria. Similarly, adult women were not negatively affected (irrespective of their employment status).[26]

These results, which show an increase in net use among workers, are consistent with the previous findings that information and awareness about malaria increased in the population, and with the idea that households became more sensitive to the importance of protecting their breadwinners, thereby adapting the intra-household allocation of nets. This evidence is also in line with results presented in Section 3 of Appendix 3, showing that malaria awareness increased especially among workers, who increased net use accordingly. Increased net use among workers may have stemmed from the observed increase in net ownership or from a change in sleeping arrangements, with workers sharing more often sleeping space with their spouse and young children.[27]

[26] Adult women include pregnant women, a category that is very vulnerable to malaria. We do not have data about pregnancy.

[27] In Section 3 of Appendix 3 we present additional results describing how the impacts of the program vary with the level of vegetation in the district ("subzoba") where villages are located, and we focus our attention on the treatment effect on malaria knowledge and on net ownership and use. We also check heterogeneity in impacts according to several characteristics of the respondent (we do not have these information for all household heads, so we use respondents as a proxy; the

Given the estimates in table 5 one could have thought that the largest increase in net use would be among children. However, it is possible that a greater awareness that malaria has a strong impact on children may just be a manifestation of a more general concern and awareness of the dangers of malaria.

5 Conclusions

The concern that government intervention crowds-out desirable private behavior is common to several areas of public policy. The standard model predicts that this will happen if private and public inputs are substitutes. This paper emphasizes a new mechanism by which government intervention may encourage a higher provision of the private input, even when private and public inputs are substitutes. This can occur when individuals have little information about the returns to their actions, and when the public intervention reveals information that may lead to an increase in their subjective expectations of the returns to their actions. This is not only interesting, but also likely to be important in a variety of settings. We apply and illustrate the relevance of this idea to the study of a malaria control program in Eritrea.

Several countries in Sub-Saharan Africa, including Eritrea, have successfully reduced the malaria burden in their territory in recent years, using a combination of free ITN distribution, LHM, case management, prompt and effective treatment, and information campaigns. Their governments are now contemplating strategies to eliminate the disease once and for all, and in particular they are considering the

respondent was the head in 61.71 percent of the households and the spouse in 33.83 percent of the cases): employment status, literacy, education, religion, tribe, female headship, family size, presence of children in the household, and wealth. We see increases in concern with malaria mainly among workers, although we see some increase in information also among the non-employed. This is plausible if information reaches everyone, but if it becomes a source of concern only for workers (because they are the ones who potentially suffer the most if they are afflicted with the disease). Regarding net ownership, we observe that impacts of IRS are much larger for families where the respondent is literate and employed, and they are lower for families in the bottom quintile of the wealth distribution.

introduction of regular IRS campaigns to achieve this goal, whereas IRS was used so far chiefly for emergency response.

Public provision of IRS may crowd-out people's private investment in the existing risk mitigating technologies, possibly leading to a resurgence of the disease rather than to a sharp decrease and its eventual eradication. In a companion paper, we document that a single IRS intervention is not sufficient to eradicate malaria completely in a policy-induced low-transmission setting like the one under investigation. It is therefore of paramount importance that people consistently make use of the preventive technologies available to them, to ensure that malaria eradication can be achieved in the medium run (possibly with the help of several IRS campaigns).

Our main result is that public IRS provision did not crowd-out private investment in any malaria control policy in Eritrea in the short run: in fact, IRS did not induce a reduction in ownership or use of ITNs, nor did it have a negative impact on any of the other risk mitigating behaviors in which villagers are engaged. If anything, spraying led to an increase in preventive behaviors. We show that IRS increased average ownership of ITNs, and that it promoted net use among workers.

We explain this with a simple model of net use in a setting where individuals have imperfect information about the risk of being infected by a mosquito carrying the malaria parasite, and update their beliefs about the level of malaria prevalence in their area of residence when they observe the introduction of a new intervention. This model proposes that public health interventions may act as marketing campaigns, capable to promote take-up of the existing preventive technologies, and as an information campaign, that fosters active use of the available risk mitigating tools. This can be true even when the original goal of the intervention was neither marketing nor the provision of information, such as in the case of an IRS campaign. Both our empirical results and our interpretation are novel in the literature.

We observe in our data a very high pre-intervention awareness about malaria, about the mode of transmission of the disease, and about who is at increased risk of being ill. We show that IRS provision promoted malaria awareness even further. Mosquito net ownership and use also increased after treatment. This increase in net use occurs mainly among household members who are currently working. We also show that net use among the most vulnerable categories (including children under the age of five and pregnant women) was not negatively affected by the rise in use among workers.

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Table 1: Randomization checks – Individual Variables

Variables (Y)	Treatment	Control	Difference
ALL HOUSEHOLD MEMBERS			
1- Female	0.52	0.52	-0.0040
	(0.50)	(0.50)	(0.0113)
2- Usually lives here	0.98	0.98	0.0062
	(0.13)	(0.16)	(0.0049)
3- Stayed here last night	0.97	0.95	0.0137
	(0.18)	(0.21)	(0.0086)
4- Age	22.34	22.00	0.3456
	(19.52)	(19.18)	(0.4924)
RESPONDENTS ONLY			
5- Age	42.05	41.43	0.6157
	(15.01)	(15.25)	(0.8926)
6- Ever attended school	0.19	0.19	0.0072
	(0.39)	(0.39)	(0.0339)
7- Only primary school	0.74	0.78	-0.0373
	(0.44)	(0.41)	(0.0527)
8- Literate	0.18	0.20	-0.0151
	(0.39)	(0.40)	(0.0321)
9- Muslim religion	0.84	0.78	0.0601
	(0.37)	(0.42)	(0.0678)
10- Tigre tribe	0.57	0.40	0.1666*
	(0.50)	(0.49)	(0.0843)
11- Married	0.93	0.94	-0.0125
	(0.26)	(0.24)	(0.0133)
		P-value [variables 1–4]	0.25
		P-value [variables 5–6,8–11]	0.16

Note: for variables 5–11: sample restricted to respondents only. Column (1): sample restricted to treatment group. Column (2): sample restricted to control group. For each variable Y, columns (1) and (2) report means, with standard deviations in parentheses. Column (3) presents the difference between (1) and (2) estimated as follows: $Y_i = \beta T_i + \varepsilon_i$, where T_i is a treatment allocation dummy. Robust standard errors are reported in parentheses. We also use an F-test to check whether groups of controls, with comparable sample sizes, jointly predict treatment and we report the p-values (we run regressions of treatment on different sets of variables). Variable 7 is not used in the joint test because it has missing values for respondents without any schooling, so it has smaller sample size than variables 5–6,8–11. Observations clustered at village level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2: Randomization checks – Household Variables

Variables (Y)	Treatment	Control	Difference
HOUSEHOLD LEVEL VARIABLES			
12- Household size	4.98	4.79	0.1844
	(2.30)	(2.28)	(0.1559)
13- Household members under 5	0.85	0.82	0.0214
	(0.90)	(0.94)	(0.0566)
14- Household members under 18	2.69	2.60	0.0925
	(1.98)	(1.96)	(0.1279)
15- Main source of drinking water			
15a- Public tap	0.42	0.43	-0.0104
	(0.49)	(0.50)	(0.0773)
15b- Unprotected well	0.25	0.23	0.0195
	(0.43)	(0.42)	(0.0545)
15c- Unprotected spring	0.13	0.14	-0.0150
	(0.33)	(0.35)	(0.0384)
16- Has any toilet	0.05	0.07	-0.0112
	(0.23)	(0.25)	(0.0232)
17- Has radio	0.25	0.24	0.0084
	(0.43)	(0.43)	(0.0324)
18- Firewood is main fuel	0.93	0.96	-0.0214
	(0.25)	(0.20)	(0.0185)
19- Has no window	0.32	0.32	0.0050
	(0.47)	(0.47)	(0.0656)
20- Number of separate rooms	1.86	1.83	0.0225
	(1.18)	(1.20)	(0.1049)
21- Number of sleeping rooms	1.39	1.38	0.0020
	(0.82)	(0.71)	(0.0509)
22- Number of sleeping spaces	4.61	4.44	-0.1641
	(2.45)	(2.35)	(0.1900)
P-value [variables 12–22]			0.925
P-value [variables 5–6,8–22]			0.276

Note: one observation per household. Column (1): sample restricted to treatment group. Column (2): sample restricted to control group. For each variable Y, columns (1) and (2) report means, with standard deviations in parentheses. Column (3) presents the difference between (1) and (2) estimated as follows: $Y_i = \beta T_i + \varepsilon_i$, where T_i is a treatment allocation dummy. Robust standard errors are reported in parentheses. We also use an F-test to check whether groups of controls, with comparable sample sizes, jointly predict treatment and we report the p-values (we run regressions of treatment on different sets of variables). Variable 7 is not used in the joint test because it has missing values for respondents without any schooling, so it has smaller sample size than variables 5–6,8–22. Observations clustered at village level. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Compliance with treatment allocation

	Control group	Treatment group
Dwelling was sprayed in past 5 months	49	604
Dwelling was not sprayed in past 5 months	679	124
Missing information or respondent does not know	80	81

Note: This table shows the number of respondents reporting that someone sprayed the interior walls of their dwelling against mosquitoes in the 5 months prior to the survey or that no one did, in the control and in the treatment groups. Five months corresponds approximately to the period of time between the IRS intervention and the survey.

Table 4: Summary of the theoretical predictions

	Imperfect substitutes	Imperfect complements
Perfect Information	$\theta^{\Psi} \leq \theta^{\Phi}$	$\theta^{\Psi} \geq \theta^{\Phi}$
Imperfect Information	$\theta^{\Psi} \leq \theta^{\Phi}$ or $\theta^{\Psi} \geq \theta^{\Phi}$	$\theta^{\Psi} \geq \theta^{\Phi}$

Note: Average use of Φ depending on the complementarity between Φ and Ψ and on the availability of information about malaria prevalence. $\theta^{\Phi} \equiv E(\phi_i^*|\Psi = 0)$ and $\theta^{\Psi} \equiv E(\phi_i^*|\Psi = 1)$.

Table 5: Information and knowledge about malaria

			$E(Y\|T=1,X) - E(Y\|T=0,X)$		
Variables	Treatment	Control	No Regressors	Basic Regressors	All Regressors
1. Mosquitoes mentioned among malaria vectors	0.908	0.854	0.0541**	0.0305*	0.0384**
	(0.289)	(0.353)	(0.0213)	(0.016)	(0.0158)
2. Malaria is a problem in community	0.726	0.670	0.0564	0.035	0.0401
	(0.446)	(0.471)	(0.0442)	(0.035)	(0.0373)
3. Children mentioned among most affected by malaria	0.863	0.788	0.0744***	0.0679***	0.0603***
	(0.344)	(0.409)	(0.0248)	(0.019)	(0.0183)
4. Pregnant women mentioned among most affected	0.367	0.365	0.002	-0.0143	-0.00637
	(0.482)	(0.482)	(0.0403)	(0.024)	(0.0263)
5. In the previous 6 months, heard/saw messages about:					
5a. ITNs	0.484	0.469	0.0152	-0.00050	0.00306
	(0.500)	(0.499)	(0.0421)	(0.038)	(0.0359)
5b. Early seeking behavior	0.537	0.501	0.0365	0.019	0.0184
	(0.499)	(0.500)	(0.0420)	(0.040)	(0.0363)
5c. Environmental management	0.450	0.387	0.0638	0.029	0.0306
	(0.498)	(0.487)	(0.0430)	(0.036)	(0.0357)
Joint tests on variables (with comparable sample size):	1–4	p-values =	0.0103	0.0021	0.0096
	5a–5c		0.4462	0.7562	0.7463

Note: one observation per household (data available for respondents only). Columns 1 and 2 report means for treatment and control groups, with standard deviations in parentheses. Columns 3–5 report the difference between treatment and control groups, estimated using probit regression (13). The specification in column 3 does not include any controls. The specification in column 4 includes controls for: Tigre tribe, Muslim religion and subzone dummies. The specification in column 5 additionally includes all controls used in the randomization checks, i.e.: gender, age, household size, number of household members under age five, number of household members under age 18, number of rooms in dwelling, number of sleeping rooms, number of sleeping spaces; and dummy variables for whether respondent: usually lives in dwelling, was there the night before, ever attended school, is literate, is married; and dummy variables for whether main water source is: public tap, unprotected well, unprotected spring; and dummy variables for whether household owns a toilet, household owns a radio, firewood is main fuel for cooking, dwelling has no windows. In all regressions, observations are clustered at village level and robust standard errors are reported in parentheses. We also run a probit regression of treatment on sets of variables in the table plus the controls listed above and we test if these sets of coefficients are jointly We report p-values at the bottom of the table. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Ownership and use of mosquito bed nets

			$E(Y\|T=1,X) - E(Y\|T=0,X)$		
Variables	Treatment	Control	No Regressors	Basic Regressors	All Regressors
1. Number of nets owned by household	1.774	1.575	0.200*	0.214**	0.214**
	(1.279)	(1.207)	(0.110)	(0.0996)	(0.0837)
2. Number of ITNs owned by household	1.444	1.278	0.166*	0.176*	0.181**
	(1.206)	(1.126)	(0.0963)	(0.0926)	(0.0821)
3. Reported net use (of each household member)	0.429	0.380	0.049	0.034	0.059*
	(0.495)	(0.486)	(0.035)	(0.033)	(0.031)
4. Number of observed nets used the night before	1.384	1.164	0.220**	0.186**	0.170**
	(1.214)	(1.054)	(0.0990)	(0.0877)	(0.0824)
5. Number of observed nets left unused the night before	0.676	0.736	-0.0600	0.0152	0.00153
	(0.993)	(1.001)	(0.0763)	(0.0626)	(0.0636)
Joint tests on variables (with comparable sample size):	1,2,4,5	p-values =	0.1468	0.0958	0.0659

Note: one observation per household for variables 1,2,4,5. One observation per individual for variable 3. In this table, "nets" refers to any bed nets, irrespective of their treatment status, whereas "ITNs" includes only LLINs and properly treated ITNs, following the definition presented in footnote 21. Columns 1 and 2 report means for treatment and control groups, with standard deviations in parentheses. Columns 3–5 report the difference between treatment and control groups, estimated using LS regression (12) for continuous outcomes and probit regression (13) for binary outcomes. The specification in column 3 does not include any controls. The specification in column 4 includes controls for: Tigre tribe, Muslim religion and subzone dummies. The specification in column 5 additionally includes all controls used in the randomization checks, i.e.: gender, age, household size, number of household members under age five, number of household members under age 18, number of rooms in dwelling, number of sleeping rooms, number of sleeping spaces; and dummy variables for whether respondent: usually lives in dwelling, was there the night before, ever attended school, is literate, is married; and dummy variables for whether main water source is: public tap, unprotected well, unprotected spring; and dummy variables for whether household owns a toilet, household owns a radio, firewood is main fuel for cooking, dwelling has no windows. In all regressions, observations are clustered at village level and robust standard errors are reported in parentheses. We also run a probit regression of treatment on sets of variables in the table plus the controls listed above and we test if these sets of coefficients are jointly significant. We report p-values at the bottom of the table. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 7: Participation in Larval Habitat Management (LHM)

			$E(Y\|T=1,X)-E(Y\|T=0,X)$		
Variables	Treatment	Control	No Regressors	Basic Regressors	All Regressors
Over the six month before the survey:					
1. Respondent participated in LHM	0.322	0.282	0.040	0.012	0.021
	(0.468)	(0.450)	(0.044)	(0.038)	(0.037)
During the month before the survey:					
2. Days spent by household in LHM	0.632	0.618	0.013	0.025	0.094
	(2.774)	(1.978)	(0.181)	(0.161)	(0.180)
3. Household members who participated in LHM	0.456	0.39	0.066	0.051	0.055
	(1.007)	(0.898)	(0.077)	(0.071)	(0.065)
4. Male household members >15 years old	0.167	0.125	0.042	0.025	0.030
who participated in LHM	(0.462)	(0.399)	(0.031)	(0.027)	(0.026)
5. Female household members >15 years old	0.215	0.219	-0.004	-0.001	-0.008
who participated in LHM	(0.47)	(0.483)	(0.038)	(0.034)	(0.033)
6. Household members <15 years old	0.075	0.046	0.029	0.027	0.033
who participated in LHM	(0.467)	(0.372)	(0.025)	(0.026)	(0.025)
Joint tests on variables (with comparable sample size):	1,2,4–6	p-values =	0.3683	0.5752	0.3652

Note: one observation per household. Columns 1 and 2 report means for treatment and control groups, with standard deviations in parentheses. Columns 3–5 report the difference between treatment and control groups, estimated using LS regression (12) for continuous outcomes and probit regression (13) for binary outcomes. The specification in column 3 does not include any controls. The specification in column 4 includes controls for: Tigre tribe, Muslim religion and subzone dummies. The specification in column 5 additionally includes all controls used in the randomization checks, i.e.: gender, age, household size, number of household members under age five, number of household members under age 18, number of rooms in dwelling, number of sleeping rooms, number of sleeping spaces; and dummy variables for whether respondent: usually lives in dwelling, was there the night before, ever attended school, is literate, is married; and dummy variables for whether main water source is: public tap, unprotected well, unprotected spring; and dummy variables for whether household owns a toilet, household owns a radio, firewood is main fuel for cooking, dwelling has no windows. In all regressions, observations are clustered at village level and robust standard errors are reported in parentheses. We also run a probit regression of treatment on sets of variables in the table plus the controls listed above and we test if these sets of coefficients are jointly significant. We report p-values at the bottom of the table. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Behaviors conducive to malaria eradication, other than LHM

			$E(Y\|T=1,X) - E(Y\|T=0,X)$		
Variables	Treatment	Control	No Regressors	Basic Regressors	All Regressors
1. Household keeps livestock >100m from home	0.807	0.776	0.031	0.068**	0.067**
	(0.395)	(0.417)	(0.032)	(0.031)	(0.030)
2. Household covers stored water	0.942	0.953	-0.011	-0.027	-0.019
	(0.234)	(0.212)	(0.020)	(0.018)	(0.017)
3. Respondent does anything to prevent mosquito bites	0.834	0.804	0.030	-0.006	0.007
	(0.372)	(0.397)	(0.031)	(0.025)	(0.025)
4. Respondent mentions using net	0.680	0.649	0.029	0.011	0.027
	(0.467)	(0.478)	(0.039)	(0.029)	(0.030)
5. Respondent mentions burning coils	0.225	0.211	0.015	0.003	-0.002
	(0.418)	(0.409)	(0.035)	(0.022)	(0.023)
6. Respondent mentions using spray	0.025	0.021	0.004	0.010	0.010
	(0.156)	(0.143)	(0.009)	(0.008)	(0.007)
7. Respondent mentions burning animal dung	0.058	0.046	0.012	0.005	0.004
	(0.234)	(0.209)	(0.014)	(0.012)	(0.011)
8. Respondent mentions burning herbs	0.048	0.054	-0.006	-0.017	-0.013
	(0.215)	(0.226)	(0.018)	(0.014)	(0.012)
9. Respondent mentions draining stagnant water	0.106	0.120	-0.014	-0.022	-0.016
	(0.309)	(0.325)	(0.021)	(0.018)	(0.016)
Joint tests on variables (with comparable sample size):	3–9	p-values =	0.8851	0.5764	0.4199

Note: one observation per household. Columns 1 and 2 report means for treatment and control groups, with standard deviations in parentheses. Columns 3–5 report the difference between treatment and control groups, estimated using probit regression (13). The specification in column 3 does not include any controls. The specification in column 4 includes controls for: Tigre tribe, Muslim religion and subzone dummies. The specification in column 5 additionally includes all controls used in the randomization checks, i.e.: gender, age, household size, number of household members under age five, number of household members under age 18, number of rooms in dwelling, number of sleeping rooms, number of sleeping spaces; and dummy variables for whether respondent: usually lives in dwelling, was there the night before, ever attended school, is literate, is married; and dummy variables for whether main water source is: public tap, unprotected well, unprotected spring; and dummy variables for whether household owns a toilet, household owns a radio, firewood is main fuel for cooking, dwelling has no windows. In all regressions, observations are clustered at village level and robust standard errors are reported in parentheses. We also run a probit regression of treatment on sets of variables in the table plus the controls listed above and we test if these sets of coefficients are jointly significant. We report p-values at the bottom of the table. *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Intra-household allocation of bed nets

	Y = 1(Net Use)				
			$E(Y\|T=1,X) - E(Y\|T=0,X)$		
Subsamples	Treatment	Control	No Regressors	Basic Regressors	All Regressors
Children under 5	0.5292	0.4970	0.0323	0.0174	0.0246
	(0.4995)	(0.5004)	(0.0471)	(0.0394)	(0.0403)
Youth aged 5–20	0.4107	0.3623	0.0484	0.0327	0.0625*
	(0.4921)	(0.4808)	(0.0393)	(0.0382)	(0.0332)
Adult male workers	0.3520	0.2697	0.0823**	0.0841**	0.1134***
	(0.4781)	(0.4443)	(0.0407)	(0.0420)	(0.0425)
Adult female workers	0.5000	0.4026	0.0974*	0.0695	0.1313**
	(0.5013)	(0.4915)	(0.0544)	(0.0568)	(0.0611)
Adult male unemployed	0.3000	0.2409	0.0591	0.0570	0.0793
	(0.4594)	(0.4286)	(0.0556)	(0.0564)	(0.0549)
Adult female unemployed	0.4714	0.4408	0.0306	0.0132	0.0269
	(0.4996)	(0.4969)	(0.0452)	(0.0405)	(0.0444)

Note: The outcome variable Y is an indicator variable =1 if individual reportedly slept under a bed net the night before the survey, and =0 otherwise. For each subsample, columns 1 and 2 report average bed net use in treatment and control villages, with standard deviations in parentheses. Columns 3–5 report the difference between treatment and control groups, estimated using probit regression (13). The specification in column 3 does not include any controls. The specification in column 4 includes controls for: Tigre tribe, Muslim religion and subzone dummies. The specification in column 5 additionally includes all controls used in the randomization checks, i.e.: gender, age, household size, number of household members under age 5, number of household members under age 18, number of rooms in dwelling, number of sleeping rooms, number of sleeping spaces; and dummy variables for whether respondent: usually lives in dwelling, was there the night before, ever attended school, is literate, is married; and dummy variables for whether main water source is: public tap, unprotected well, protected unprotected spring; and dummy variables for whether household owns a toilet, household owns a radio, firewood is main fuel used for cooking, dwelling has no windows. In all regressions, observations are clustered at village level and robust standard errors are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Appendix 1: Theoretical Framework

May 9, 2012

For Online Publication

This appendix complements Section "Theoretical Framework" in "Do Public Health Interventions Crowd Out Private Health Investments? Malaria Control Policies in Eritrea" by P. Carneiro, A. Locatelli, T. Ghebremeskel and J. Keating.

1 Setting

There are N identical workers, indexed by $i = 1, 2, \ldots, N$. Each worker has the same time endowment $time_i \equiv T, \forall i$. There is only one firm, with infinite labor demand at wage w, so labor demand is perfectly elastic. Labor supply is perfectly inelastic: workers want to spend their entire time endowment at work. Malaria may affect workers' time endowment, by reducing available time from T to $T-t, t > 0$[1]. We assume that mosquitoes bite and infect all workers they find. The probability that an infected mosquito finds and infects a worker is $\pi \geq 0$.

Two technologies, namely ITNs and IRS, are available to protect workers from malaria.[2] In the following, we refer to ITNs and IRS as Φ and Ψ respectively. Technology Φ, ITNs, is available to all workers, and it can protect them from infected

[1]This simplifying assumption means that workers can catch malaria just once a year and all malaria cases entail an identical loss of working time, equal to t.

[2]In reality, there are of course more than two technologies for malaria control. Considering only two helps simplify the model. In addition, two is all we need to explain our empirical results.

mosquitoes with probability $p^\Phi \in (0,1)$, preventing a reduction in their time endowment. Adoption of Φ causes disutility to the worker ($d_i > 0$),[3] so some workers may decide not to use it. Notice that we let $p^\Phi < 1$ because it is still possible for a person sleeping under a net to be bitten by a mosquito before or after sleeping, or through the net if the body touches it, or by any mosquitoes found inside the net.

Technology Ψ, IRS, can protect them from infectious bites with probability $p^\Psi \in (0,1)$, preventing a reduction in time endowment. Use of Ψ does not entail any disutility for workers. Therefore, all workers will choose to use it if it is made available to them. Notice again that we let $p^\Psi < 1$ because, despite IRS, it remains possible for a person living in dwelling treated with IRS to be bitten by a mosquito, both inside or outside the house.

Suppose technology Φ is available to all who want it. Technology Ψ may be introduced on top of Φ in an attempt to grant workers additional protection from malaria and allow them to work as much as possible. We assume that using two technologies jointly offers more protection than using either alone:

Assumption 1. $\max(p^\Phi, p^\Psi) < p^{\Phi\cup\Psi}$,

where $p^{\Phi\cup\Psi}$ is the probability that at least one technology works, when both are in place. Assumption 1 says that using two technologies jointly cannot offer less protection than using either alone. This seems a reasonable assumption, in light of the evidence presented in **?** that combined use of IRS and ITNs reduces the probability of malaria infection more than use of either technology alone. They show that the protective efficacy of either technology is unaffected by the use of the other.

Every worker i is risk neutral, with utility function $U_i = Y_i - \phi_i d_i$, where ϕ_i is an indicator variable equal to 1 if worker i chooses to use Ψ and 0 otherwise, and

[3] Disutility may arise from a variety of factors that negatively impact ITN users, including: the need to hang the net over the bed every night; sleeping closer to other household members to fit more people inside a net; a reduction in ventilation during the hours of sleep; possible allergic reaction from contact with the insecticide on the ITN. Disutility thus defined may vary from person to person, as each individual may be more or less susceptible to different facets of the problem.

d_i represents an idiosyncratic disutility incurred by user i of technology Ψ. Notice that, once the disutility d is allowed to vary from person to person, there is no need to specify the utility function as $U_i = u_i(Y_i) - \phi_i d_i$ or as $U_i = u(Y_i) - \phi_i d_i$. We just need to avoid the case in which all workers choose the same ϕ_i in the utility maximization problem, which would occur if the utility function were $U_i = Y_i - \phi_i d$. Our specification accomplishes this goal in the simplest way.

Each worker chooses whether to use Φ, to maximize his own expected utility:

$$\phi_i^* \in \arg\max_{\phi_i \in \{0,1\}} E(U_i | \Psi) \tag{1}$$

In this simple model, we do not account for any externalities which may arise from others' use of ITNs; although they are potentially important, our main point can be made without mentioning them. In Section 5 we discuss why we do not account for externalities in our model.

2 Perfect information

In our setting with exogenous wage w, workers are actually maximizing their expected time endowment $E(time_i)$. Under perfect information, all workers know that the probability of malaria infection, π, is $\overline{\pi} > 0$ if they do not use any preventive technology. The government makes preventive technology Φ freely available to all who want it. The expected time endowment $E(time_i)$ of worker i depends on whether he uses Φ:

$$\begin{aligned} E(time_i) &= (1-\overline{\pi})T + \overline{\pi}\left\{(1-\phi_i)(T-t) + \phi_i[(p^{\Phi}T + (1-p^{\Phi})(T-t)]\right\} \\ &= (1-\overline{\pi})T + \overline{\pi}\left[(T-t) + \phi_i p^{\Phi} t\right] \\ &= T - \overline{\pi}t\left(1 - \phi_i p^{\Phi}\right) \end{aligned} \tag{2}$$

If no mosquitoes find and infect worker i, he will have full time endowment T, irrespective of his use of Φ. If instead a mosquito finds him, if he does not sleep under an ITN, he will lose time endowment t and will be left with $T-t$. Net use would grant him protection with probability p^{Φ}, preventing him from losing t.

Worker i will use technology Φ if and only if its use can increase his expected utility, which happens if the expected gains can compensate for the disutility incurred from its use:

$$
\begin{aligned}
\phi_i^* = 1 \quad &\Leftrightarrow \quad E(U_i|\phi_i = 1) > E(U_i|\phi_i = 0) \\
&\Leftrightarrow \quad wE(time_i|\phi_i = 1) - d_i > wE(time_i|\phi_i = 0) \\
&\Leftrightarrow \quad w\left(T - \overline{\pi}t + \overline{\pi}p^{\Phi}t\right) - d_i > w\left(T - \overline{\pi}t\right) \\
&\Leftrightarrow \quad w\overline{\pi}p^{\Phi}t - d_i > 0 \\
&\Leftrightarrow \quad w\overline{\pi}p^{\Phi}t > d_i
\end{aligned} \tag{3}
$$

Technology Ψ becomes available to the government, who can decide whether to introduce it in addition to technology Φ. Workers can observe the decision of the government. If Ψ is not introduced, the expected time available to worker i will remain unchanged and so will his decision about net use, so that:

$$E(time_i|\Psi = 0) \quad = \quad T - \overline{\pi}t\left(1 - \phi_i p^{\Phi}\right) \tag{4}$$

$$\text{If } \Psi = 0 \text{ then } \phi_i^* = 1 \quad \Leftrightarrow \quad w\overline{\pi}tp^{\Phi} > d_i \tag{5}$$

If Ψ is introduced, i.e., if $\Psi = 1$, the expected time available to worker i is:

$$
\begin{aligned}
E(time_i|\Psi = 1) \quad &= \quad (1-\overline{\pi})T + \overline{\pi}\left\{ \begin{array}{l} (1-\phi_i)[(p^{\Psi}T + (1-p^{\Psi})(T-t)] + \\ \phi_i[(p^{\Phi\cup\Psi}T + (1-p^{\Phi\cup\Psi})(T-t)] \end{array} \right\} \\
&= \quad (1-\overline{\pi})T + \overline{\pi}\left\{ \begin{array}{l} (T-t) + [p^{\Psi}T - p^{\Psi}(T-t)]^{1-\phi_i} \times \\ {[p^{\Phi\cup\Psi}T - p^{\Phi\cup\Psi}(T-t)]^{\phi_i}} \end{array} \right\} \\
&= \quad T - \overline{\pi}t[1 - (p^{\Psi})^{1-\phi_i}(p^{\Phi\cup\Psi})^{\phi_i}]
\end{aligned} \tag{6}
$$

As before, worker i will use technology Φ if and only if its use can increase his expected utility:

$$
\begin{aligned}
&\text{If } \Psi = 1 \quad \text{then} \\
&\phi_i^* = 1 \quad \Leftrightarrow \quad E(U_i|\Psi = 1, \phi_i = 1) > E(U_i|\Psi = 1, \phi_i = 0) \\
&\qquad\qquad \Leftrightarrow \quad wE(time_i|\Psi = 1, \phi_i = 1) - d_i > wE(time_i|\Psi = 1, \phi_i = 0) \\
&\qquad\qquad \Leftrightarrow \quad w(T - \overline{\pi}t + \overline{\pi}p^{\Phi\cup\Psi}t) - d_i > w(T - \overline{\pi}t + \overline{\pi}p^{\Psi}t) \\
&\qquad\qquad \Leftrightarrow \quad w\overline{\pi}p^{\Phi\cup\Psi}t - w\overline{\pi}p^{\Psi}t - d_i > 0 \\
&\qquad\qquad \Leftrightarrow \quad w\overline{\pi}t(p^{\Phi\cup\Psi} - p^{\Psi}) > d_i
\end{aligned} \tag{7}
$$

This means that, once IRS campaigns have been rolled out, workers will choose to sleep under an ITN if and only if the *additional* expected gains from its use can compensate for the disutility incurred from use of the technology.

To assess the relationship between conditions (5) and (7) and thus find conditions for use of Φ, we need to make an additional assumption about the relationship between the protection offered by Φ alone, p^{Φ}, and the additional protection Φ offers when Ψ is also available, $p^{\Phi\cup\Psi} - p^{\Psi}$. We explore two alternative assumptions.

The assumption that seems most sensible to us is that the additional protection offered by Φ when Ψ is also available cannot exceed that granted when Ψ is not offered. This assumption means that Φ and Ψ are imperfect substitutes and can be formalized as follows:

Assumption 2. $p^{\Phi\cup\Psi} \leq p^{\Phi} + p^{\Psi}$.

Proposition 1 follows:

Proposition 1. *If workers are perfectly informed that the probability of infection is* $\overline{\pi} > 0$ *if they do not use any preventive technology, and if technologies* Φ *and* Ψ *are (imperfect) substitutes, then average use of technology* Φ *cannot be higher if* Ψ *is introduced than if it is not, i.e.,* $\Pr(\theta^{\Psi} > \theta^{\Phi}) = 0$*, where* $\theta^{\Psi} \equiv E(\phi_i^*|\Psi = 1)$ *and* $\theta^{\Phi} \equiv E(\phi_i^*|\Psi = 0)$.

Proof. We have shown that if $\Psi = 0$ then $\phi_i^* = 1$ if and only if $w\overline{\pi}tp^{\Phi} > d_i$, and that if $\Psi = 1$ then $\phi_i^* = 1$ if and only if $w\overline{\pi}(p^{\Phi\cup\Psi} - p^{\Psi})t > d_i$. Assumption 2 implies that $p^{\Phi\cup\Psi} - p^{\Psi} \leq p^{\Phi}$. Notice now that condition (7) is stricter than (5), i.e. (7)$\Rightarrow$(5) but (5)$\not\Rightarrow$(7). Therefore, a worker who uses Φ when Ψ is available, would have certainly used it also in the absence of Ψ. Then average use of Φ cannot be higher if Ψ is introduced than if it is not, i.e. $\Pr(\theta^{\Psi} > \theta^{\Phi}) = 0$. $\square$

Consider now the case in which technologies Φ and Ψ are imperfect complements, i.e. demand for one technology increases with ownership of the other. Assumption 2 is then replaced by the following:

Assumption 3. $p^{\Phi\cup\Psi} \geq p^{\Phi} + p^{\Psi}$.

Proposition 2 follows:

Proposition 2. *If workers are perfectly informed that the probability of infection is* $\overline{\pi} > 0$ *if they do not use any preventive technology, and if technologies* Φ *and* Ψ *are (imperfect) complements, then average use of technology* Φ *cannot be lower if* Ψ *is introduced than if it is not, i.e.,* $\Pr(\theta^{\Psi} < \theta^{\Phi}) = 0$.

3 Imperfect information

In a more realistic setting, workers do not know the true value of π. Suppose that π can only take one of two values: 0 or $\overline{\pi} > 0$. This formulation simplifies considerably the structure of the problem, still capturing its essence, and it seems suitable to study the very low transmission environment under investigation.

Each worker i is endowed with a prior $P_i(\pi = \overline{\pi})$ drawn from a $Uniform(0, 1)$. Notice that $P_i(\pi = 0) = 1 - P_i(\pi = \overline{\pi})$. Workers believe that the provider of Ψ, i.e. the government, has perfect knowledge about π. Therefore, it is reasonable to assume that all individuals believe that the probability that the government sprays when the true risk of infection is 0 cannot exceed the probability that it does so when malaria poses a threat:[4]

Assumption 4. $\Pr(\Psi = 1|\pi = \overline{\pi}) \geq \Pr(\Psi = 1|\pi = 0)$.

The government makes preventive technology Φ freely available to all who want it, and technology Ψ is not yet available. The expected time available to worker i will be:

$$
\begin{aligned}
E(time_i) &= (1-p_i)T + p_i\left[(1-\overline{\pi})T + \overline{\pi}\left\{\begin{array}{l}(1-\phi_i)(T-t)+\\ \phi_i[(p^{\Phi}T + (1-p^{\Phi})(T-t)]\end{array}\right\}\right] \\
&= (1-p_i)T + p_i(T - \overline{\pi}t + \overline{\pi}\phi_i p^{\Phi}t) \\
&= T - p_iT + p_i(T - \overline{\pi}t + \overline{\pi}\phi_i p^{\Phi}t) \\
&= T - p_i\overline{\pi}t(1-\phi_i p^{\Phi}) \qquad (8)
\end{aligned}
$$

where $p_i \equiv P_i(\pi = \overline{\pi})$. Notice that condition (14) is analogous to (2), but for the presence of the extra weight p_i, which represents the prior that $\pi = \overline{\pi}$. Worker i will use technology Φ if and only if its use can increase his expected utility:

$$
\begin{aligned}
\phi_i^* = 1 \quad &\Leftrightarrow \quad E(U_i|\phi_i = 1) > E(U_i|\phi_i = 0) \\
&\Leftrightarrow \quad wE(time_i|\phi_i = 1) - d_i > wE(time_i|\phi_i = 0) \\
&\Leftrightarrow \quad w[T - p_i\overline{\pi}t(1-p^{\Phi})] - d_i > w[T - p_i\overline{\pi}t] \\
&\Leftrightarrow \quad w[T - p_i\overline{\pi}t + p^{\Phi}p_i\overline{\pi}t] - d_i > w[T - p_i\overline{\pi}t] \\
&\Leftrightarrow \quad w[p^{\Phi}p_i\overline{\pi}t] - d_i > 0 \\
&\Leftrightarrow \quad p_i w\overline{\pi}\, p^{\Phi}t > d_i \qquad (9)
\end{aligned}
$$

where $p_i \equiv P_i(\pi = \overline{\pi})$. Condition (16) says that worker i will use an ITN if

[4]People are aware that the government has successfully managed to drastically reduce malaria in recent years, and so they understand that it is committed to fight the disease. This makes the government "credible".

and only if the expected protection granted from its use can more than compensate from the disutility incurred. Compare (16) to (3) and notice that the new condition depends on the prior probability of malaria infection.

Technology Ψ becomes available to the government, who can decide whether to introduce it in addition to technology Φ. Workers can observe the decision of the government. Workers update their beliefs using Bayes' rule after observing the realization of Ψ. Lemma 1 describes how workers update their beliefs if they observe that the government has introduced Ψ, and Lemma 2 describes how workers update their beliefs if they observe that the government has *not* introduced Ψ.

Lemma 1. $P_i(\pi = \overline{\pi}|\Psi = 1) \geq P_i(\pi = \overline{\pi})$*, i.e., if the government introduces* Ψ*, the posterior probability of malaria infection* $P_i(\pi = \overline{\pi}|\Psi = 1)$ *cannot be smaller than the prior probability of malaria infection* $P_i(\pi = \overline{\pi})$*.*

Proof. Recall that, when workers observe Ψ, they update their beliefs using Bayes' rule:

$$\begin{aligned} P_i(\pi = \overline{\pi}|\Psi = 1) &= \frac{P(\Psi = 1|\pi = \overline{\pi})P_i(\pi = \overline{\pi})}{P(\Psi = 1)} \\ &= \frac{P(\Psi = 1|\pi = \overline{\pi})P_i(\pi = \overline{\pi})}{P(\Psi = 1|\pi = \overline{\pi})P(\pi = \overline{\pi}) + P(\Psi = 1|\pi = 0)P(\pi = 0)} \end{aligned}$$

By Assumption 4, workers also know that $P(\Psi = 1|\pi = \overline{\pi}) \geq P(\Psi = 1|\pi = 0)$.

Assume by contradiction that $P_i(\pi = \overline{\pi}|\Psi = 1) < P_i(\pi = \overline{\pi})$.

$$\begin{aligned} P_i & \; (\pi = \overline{\pi}|\Psi = 1) < P_i(\pi = \overline{\pi}) \\ \Leftrightarrow & \; \frac{P(\Psi = 1|\pi = \overline{\pi})P_i(\pi = \overline{\pi})}{P(\Psi = 1|\pi = \overline{\pi})P(\pi = \overline{\pi}) + P(\Psi = 1|\pi = 0)P(\pi = 0)} < P_i(\pi = \overline{\pi}) \\ \Leftrightarrow & \; P(\Psi = 1|\pi = \overline{\pi}) < P(\Psi = 1|\pi = \overline{\pi})P(\pi = \overline{\pi}) + P(\Psi = 1|\pi = 0)P(\pi = 0) \\ \Leftrightarrow & \; P(\Psi = 1|\pi = \overline{\pi})[1 - P(\pi = \overline{\pi})] < P(\Psi = 1|\pi = 0)P(\pi = 0) \\ \Leftrightarrow & \; P(\Psi = 1|\pi = \overline{\pi})P(\pi = 0) < P(\Psi = 1|\pi = 0)P(\pi = 0) \\ \Leftrightarrow & \; P(\Psi = 1|\pi = \overline{\pi}) < P(\Psi = 1|\pi = 0) \end{aligned}$$

Contradiction! $\square$

Following the introduction of Ψ, and given that worker specific disutility d_i is left unchanged, workers may revise their beliefs that $\pi = \overline{\pi}$ only upward: more workers may then choose to use Φ.

Lemma 2. $P_i(\pi = \overline{\pi}|\Psi = 0) \leq P_i(\pi = \overline{\pi})$*, i.e., if the government does not provide* Ψ*, the posterior probability of malaria infection* $P_i(\pi = \overline{\pi}|\Psi = 0)$ *cannot be larger than the prior probability of malaria infection* $P_i(\pi = \overline{\pi})$*.*

Proof. Workers update their beliefs using Bayes' rule after observing that the Government has not introduced Ψ:

$$\begin{aligned} P_i(\pi = \overline{\pi}|\Psi = 0) &= \frac{P(\Psi = 0|\pi = \overline{\pi})P_i(\pi = \overline{\pi})}{P(\Psi = 0)} \\ &= \frac{P(\Psi = 0|\pi = \overline{\pi})P_i(\pi = \overline{\pi})}{P(\Psi = 0|\pi = \overline{\pi})P(\pi = \overline{\pi}) + P(\Psi = 0|\pi = 0)P(\pi = 0)} \end{aligned}$$

Notice that Assumption 4 implies that: $P(\Psi = 0|\pi = \overline{\pi}) \leq P(\Psi = 0|\pi = 0)$.

Assume by contradiction that $P_i(\pi = \overline{\pi}|\Psi = 0) > P_i(\pi = \overline{\pi})$.

$$\begin{aligned} P_i & \quad (\pi = \overline{\pi}|\Psi = 0) > P_i(\pi = \overline{\pi}) \\ \Leftrightarrow & \quad \frac{P(\Psi = 0|\pi = \overline{\pi})P_i(\pi = \overline{\pi})}{P(\Psi = 0|\pi = \overline{\pi})P(\pi = \overline{\pi}) + P(\Psi = 0|\pi = 0)P(\pi = 0)} > P_i(\pi = \overline{\pi}) \\ \Leftrightarrow & \quad P(\Psi = 0|\pi = \overline{\pi}) > P(\Psi = 0|\pi = \overline{\pi})P(\pi = \overline{\pi}) + P(\Psi = 0|\pi = 0)P(\pi = 0) \\ \Leftrightarrow & \quad P(\Psi = 0|\pi = \overline{\pi})[1 - P(\pi = \overline{\pi})] > P(\Psi = 0|\pi = 0)P(\pi = 0) \\ \Leftrightarrow & \quad P(\Psi = 0|\pi = \overline{\pi})P(\pi = 0) > P(\Psi = 0|\pi = 0)P(\pi = 0) \\ \Leftrightarrow & \quad P(\Psi = 0|\pi = \overline{\pi}) > P(\Psi = 0|\pi = 0) \end{aligned}$$

Contradiction! $\square$

If the government chooses not to provide Ψ, and given that worker specific disutility d_i is left unchanged, workers may revise their beliefs that $\pi = \overline{\pi}$ only downward: fewer workers may then choose to use Φ.

Lemma 3. *Lemma 1 and Lemma 2 imply that* $P_i(\pi = \overline{\pi}|\Psi = 1) \geq P_i(\pi = \overline{\pi}|\Psi = 0)$.

Observation of the decision about the introduction of Ψ has implications for the computation of the expected time available to worker i and for his optimal choice to use Φ.

If the government introduces technology Ψ, the expected time available to worker i will be:

$$\begin{aligned} E & \ (time_i|\Psi = 1) = \\ &= (1 - P_i^1)T + P_i^1 \left[(1-\overline{\pi})T + \overline{\pi} \left\{ \begin{array}{l} (1-\phi_i)[(p^{\Psi}T + (1-p^{\Psi})(T-t)]+ \\ \phi_i[(p^{\Phi\cup\Psi}T + (1-p^{\Phi\cup\Psi})(T-t)] \end{array} \right\} \right] \\ &= (1 - P_i^1)T + P_i^1 \left\{ \begin{array}{l} (1-\overline{\pi})T + \overline{\pi}(p^{\Psi})^{1-\phi_i}(p^{\Phi\cup\Psi})^{\phi_i}T \\ +[1-(p^{\Psi})^{1-\phi_i}(p^{\Phi\cup\Psi})^{\phi_i}](T-t) \end{array} \right\} \\ &= (1 - P_i^1)T + P_i^1[T - \overline{\pi}t + \overline{\pi}(p^{\Psi})^{1-\phi_i}(p^{\Phi\cup\Psi})^{\phi_i}t] \\ &= T - P_i^1\overline{\pi}t[1-(p^{\Psi})^{1-\phi_i}(p^{\Phi\cup\Psi})^{\phi_i})] \end{aligned} \tag{10}$$

where $P_i^1 \equiv P_i(\pi = \overline{\pi}|\Psi = 1)$. Having updated their beliefs, workers will use Φ if and only if its use can increase their own expected utility:

$$\begin{aligned} \text{If } \Psi = 1 \text{ then } \phi_i^* = 1 \ &\Leftrightarrow \ E(U_i|\Psi = 1, \phi_i = 1) > E(U_i|\Psi = 1, \phi_i = 0) \\ &\Leftrightarrow \ wE(time_i|\Psi = 1, \phi_i = 1) - d_i > wE(time_i|\Psi = 1, \phi_i = 0) \\ &\Leftrightarrow \ Tw - P_i^1 w\overline{\pi}t(1 - p^{\Phi\cup\Psi}) - d_i > Tw - P_i^1 w\overline{\pi}t(1-p^{\Psi}) \\ &\Leftrightarrow \ P_i^1 w\overline{\pi}t(p^{\Phi\cup\Psi} - p^{\Psi}) > d_i \end{aligned} \tag{11}$$

where $P_i^1 \equiv P_i(\pi = \overline{\pi}|\Psi = 1)$.

If the government does not introduce technology Ψ, the expected time available to worker i will be:

$$
\begin{aligned}
E(time_i|\Psi=0) &= (1-P_i^0)T + P_i^0\left[(1-\overline{\pi})T + \overline{\pi}\left\{\begin{array}{l}(1-\phi_i)(T-t)+\\ \phi_i[(p^\Phi T + (1-p^\Phi)(T-t)]\end{array}\right\}\right] \\
&= (1-P_i^0)T + P_i^0(T - \overline{\pi}t + \overline{\pi}\phi_i p^\Phi t) \\
&= T - P_i^0 T + P_i^0(T - \overline{\pi}t + \overline{\pi}\phi_i p^\Phi t) \\
&= T - P_i^0\overline{\pi}t(1-\phi_i p^\Phi) \qquad (12)
\end{aligned}
$$

where $P_i^0 \equiv P_i(\pi = \overline{\pi}|\Psi = 0)$. Having updated their beliefs, workers will use Φ if and only if its use can increase their own expected utility:

$$
\begin{aligned}
\text{If } \Psi = 0 \text{ then } \phi_i^* = 1 &\Leftrightarrow E(U_i|\Psi=0,\phi_i=1) > E(U_i|\Psi=0,\phi_i=0) \\
&\Leftrightarrow wE(time_i|\Psi=0,\phi_i=1) - d_i > wE(time_i|\Psi=0,\phi_i=0) \\
&\Leftrightarrow Tw - P_i^0 w\overline{\pi}t(1-p^\Phi) - d_i > Tw - P_i^0 w\overline{\pi}t \\
&\Leftrightarrow P_i^0 w\overline{\pi}tp^\Phi > d_i \qquad (13)
\end{aligned}
$$

where $P_i^0 \equiv P_i(\pi = \overline{\pi}|\Psi = 0)$.

To summarize, we have shown that, under imperfect information about π, and after the realization of Ψ has been observed:

$$
\begin{aligned}
E(time_i|\Psi=0) &= T - P_i(\pi=\overline{\pi}|\Psi=0)\overline{\pi}t(1-\phi_i p^\Phi) \qquad (14) \\
E(time_i|\Psi=1) &= T - P_i(\pi=\overline{\pi}|\Psi=1)\overline{\pi}t[1-(p^\Psi)^{1-\phi_i}(p^{\Phi\cup\Psi})^{\phi_i}] \qquad (15) \\
\text{If } \Psi = 0 \text{ then } \phi_i^* = 1 &\Leftrightarrow P_i(\pi=\overline{\pi}|\Psi=0)w\overline{\pi}t\, p^\Phi > d_i \qquad (16) \\
\text{If } \Psi = 1 \text{ then } \phi_i^* = 1 &\Leftrightarrow P_i(\pi=\overline{\pi}|\Psi=1)w\overline{\pi}t(p^{\Phi\cup\Psi} - p^\Psi) > d_i \qquad (17)
\end{aligned}
$$

Conditions (16) and (17) are analogous to (5) and (7). From Lemma 3 we know that $P_i(\pi = \overline{\pi}|\Psi = 1) \geq P_i(\pi = \overline{\pi}|\Psi = 0)$. As in the perfect information case, the relationship between $(p^{\Phi\cup\Psi} - p^\Psi)$ and p^Φ depends on whether we make

Assumption 2 or 3: If Φ and Ψ are substitutes, $p^{\Phi\cup\Psi} - p^{\Psi} \leq p^{\Phi}$; If instead Φ and Ψ are complements, $p^{\Phi\cup\Psi} - p^{\Psi} \geq p^{\Phi}$.

Proposition 3. *In the imperfect information setting, if workers are Bayesian updaters and if Φ and Ψ are (imperfect) substitutes, i.e., if $p^{\Phi\cup\Psi} \leq p^{\Phi} + p^{\Psi}$, the share θ^{Ψ}, of workers who choose to use Φ once Ψ is introduced, may be larger or smaller than θ^{Φ}, the share of workers using Φ when Ψ is not introduced.*

Proof. Lemma 3 and Assumption 2 imply that (16) $\not\Rightarrow$ (17) and (16) $\not\Leftarrow$ (17). So it is possible that $\theta^{\Psi} < \theta^{\Phi}$, or that $\theta^{\Psi} = \theta^{\Phi}$, or that $\theta^{\Psi} > \theta^{\Phi}$. □

Notice in particular that $P(\theta^{\Psi} > \theta^{\Phi}) > 0$. This is contrast with the analogous result for the perfect information case, for which we showed that $P(\theta^{\Psi} > \theta^{\Phi}) = 0$.

Proposition 4. *In the imperfect information setting, if workers are Bayesian updaters and if Φ and Ψ are (imperfect) complements, i.e., if $p^{\Phi\cup\Psi} \geq p^{\Phi} + p^{\Psi}$, the share θ^{Ψ}, of workers who choose to use Φ once Ψ is introduced, cannot be smaller than θ^{Φ}, the share of workers using Φ when Ψ is not introduced.*

Proof. $P_i(\pi > 0|\Psi = 1) \geq P_i(\pi > 0)$ and $p^{\Phi\cup\Psi} \geq p^{\Phi} + p^{\Psi}$ imply that (16) $\Rightarrow$ (17) and (16) $\not\Leftarrow$ (17). So it is possible that $\theta^{\Psi} > \theta^{\Phi}$, or that $\theta^{\Psi} = \theta^{\Phi}$, but not that $\theta^{\Psi} < \theta^{\Phi}$. □

In this case we obtain the same prediction as in the perfect information case, i.e. that $P(\theta^{\Psi} > \theta^{\Phi}) > 0$.

4 Theoretical Predictions

Table 1 summarizes the predictions of this model.

5 Externalities

In this simple model, we have not accounted for any externalities which may arise from others' use of ITNs, i.e., we do not model $\Pr(\phi_i)$ as function of $\Pr(\phi_{-i})$, where

Table 1: Summary of the theoretical predictions

	Imperfect substitutes	Imperfect complements
Perfect Information	$\theta^{\Psi} \leq \theta^{\Phi}$	$\theta^{\Psi} \geq \theta^{\Phi}$
Imperfect Information	$\theta^{\Psi} \leq \theta^{\Phi}$ or $\theta^{\Psi} \geq \theta^{\Phi}$	$\theta^{\Psi} \geq \theta^{\Phi}$

Note: Average use of Φ depending on the complementarity between Φ and Ψ and on the availability of information about malaria prevalence. $\theta^{\Phi} \equiv E(\phi_i^*|\Psi = 0)$ and $\theta^{\Psi} \equiv E(\phi_i^*|\Psi = 1)$.

$-i$ includes all agents but i. We do so because it is unclear, in reality, which of the following arguments are most relevant to agents in their decision to adopt technology Φ and to sustain its use, and whether they keep any of such considerations into account at all.

1. First of all, the more people use nets, the less likely it is that mosquitoes will carry the disease, because mosquitoes do not *have* malaria, but they can only *transfer* malaria from infected persons to healthy persons.

2. Secondly, because ITNs are treated with insecticide, the more ITNs are used[5], the smaller the size of the mosquito population, the lower the need to sleep under an ITN.

3. Third, people may learn about the importance of using an ITN from their peers (e.g. their neighbors and the members of their tribe or religious group) so that the larger the group of adopters within a certain network, the more people are likely to follow their example.

From the first two channels points we see how increased ITN use in the community may put downward pressure on agents' individual ITN use. In the extreme case in which everyone else sleeps under an ITN, a person cannot benefit from doing so: in fact, mosquito bites can at worst be annoying but certainly not infectious,

[5] What matters for this effect is that they are not inside a container, but that they are hung in the house. This effect does not require people to actually sleep under a net.

as the vector cannot bite anyone else who has malaria[6]. If instead no one sleeps under an ITN, then a person benefits the most from doing so, because there are many mosquitoes and they are very likely to carry the disease. Finally, in an intermediate situation, such as the one we investigate in this paper, benefits from ITN use decline with the share of net users in the village.

We notice that the information campaigns conducted in Eritrea explain to the people that people can get malaria only from mosquito bites, that they should use ITNs to protect themselves from mosquitoes, and that the insecticide on ITNs can kill mosquitoes. These are simple messages, that even illiterate people can easily understand.

As a result of this information strategy, we believe that the people in our study area are *not* aware that mosquitoes are *solely* a vector, rather than the source of malaria, i.e., it seems reasonable to assume that people regard all mosquitoes as a source of malaria, irrespective of whether they have bitten an infected person. This consideration allows us to rule out the first channel. On the one hand, the second channel may be well understood by the people – though we have no data on this. If people understand that the more ITNs are used, the smaller the size of the mosquito population, incentives for net use will be small in villages with high usage rates. Finally, in the presence of network effects, agents may learn about the importance of ITN use from their peers and be more willing to sleep under an ITN when more community members do so.

Having no data on the importance and on the relative size of these two channels, and hence on the overall effect of average net use in the community on own net use, we prefer to exclude this consideration from our model, and we do not model $\Pr(\phi_i)$ as function of $\Pr(\phi_{-i})$. Richer data may help shed some light on this point, which future research shall try to address.

[6]Except outside the sleeping hours, in the evening and in the early morning.

Appendix 2: Data

May 9, 2012

For Online Publication

This section provides a definition of all variables used in "Do Public Health Interventions Crowd Out Private Health Investments? Malaria Control Policies in Eritrea" by P. Carneiro, A. Locatelli, T. Ghebremeskel and J. Keating.

1 Randomization checks

Variables available for all household members:

1. *female* is an indicator variable =1 if person is a female, and =0 otherwise.
2. *usually lives here* is an indicator variable =1 if person reportedly normally lives in the dwelling where the interview was conducted, and =0 otherwise.
3. *stayed there last night* is an indicator variable =1 if person reportedly spent the night before the interview in the dwelling where the interview was conducted, and =0 otherwise.
4. *age* is the reported age in years of the person, =0 if less than 1 year old.

Variables available for respondents only:

1. *age* is the reported age in years of the respondent.

2. *ever attended school* is an indicator variable =1 if respondent reportedly ever attended school, and =0 otherwise.

3. *only primary school* is an indicator variable =1 if respondent reportedly has some schooling but did not progress to secondary school; =0 if respondent has some schooling and progressed to secondary school; missing if respondent has no schooling, or if respondent has some schooling but educational achievement is not recorded in the data.

4. *literate* is an indicator variable =1 if respondent reportedly can read and write in one language without any difficulty, and =0 otherwise.

5. *Muslim religion* is an indicator variable =1 if respondent is Muslim, and =0 otherwise.

6. *Tigre tribe* is an indicator variable =1 if respondent belongs to the Tigre tribe, and =0 otherwise.

7. *married* is an indicator variable =1 if respondent is married, and =0 otherwise.

Variables available at household level:

1. *household size* is the number of members of the household at the time of the survey, including all people who normally eat and sleep together in the same dwelling (both present and away at the time of the survey) and any guests that were with the household at the time of the survey.

2. *household members under 5* is the number of household members (as defined at bullet point 1) whose age was not greater than 5 years.

3. *household members under 18* is the number of household members (as defined at bullet point 1) whose age was not greater than 18 years.

4. *main source of drinking water: public tap* is an indicator variable =1 if the main source of drinking water of the household was a public tap, and =0 otherwise.

5. *main source of drinking water: unprotected well* is an indicator variable =1 if the main source of drinking water of the household was an unprotected well, and =0 otherwise.

6. *main source of drinking water: unprotected spring* is an indicator variable =1 if the main source of drinking water of the household was an unprotected spring, and =0 otherwise.

7. *has any toilet* is an indicator variable =1 if dwelling has a toilet, and =0 otherwise.

8. *has radio* is an indicator variable =1 if household owns a radio, and =0 otherwise.

9. *firewood is main fuel* is an indicator variable =1 if firewood is the main fuel used by the household for cooking, and =0 otherwise.

10. *has no window* is an indicator variable =1 if dwelling has no windows, and =0 otherwise.

11. *number of separate rooms* is the number of separate rooms that compose the dwelling.

12. *number of sleeping rooms* is the number of separate rooms used for sleeping in the dwelling.

13. *number of sleeping spaces* is the number of sleeping spaces available inside the dwelling.

2 Compliance with treatment allocation

1. *Dwelling was sprayed in past 5 months* is an indicator variable =1 if dwelling was reportedly sprayed in the 12 months before the survey and this reportedly

happened no earlier than 5 months prior to the survey; =0 if dwelling was reportedly not sprayed in the 12 months before the survey, or if dwelling was reportedly sprayed in the 12 months before the survey and this reportedly happened more than 5 months prior to the survey; is missing if respondent does not know whether dwelling was sprayed in the 12 months before the survey, or if respondent was not asked whether dwelling was sprayed in the 12 months before the survey, or if the answer of the respondent is not recorded in the data.

2. *Dwelling was not sprayed in past 5 months* is an indicator variable = $1-$ *Dwelling was sprayed in past 5 months*.

3 Information and knowledge about malaria

1. *Mosquitoes mentioned among malaria vectors* is an indicator variable =1 if respondent mentioned mosquitoes answering the question "How does one get malaria?", and =0 otherwise. This variable is =1 also if respondent mentioned mosquitoes and additionally mentioned other incorrect options. Correct answer is: mosquitoes.

2. *Malaria is a problem in community* is an indicator variable =1 if respondent answered yes to the question "Is malaria a problem in this community?", and =0 otherwise. *Don't know* was recoded to missing.

3. *Children mentioned among most affected by malaria* is an indicator variable =1 if respondent answered "children" or "children and pregnant women" to the question "Who is most affected by malaria?", and =0 otherwise. Correct answer is: children and pregnant women.

4. *Pregnant women mentioned among most affected* is an indicator variable =1 if respondent answered "pregnant women" or "children and pregnant women"

to the question “Who is most affected by malaria?”, and =0 otherwise. Correct answer is: children and pregnant women.

5. *In the previous 6 months, heard/saw messages about: ITNs* is an indicator variable =1 if respondent answered yes to the question “During the last six months have you heard or seen any messages about insecticide treated mosquito nets?”, and =0 otherwise.

6. *In the previous 6 months, heard/saw messages about: early seeking behavior* is an indicator variable =1 if respondent answered yes to the question “During the last six months, have you heard or seen any messages about early seeking behavior for malaria treatment?”, and =0 otherwise.

7. *In the previous 6 months, heard/saw messages about: environmental management* is an indicator variable =1 if respondent answered yes to the question “During the last six months, have you heard or seen any messages about environmental management to control mosquitoes?”, and =0 otherwise.

4 Ownership and use of mosquito bed nets

In this Section we provide a clear definition of ITNs, which are a subset of all bed nets:

Definition 1. *We include in the definition of “ITNs”: all Long Lasting Insecticide treated Nets (LLINs), which were distributed in the area starting from 2006 and whose insecticide is effective for 3–5 years; all ITNs acquired in the 3 years prior to the survey (which are most likely LLINs, since the government distributed exclusively LLINs since 2006); and all ITNs that were re-treated in the 12 months before the survey (i.e., the answer to the question “How long ago was the net last soaked or dipped? If less than 1 month ago, record 00 months.” was not >11 months), in accordance with NMCP guidelines.*

1. *Number of nets owned by household* = number of bed nets reportedly owned by household, including 0 if household had none.

2. *Number of ITNs owned by household* = number of ITNs (see definition 1) owned by household, including 0 if household had none.

3. *Willingness to pay for an ITN, having none (Nakfa)* = reported maximum willingness to pay (in Eritrean currency, called Nakfa. 1 US dollar = 15 Nakfa) for a bed net. This question was asked only to respondents who reported having no bed nets and who answered *yes* to the question "Would you be willing to pay for a bed net?". Answers were recoded from missing to 0 if respondent reported having no bed nets and answered *no* to the question "Would you be willing to pay for a bed net?".

4. *Reported net use (of each household member)* =1 if person reportedly slept under a bed net the night before the survey, and =0 otherwise.

5. *Number of observed nets used the night before* = number of bed nets observed during survey and reportedly used the night before the survey by at least one household member.

6. *Number of observed nets left unused the night before* = difference between the total number of nets observed during the survey and the number of observed nets used the night before.

7. *"Full" net coverage (≥ 1 net per 1.5 household members)* is an indicator variable =1 if the ratio of the number of household members to the number of owned bed nets is not greater than 1.5, and =0 otherwise.

8. *"Adequate" net coverage (≥ 1 net per 2 household members)* is an indicator variable =1 if the ratio of the number of household members to the number of owned bed nets is not greater than 2, and =0 otherwise.

5 Participation in larval habitat management (LHM)

Remark 1. *For all variables, "don't know" was recoded to missing in order to obtain indicator variables.*

Remark 2. *Due to an incorrect skip instruction, no further information on LHM was recorded if the respondent reported not participating in LHM during the previous 6 months.*

1. *Respondent participated in LHM* =1 if respondent answered *yes* to the question "In the past six months, have you participated in environmental management in the village?", and =0 otherwise.

2. *Days spent by household in LHM* =1 if respondent answered *yes* to the question "For how many days did your household participate during the last month?", and =0 otherwise.

3. *Household members who participated in LHM* = total number of household members who participated in LHM during the last month. Missing values were recoded to 0 because only positive numbers were recorded in the data. Answers "don't know" were recoded to missing.

4. *Male household members >15 years old who participated in LHM* = number of male household members older than 15 who participated in LHM during the last month. Missing values were recoded to 0 because only positive numbers were recorded in the data. Answers "don't know" were recoded to missing.

5. *Female household members >15 years old who participated in LHM* = number of female household members older than 15 who participated in LHM during the last month. Missing values were recoded to 0 because only positive numbers were recorded in the data. Answers "don't know" were recoded to missing.

6. *Household members <15 years old who participated in LHM* = = number of household members younger than 15 who participated in LHM during the last month. Missing values were recoded to 0 because only positive numbers were recorded in the data. Answers "don't know" were recoded to missing.

6 Behaviors conducive to malaria eradication, other than LHM

1. *Household keeps livestock >100m from home* is an indicator variable = 1 if respondent answered *yes* to the question "Are these animals kept 100 metres or less from your house?", and =0 otherwise. Answer *don't know* was recoded to missing. This question was asked only if respondent answered *yes* to the question "Do you have livestock such as goats, sheep or camels etc?").

2. *Household covers stored water* is an indicator variable = 1 if respondent answered yes to the question "Is the stored water covered?", and =0 otherwise. Answer *don't know* was recoded to missing. This question was asked only if respondent answered *yes* to the question "Does this household usually store water for domestic use?".

3. *Respondent does anything to prevent mosquito bites* is an indicator variable =1 if respondent answered *yes* to the question "Do you do things to stop mosquitoes from biting you?", and =0 otherwise.

Question "What do you do to stop mosquitoes from biting you?" was asked only if respondent answered *yes* to the question "Do you do things to stop mosquitoes from biting you?". Variables 4–9 in Table 8 "Behaviors conducive to malaria eradication, other than LHM" are indicator variables =1 if they were mentioned among the possible answers to the question "What do you do to stop mosquitoes from biting you?", and =0 if not mentioned or if respondent answered *no* to the question "Do you do things to stop mosquitoes from biting you?".

7 Intra-household allocation of bed nets

The dependent variable *Net Use* is the same variable called *Reported net use (of each household member)*, defined at bullet point 4 in Section 4 of this Appendix.

Appendix 3: Additional Data Analysis

May 9, 2012

For Online Publication

This appendix complements Sections "Data" and "Data Analysis" in "Do Public Health Interventions Crowd Out Private Health Investments? Malaria Control Policies in Eritrea" by P. Carneiro, A. Locatelli, T. Ghebremeskel and J. Keating.

1 Malaria prevalence

At the time of the survey, all present and consenting household members were tested for malaria using Carestart® rapid diagnostic tests (RDT), and microscopy was used to validate positive RDT results. Table 1 shows that 5,502 people were tested with RDT. 1,120 people were absent at the time of the survey – mainly youth in school age (56%), and adult working men (20%) and unemployed men (10%) – and they could not be tested. In addition, 651 people refused testing – mostly school age youths (46%) and children under the age of five (33%).

Among those tested, 13 individuals tested positive in the control group and 17 tested positive in the treatment group. Table 2 shows that the difference in the share of positive RDTs between the two groups is very small (and positive) and not significant.

Malaria prevalence was extremely low in the area under investigation, but recall that this study was conducted in an area where malaria prevalence was drastically reduced over the past decade, to understand whether the introduction of IRS on top of the existing package of intergraded interventions was an effective strategy to eradicate malaria completely.

We notice that these figures are in line with those provided by the NMCP of Eritrea. The total[1] number of malaria cases registered in Gash Barka in 2008 was 20,320, and the population of the region was estimated at 670,000. Therefore, the share of the population having malaria (and reporting to a health facility) was about 3% in 2008. We tested 5,502 people in the survey, so the expected number of malaria cases among them over the whole year is 165, i.e., 3% of 5,502. Positive RDTs indicate a malaria infection that occurred in the month prior to the test. Between 2002–2007, the percentage of malaria cases occurred in September (i.e., roughly in the month before our survey) was 15%. So, finally, the expected number of positive RDTs at the beginning of October was 25, i.e., 15% of 165. The number of positive RDTs in our sample is a bit larger than this, possibly because not all malaria patients report to health facilities, so that official figures may provide underestimates of the real number of malaria cases. In addition, our survey was conducted in the most malarious villages of the region.

2 Use of bed nets in the absence of IRS

Table **??** shows that, in the absence of IRS (in control villages), net usage varies greatly by age and employment status: children under 5 are the most likely to sleep under a bed net (50%), followed by school age youths (36%), unemployed and employed women in working age (44 and 40%) and finally by employed and unemployed adult men (27 and 24%). No significant gender differences were observed among children under five or among young people. Among employed adults, women are much more likely to sleep under a bed net (+13%) and the same is true among the unemployed (+20%).

[1] Sum of IPD (in patient department) and OPD (out patient department) cases.

3 Heterogeneous treatment effects

It is possible that the impact of IRS varied across groups of individuals or households. E.g., households residing in more arid areas may have reacted differently from those living in villages with more vegetation, either because the direct impact of spraying is different across areas, or because the role of information and perceptions varies. Similarly workers may have been impacted in a different way from the unemployed,[2] because they have more to lose more from a malaria infection. We also investigate whether the IRS campaign impacted poorer households differently than wealthier households.

In Section 3.1 we introduce the vegetation variables used in our analysis, and in Section 3.2 we explain how we construct our wealth index. Data analysis is presented in Section 3.3, which concludes this appendix.

3.1 Vegetation index (NDVI)

In the absence of data on the exact location and altitude of each village, we complement our dataset with subzone[3] level panel data on a vegetation index called Normalized Difference Vegetation Index (NDVI). The NDVI is obtained from the analysis of the color spectrum of satellite imagery, and it ranges between -1 and 1. In the absence of water surfaces or snow, it ranges between 0 and 1, where

[2]Our definition of unemployed includes those adults (>20 years old) who are out of the labor force as well as those who are enrolled in National Service. During the training sessions that preceded data collection there was a discussion regarding the employment question "Is (NAME) currently working?". Allowed answers included "1. Unemployed", "2. Self-employed", and "3. Employed". Trainees and supervisors suggested that National Service, not being allowed as an answer, should have been regarded as unemployment, and it was agreed to do so in the field. So, our definition of unemployed includes also those in National Service. A problem arises here in that the size of National Service is very large in Eritrea. It is compulsory for some years for all young people of the country, for men and women alike, and it continues for many well into their thirties or forties. The salary provided to people in National Service is very low, and therefore it was deemed to be a form of unemployment.

[3]*Subzones*, also called *subzobas*, are the districts that compose Zone Gash Barka, where the intervention was conducted. A description of the area under investigation is presented in Appendix 4

1 means most vegetation and 0 stands for least vegetation. This index has been shown to be very highly correlated with the species of malaria called *Plasmodium falciparum*, which accounts for more than 80% of malaria inflections in Eritrea (Shililu, Ghebremeskel, Seulu, Mengistu, Fekadu, Zerom, Asmelash, Sintasath, Mbogo, Githure, Brantly, Beier, and Novak (2004)), and generally measures the overall propensity of an area to harbor mosquito populations and thus proxies transmission intensity. NDVI has been used extensively in the literature to model malaria transmission and to forecast epidemics (see, e.g., Nihei, Hashida, Kobayashi, and Ishii (2002) and Gaudart, Toure, Dessay, Dicko, Ranque, Forest, Demongeot, and K Doumbo (2009), discussed in the following).

Vegetation data was retrieved from the website of the International Research Institute for Climate and Society (IRI) of Columbia University, which provides free Interactive maps of the Normalized Difference Vegetation Index (NDVI) for Africa (http://iridl.ldeo.columbia.edu/maproom/.Health/.Regional/.Africa/.Malaria/.NDVI/). Selecting East Africa and zooming-in on Eritrea, it is possible to display the districts, i.e. zones and subzones. For zone Gash Barka only, we downloaded the available time series for each subzone, over the entire available period. The website reports that its data source is the United States Geological Survey, Land Processes Distributed Active Archive Center, Moderate Resolution Imaging Spectroradiometer (USGS LandDAAC MODIS).

We focused on data from the period 2000–2009 and we kept in mind the conclusions of Gaudart et al. (2009), who use NDVI from 1981–2006 to assess the statistical relationship between NDVI and the incidence of *P. falciparum* in Sudan (which borders on Eritrea); they find that the seasonal pattern of *P. falciparum* incidence is significantly explained by NDVI and they also identify a threshold NDVI value of 0.361, above which an increase in the incidence of parasitemia is predicted. Similary, Nihei et al. (2002) study 1997 NDVI data from the Indochina Peninsula and find that *P. falciparum* Malaria is most prevalent in regions with NDVI >0.4 for at least 6 consecutive months.

Following Gaudart et al. (2009),[4] for each subzone we counted the number of 2–week periods in which NDVI exceeded 0.361. We also tried a lower threshold of 0.3 to allow for a possibly lower threshold in the context of Eritrea. Tables A and B in Figure 1 report the number of 2-week periods with NDVI above the threshold shown in the table header. Cells are colored from red to green (or blue), from the lowest to the highest value. Red means arid, while green (or blue) means with more vegetation.

Based on these two similar tables, we assigned a value $ndvi \in \{0, 1, 2\}$ to each subzone, where 0 hints to "very limited vegetation", 1 stands for "some vegetation" and 2 means "with significant vegetation". The resulting classification of subzones is presented in Table 4.

3.2 Wealth Index

As is standard in Demographic and Health Surveys (DHS), we construct an asset index using factor analysis, exploiting the information available in our dataset. In particular, we use data on households' main water source, toilet type, fuel used for cooking, wall and roof material, presence and type of any windows, access to electricity, ownership of electronics and any vehicles, size of the dwelling[5] and ownership of any livestock.

We obtain an index, which seems to describe well the differences between poorer and richer households (despite the fact that it explains just about 5% of the total variance of the variables used to construct it). In particular, conducting a comparison by wealth quintile, as households become wealthier: they source their water from a tap rather than from an unprotected well; they use a latrine rather than going to the bushes; they cook not only with firewood, but also with more expensive charcoal; their walls are made far less often in cane and wood, but rather in stone

[4] Over the whole time period covered by our data, in no subzone was an NDVI over 0.35 observed for more than 5 consecutive months, so the results of Nihei et al. (2002) cannot be applied in our setting.

[5] Number of persons per room is used as a proxy.

and cement; roofs are more solidly made in stone and cement, rather than in leafs; dwellings have windows, often with shutters, unlike their poorer versions which have none; they have access to electricity and a the majority has a radio; and some even have some vehicles, and especially a bike or a cart.

Our dataset includes information on household expenditure, which we do not use for this paper. Comparing household expenditure by wealth quintile we find that it progressively increases from 625 to 750 Nakfa[6] if we look at overall food expenditure, and from 725 to 1000Na considering total per-capita expenditure. As expected, per-capita expenditure for basic food is roughly constant over the whole distribution, at about 500Na per month, and the expenditure share spent on food decreases from 84% to 70%. This provides further evidence that our wealth index provide a suitable proxy for the actual unobservable household wealth.

However, dwellings do not become bigger relative to household size, leaving the ratio of persons per room to about 4:1. Finally, livestock ownership seems to be most common in the 3rd and 4th quintile, possibly because the richest quintile is not engaged in farming but in more productive activities. Details on the construction of the wealth index are presented in Section 3.2.1.

3.2.1 Construction of the Wealth Index

We constructed our wealth index following the method suggested by Filmer and Pritchett (2001), which has become the standard in DHS. To do so, we exploited the data we have on: main water source and fuel for cooking; presence and type of any toilet facility and of any windows; main material of the walls and of the roof; access to electricity and ownership of any consumer electronics, e.g. radio and TV; ownership of any vehicles; livestock ownership; and number of household members per room.

Ownership variables are dichotomous. Following the Filmer-Pritchett (FP) procedure, we split all categorical variables into sets of dummy variables, and we use

[6] 1 USD = 15 Nakfa.

Principal Components Analysis (PCA)[7] to assign the indicator weights. Finally, we use only the first factors produced by PCA to represent our wealth index, as suggested by McKenzie (2005)[8].

In Sections 3.2.2 and 3.2.3 we discuss some problems related to the use of PCA for the construction of the wealth index, and to the use of the FP procedure in particular. In Section 3.2.4 we discuss alternative methods to conduct PCA. Section 3.2.5 reports some checks we conducted on the internal coherence of the weights used for our wealth index. Overall, our analysis suggests that this may be a good proxy for household wealth, so in Section 3.2.6 we conclude explaining why we prefer the FP method over the other possibilities we explored.

3.2.2 Issues with PCA

McKenzie (2005) highlights the importance of using a wide-enough range of asset variables for the construction of a wealth index with PCA. A narrow range may result in two problems called *clumping* and *truncation*. Clumping (or clustering) occurs when the wealth index groups households into a limited number of groups. Truncation arises from limited variation in asset ownership, which may makes it hard to distinguish groups with small wealth differences. This could be an important problem if we were interested in distinguishing several degrees of poverty. Notice finally that the difficulty in distinguishingly household by socio-economic status (SES) may arise from the fact they are actually homogenous along the wealth margin.

These two problems could arise in the Eritrean setting we are considering. In Zone Gash Barka, in fact, asset ownership is very limited and the range of owned asset is quite narrow. Most dwelling are similar and most households do not have toilets. Also, almost no one has electricity. This situation may make it hard to group our households by wealth level.

[7] We use the *factor* command in STATA 10.1 for PCA.

[8] McKenzie (2005) considered using more than one PC to characterize socio-economic status and he concluded that the first PC was enough as a wealth measure.

Our situation is akin to that faced by Vyas and Kumaranayake (2006), who analyze villages in rural Ethiopia and raise analogous issues. As a possible solution, they stress the importance of including additional variables that can capture intra-household inequality, if available. Our wealth index relies on all available assets information contained in our dataset.

3.2.3 Issues with the Filmer-Pritchett procedure

Several issues can arise using the FP procedure for PCA. First of all, Kolenikov and Angeles (2009) discuss why the FP procedure should not be used for the analysis of discrete data. Distributional assumptions are violated because the procedure assumes that variables are continuously distributed.

Problems relating to high skewness and kurtosis are also likely to arise in the analysis of discrete data with little or no variation. This happens also in our case, and Figure 2 shows that the wealth distribution is indeed skewed to the right. Vyas and Kumaranayake (2006) have a similar problem in Ethiopia, and they suggest that this shape highlights the extent of clumping and truncation, so that it may be hard to distinguish socio-economic groups.

Further, wealth indexes obtained from FP PCA often explain only a small percentage of the variance in asset ownership, 5% in our case. We also try to collapse asset categories that include a very small number of households into broader categories, and even so the percentage of explained variance raises to just about 7%. These figures are quite low, also compared to the studies surveyed in Vyas and Kumaranayake (2006), where the first PC accounted for 12–27% of the total variation.

3.2.4 Alternative PCA methods

Kolenikov and Angeles (2009) use montecarlo simulations and a DHS dataset from Bangladesh to compare three methods to conduct PCA:

1. The mainstream PCA methodology that follows Filmer and Pritchett (2001) in splitting categorical variables into dummies, e.g., dwelling materials and

water source;

2. The use of ordinal variables, depending on quality of, e.g., dwelling materials and water source;

3. The use of polychoric correlations for PCA.

They conclude that the polychoric method 3 should not be used, unless we want to estimate precisely the proportion of explained variance for important reporting or decision making purposes. This is the only gain it offers compared to method 2. Between methods 1 and 2, Kolenikov and Angeles (2009) recommend using the latter wherever possible, exploiting the information available from the ordering. They argue that this improves the goodness of fit and limits the extent to which we under-estimate the explained variance.

3.2.5 Internal coherence

We checked whether the wealth index we obtained from FP CPA was sensible. To do so, with divided households by wealth quintile and we checked whether ownership of assets and quality of dwelling materials increased with SES. From Tables 5–7 we can *generally* see that, as wealth increases: water sources improve; households have better toilets and use bushes less often; they use not only firewood to cook, but also electricity and fuels; they have more solid walls (not made in wood or cane but more often in cement, bricks or stone) and roofs (made in cement or stone rather than leafs); own electronics, especially a radio, and hence have better access to information; they also have some vehicles, mainly bikes and carts. Finally, the number of persons per room does not change much.

There are however some instances in which we expect ownership to increase over SES, while the opposite is observed. In other cases, ownership initially increases and then decreases as households become wealthier, while monotonicity is expected. The main explanation lies in that the FP procedure works with dichotomous variables only and does not exploit the ordinal information available in the data; as a result, it only pays attention to the number of individuals that own an asset or not, irrespective of its quality and worth.

In particular, the scores assigned by the FP procedure to different materials and assets owned should increase in their quality and worth. E.g., car ownership should receive a higher score than bike ownership, and cement walls a higher score than weaker bamboo walls. This property held in the analysis of Filmer and Pritchett (2001), but it is possible that it fails.[9]

We can use the last column of Tables 5–7 to check whether the weights given by PCA have the sign we expect and are monotonically increasing in assets quality. Table 7 shows that ownership of any electronics is associated with a large positive weight: only radios are owned by a non-trivial population share, and this increases monotonically over wealth quintiles. Vehicles ownership also receives positive weights, and their ownership increases with SES. However more expensive electronics and vehicles do not receive a higher weight, probably as a result of the extremely low ownership rates in the data. The number of rooms and livestock ownership receive almost no weight.

Tables 5 and 6 show that our wealth index can account for: better access to water, especially comparing public tap to wells and springs as sources; better toilet facilities, keeping in mind that bushes are the most common option in all wealth quintiles, on a decreasing trend with SES; more expensive cooking fuels, whereas poor people use firewood; stronger wall and roof types, made of stone, cement and adobe rather than canes, wood planks and leafs; and finally for the presence of any windows and their quality.

[9]For example, a negative weight is assigned to bike ownership in McKenzie (2005) and wall types of better quality are sometimes given smaller weights in Kolenikov and Angeles (2009). McKenzie concluded that this problem was not severe in his case, because the weight with the wrong sign was actually small and thus unlikely to affect the results. An important problem of internal incoherence is found instead by Vyas and Kumaranayake (2006) in data from rural Ethiopia.

3.2.6 Discussion

The evidence presented in Section 3.2.5 tells us that the simple wealth index obtained from PCA following Filmer and Pritchett (2001) does a pretty good job in terms of explaining variation in SES among our households, in spite of all criticism moved to this approach (presented in Section 3.2.3).

It is true that we do not explicitly exploit any ordinal information on our asset variables. Assigning appropriate weights is a daunting task and we find arbitrary the suggestion from Kolenikov and Angeles (2009) to recode categorical answers as 1,2,3. . . , so we prefer to follow the FP procedure.

Our index seems to explain only 5% of total variation. Kolenikov and Angeles (2009) show that the explained variance is severely underestimated using the FP method (and more so, the more categories are contained in the original variables). Our estimate of the share of total variation explained is probably a large underestimate, given how well it can qualitatively describe variation in asset ownership.

Indeed, we may have too many categories in our variables, and we may collapse those with few households. We tried this exercise, which we also found arbitrary in the definition of the larger categories, and we did not gain in terms of internal coherence or explained variance, always below 7%.

For all these reasons, we chose to construct our wealth index using principal component analysis á la Filmer and Pritchett.

3.3 Data analysis

As we said, it is possible that the impact of IRS varied across groups of individuals or households. First of all, we analyzed this possibility for the case of the information outcomes, i.e., malaria awareness and knowledge that mosquitoes are the vectors. Tables 8–9 report in column 1 the estimates of homogeneous treatment effects obtained from probit regression (1), and in columns 2–5 the estimates of heterogeneous treatment effects obtained from probit regressions (2) and (3), which allow the impact of IRS to vary depending on the local vegetation level (introduced in Section 3.1 of this Appendix) and on the employment status[10] of the respondent:

$$\Pr(Y = 1) = \Phi(\alpha + \beta T + \gamma X) \quad (1)$$

$$\Pr(Y = 1) = \Phi(\alpha + \beta_0 T + \beta_1 T \times (ndvi = 1) + \beta_2 T \times (ndvi = 2) + \gamma X) \quad (2)$$

$$\Pr(Y = 1) = \Phi(\alpha + \beta_0 T + \beta_1 work + \beta_2 T \times work + \gamma X) \quad (3)$$

where T is short for $Treatment$, $X = (female, Tigre\ tribe, Muslim, subzone\ dummies)$; $ndvi$ is a variable =0 if a subzone is arid, =1 if it has some vegetation and =2 if it has significant vegetation; and $work$ is an indicator variable =1 if respondent works (i.e., they are employed or self-employed) and =0 otherwise (i.e., they are unemployed, in National Service, or out of the labor force). We omitted from model (2) the main effects for $ndvi$, to avoid collinearity with subzone dummies.

Our estimates in Table 8 suggest that malaria awareness did not change on average in any vegetation area, but we do find a significant 10% increase among workers (column 3). In columns 4 and 5 of this table, we restrict the sample in turn to men and women in working age and we find similar estimates; however the sample size is now smaller, so standard errors are higher and estimates are not significant.

Table 9 shows that knowledge that mosquitoes are a malaria vector increased on average by 3%, and that the increase was concentrated among respondents living in subzones with more vegetation. Knowledge increased especially among the unemployed (+4.68%), and particularly among unemployed men (+11.1%). Overall, these results suggest that while the unemployed learned that mosquitoes are the vector, it is workers who became more worried about malaria.

Secondly, we looked for heterogenous treatment effects on net ownership. Table 10 shows that households with literate heads,[11] or whose head ever went to school,

[10] See footnote 2.

[11] We use respondents as a proxy for household heads, because this information is available for all respondents but not for all households heads. 62% of respondents were household heads and 34% of respondents were partners of the head. We replicated these regressions including and excluding respondents who are not the head or the spouse. Their inclusion does not affect the estimates, so we

acquired significantly more nets than those with an illiterate head, or whose head never went to school. We estimate an increase in net ownership of 0.35–0.49 nets for the former group vs. only 0.16–0.20 for the latter. Only households with an employed head increased their stock of nets (+0.31), as the others could probably not afford to. We expected some difference across tribes and religions, due e.g. to different traditions and sleeping patterns: the largest treatment effect (+0.37) is observed in the Tigrigna tribe, which is the only non-Muslim tribe in the area, while increases were at best modest among Muslim tribes, such as the Tigre.

From Table 11 we can see that the treatment effect was only slightly larger in male-headed households than in female-headed ones (+0.24 vs +0.21). Households without children under five, who have significantly less nets in the absence of treatment, acquired 0.28 new nets on average, while a smaller increase of 0.19 nets is observed among households with young children. Finally, we checked whether treatment effects varied depending on households' wealth. The poorest households did not (or could not) increase their stock of nets, while an increase of about 0.40 units is generally observed among wealthier households.

In the main body of the paper we study how the IRS intervention affected intra-household allocation of bed nets and we show that use increased among workers, especially men, and that it did not decrease among the groups that are deemed at highest risk from malaria, i.e., children under the age of five and adult women (including pregnant women, which our dataset does not allow us to identify).

4 IV estimation

In the main text we report simple comparisons between treatment and control communities. Given that compliance with spraying was not perfect, we additionally report in this section instrumental variable estimates of the impact of IRS on various outcomes, where each household's participation in spraying is instrumented by

use the unrestricted sample.

the community level treatment indicator. In particular we estimate the coefficient β in equation (4) using two-stage least-squares:

$$\begin{aligned} Y &= \alpha + \beta \, Spray5months + \gamma_1 \, Tigre\, Tribe \\ &\quad + \gamma_2 \, Muslim + \gamma' \, Subzones + X_{other}\lambda + u \qquad (4) \\ Spray5months &= \theta_1 + \theta_2 \, Treatment + \theta_3 \, Tigre\, Tribe \\ &\quad + \theta_4 \, Muslim + \zeta' \, Subzones + X_{other}\xi + v \qquad (5) \end{aligned}$$

where $Spray5months$ is an indicator variable that takes value 1 if the dwelling of household i was sprayed with insecticide in the five months before the survey, and 0 otherwise.

Estimates are reported in tables 12–15. The first two columns of each table present means and standard deviations for each variable, for treatment and control villages. The remaining columns report differences (and corresponding standard errors) between treatment and control villages using three different specifications (which, given our experimental design, we interpret as the impact of the program). The first specification does not account for any control variables, and therefore corresponds to a simple difference in means between the two sets of villages. The second and third specifications include, respectively, a very simple set of control variables (dummy indicating whether an individual belongs to the Tigre tribe,[12] a dummy indicating Muslim religion, and dummies for subzone of residence), and a more complete set of control variables which includes all the variables we analyzed in the randomization checks (which we call X_{other} in equations (4) and (5) above).[13]

We also estimate the impact of the intervention on the intra-household allocation of bed nets using regression (4), letting Y be a dummy for net use, and restricting the sample in turn to each socio-demographic category. Estimates are presented in table 16. For each socio-demographic group, the first two columns of table 16 present

[12] This is the main tribe in Gash Barka and it is over-represented in treatment villages.

[13] School enrolment is excluded because it is recorded only for children in school age.

average bed net use in treatment and control villages, with standard deviations in parentheses. The remaining three columns present the impact of the intervention on the intra-household allocation of bed nets, with the same sets of controls used in tables 12–15 .

The estimates presented in tables 12–16 are very similar to those presented in tables 5–9 in the paper. Our main conclusions are essentially unchanged.

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Table 1: Malaria status (tested with rapid diagnostic tests RDT)

Status	Control group	Treatment group	Row total
Positive RDT	13	17	30
Negative RDT	2,617	2,855	5,472
Away (not tested)	519	601	1,120
Refused RDT	384	267	651
Missing values	340	282	622
Total	3,873	4,022	7,895

Table 2: Treatment effect on positive RDT malaria cases

			$E(Y\|T=1,X) - E(Y\|T=0,X)$	
Variables	Treatment	Control	No Regressors	Basic Regressors
Positive RDT	0.006	0.005	0.001	0.001
	(0.077)	(0.070)	(0.003)	(0.002)
Observations	2,872	2,630	5,502	4,664

Note: Sample restricted to individuals tested for malaria using Rapid Diagnostic Test (RDT). Columns 1 and 2 report means for treatment and control groups. Standard deviations are reported in parentheses. Differences in Columns 3 and 4 estimated using probit regressions, for which marginal effects are reported. The difference in Column 3 is estimated without any controls. The difference in Column 4 is estimated including the following set of controls: dummy for Tigre tribe, dummy for Muslim religion, and subzone dummies. Observations are clustered at village level. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Average use of bed nets in different demographic groups in the absence of IRS

Subsample:	All	Men	Women	Difference
Children under 5	0.50	0.51	0.48	-0.03
	(0.50)	(0.50)	(0.50)	(0.03)
Youth aged 5–20	0.36	0.34	0.38	0.05
	(0.48)	(0.47)	(0.49)	(0.03)
Adult workers	0.31	0.27	0.40	0.13***
	(0.46)	(0.44)	(0.49)	(0.03)
Adult unemployed	0.39	0.24	0.44	0.20***
	(0.49)	(0.43)	(0.50)	(0.03)

Note: in this table, "nets" refers to any bed nets, irrespective of their treatment status. Sample restricted to the control group. Columns 1–3 report average net use, with standard deviations in parentheses. Sample restricted to male individuals in Column 2. Sample restricted to female individuals in Column 3. Column 4 reports the difference in average net use between women and men estimated using LS regression; robust standard errors are reported in parentheses. Observations are clustered at village level.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 4: Classification of the subzones of Gash Barka by vegetation level

Vegetation	Subzones	ndvi
Arid	Akurdet, Dighe, Forto, Mensura	0
With some vegetation	Barentu, Gogne, Haykota, Mogolo, Tesseney	1
With much vegetation	Goluj, Laelay-Gash, Mulki, Shambko	2

Table A. Number of 2-week periods with NDVI > 0.361

	LAELAY-GASH	GOLUJ	MULKI	SHAMBKO	TESSENEY	GOGNE	BARENTU	HAYKOTA	MOGOLO	MENSURA	AKURDET	DIGHE	FORTO
2000	6	5	3	3	2	2	2	0	0	0	0	0	0
2001	7	6	5	4	5	4	2	3	1	2	0	0	0
2002	5	5	4	4	2	4	1	1	0	0	0	0	0
2003	6	5	5	5	4	4	4	3	2	1	0	0	0
2004	7	5	2	3	0	2	1	0	0	0	0	0	0
2005	7	6	4	4	3	4	0	0	1	0	0	0	0
2006	7	4	5	5	4	3	3	3	3	0	0	0	0
2007	7	7	7	7	5	6	6	5	5	3	0	2	0
2008	5	5	3	2	3	2	1	0	0	0	0	0	0
2009	4	5	4	3	1	3	3	0	0	0	0	0	0
10y avg	6.1	5.3	4.2	4.0	2.9	3.4	2.3	1.5	1.2	0.6	0.0	0.2	0.0
rank	1	2	3	4	6	5	7	8	9	10	12	11	12
5y avg	6.0	5.4	4.6	4.2	3.2	3.6	2.6	1.6	1.8	0.6	0.0	0.4	0.0
rank	1	2	3	4	6	5	7	9	8	10	12	11	12
3y avg	5.3	5.7	4.7	4.0	3.0	3.7	3.3	1.7	1.7	1.0	0.0	0.7	0.0
rank	2	1	3	4	7	5	6	8	8	10	12	11	12

high NDVI — medium NDVI — low NDVI

Table B. Number of 2-week periods with NDVI > 0.3

	LAELAY-GASH	GOLUJ	MULKI	SHAMBKO	TESSENEY	GOGNE	BARENTU	HAYKOTA	MOGOLO	MENSURA	AKURDET	DIGHE	FORTO
2000	9	8	7	6	5	4	4	2	0	0	0	0	0
2001	9	8	8	5	5	5	4	4	2	3	2	1	0
2002	8	6	5	5	4	4	4	4	3	1	0	0	0
2003	8	7	7	6	5	5	4	4	4	3	1	1	0
2004	7	8	6	6	2	5	4	2	1	0	0	0	0
2005	8	8	7	6	5	4	3	1	3	0	0	0	0
2006	8	8	8	7	4	4	4	4	4	3	0	0	0
2007	9	8	9	8	5	7	6	5	5	5	2	4	0
2008	8	8	7	5	3	4	4	2	1	0	0	0	0
2009	6	6	4	5	4	4	3	1	3	1	0	0	0
10y avg	8.0	7.5	6.8	5.9	4.2	4.6	4.0	2.9	2.6	1.6	0.5	0.6	0.0
rank	1	2	3	4	6	5	7	8	9	10	12	11	13
5y avg	7.8	7.6	7.0	6.2	4.2	4.6	4.0	2.6	3.2	1.8	0.4	0.8	0.0
rank	1	2	3	4	6	5	7	9	8	10	12	11	13
3y avg	7.7	7.3	6.7	6.0	4.0	5.0	4.3	2.7	3.0	2.0	0.7	1.3	0.0
rank	1	2	3	4	7	5	6	9	8	10	12	11	13

high NDVI — medium NDVI — low NDVI

Figure 1: Classification of subzones of Gash Barka by vegetation level.

In Tables A and B in this figure, each column refers to the subzone shown in the header. Tables A and B show the number of 2-week periods with NDVI above a threshold of 0.361 (in Table A) or 0.3 (in Table B). "10y avg" is the average number of 2-week periods with NDVI above the threshold in the subzone in the 10-year interval 2000-2009. "5y avg" is the average number of 2-week periods with NDVI above the threshold in the subzone in the most recent 5-year interval, i.e., 2005-2009. "3y avg" is the average number of 2-week periods with NDVI above the threshold in the subzone in the most recent 3-year interval, i.e., 2007-2009. Subzones are sorted from left to right according to their rank in 10-year average number of 2-week periods with NDVI above the threshold. The most arid subzones are on the right and those with more vegetation are on the left.

Table 5: Asset ownership, by wealth quintile

	(1)	(2)	(3)	(4)	(5)	Factor loadings
Water source						
piped into dwelling	0.000	0.000	0.000	0.003	0.006	0.040
piped into yard	0.003	0.006	0.003	0.003	0.003	-0.008
public tap	0.000	0.359	0.497	0.583	0.675	0.333
tube well	0.071	0.097	0.058	0.078	0.068	-0.026
protected well	0.136	0.094	0.049	0.026	0.026	-0.121
unprotected well	0.453	0.223	0.208	0.197	0.107	-0.187
protected spring	0.032	0.013	0.010	0.006	0.032	0.019
unprotected spring	0.243	0.133	0.143	0.081	0.062	-0.124
other	0.061	0.074	0.032	0.023	0.019	-0.049
Toilet type						
flush to PSS	0.000	0.000	0.000	0.000	0.006	0.044
flush to septic tank	0.000	0.000	0.000	0.000	0.006	0.045
to other byte	0.000	0.000	0.000	0.000	0.010	0.042
vip latrine	0.000	0.000	0.000	0.006	0.032	0.118
pit latrine slab	0.000	0.000	0.000	0.003	0.049	0.166
pit latrine open	0.000	0.000	0.000	0.013	0.153	0.339
composting	0.000	0.000	0.000	0.003	0.000	-0.001
bucket	0.000	0.000	0.000	0.000	0.003	0.046
hanging	0.000	0.000	0.000	0.000	0.010	0.037
bush	1.000	1.000	1.000	0.971	0.724	-0.406
other	0.000	0.000	0.000	0.003	0.006	0.034
Main cooking fuel						
electricity	0.000	0.000	0.000	0.003	0.003	0.012
kerosene	0.000	0.000	0.000	0.000	0.023	0.181
coal	0.000	0.000	0.000	0.000	0.019	0.143
charcoal	0.000	0.000	0.000	0.065	0.198	0.312
firewood	1.000	1.000	0.994	0.922	0.747	-0.399
dung	0.000	0.000	0.006	0.010	0.000	-0.002
other	0.000	0.000	0.000	0.000	0.010	0.205
Observations	309	309	308	309	308	

Table 6: Asset ownership, by wealth quintile (continued)

	(1)	(2)	(3)	(4)	(5)	Factor loadings
Main wall material						
None	0.010	0.071	0.026	0.016	0.019	0.005
Cane	0.498	0.366	0.224	0.094	0.117	-0.235
Bamboo	0.000	0.087	0.169	0.188	0.127	0.050
Stone wood	0.000	0.071	0.175	0.320	0.299	0.185
Uncovered adobe	0.000	0.000	0.000	0.006	0.006	0.058
Plywood	0.000	0.000	0.006	0.000	0.000	-0.009
Carton	0.006	0.016	0.010	0.010	0.000	-0.028
Cement	0.000	0.000	0.003	0.013	0.023	0.096
Stone cement	0.000	0.000	0.006	0.036	0.097	0.173
Bricks	0.000	0.000	0.036	0.087	0.068	0.083
Cement blocks	0.000	0.000	0.000	0.003	0.110	0.408
Covered adobe	0.000	0.000	0.016	0.013	0.006	0.017
Wood planks	0.424	0.236	0.120	0.074	0.029	-0.235
Other	0.061	0.152	0.208	0.139	0.097	-0.026
Main roof material						
Leaf	0.702	0.680	0.510	0.456	0.386	-0.193
Cane	0.000	0.000	0.003	0.003	0.000	-0.004
Bamboo	0.006	0.000	0.006	0.003	0.003	-0.012
Stone mud	0.100	0.104	0.162	0.139	0.136	0.004
Uncovered adobe	0.084	0.061	0.156	0.178	0.133	0.033
Cement	0.000	0.000	0.000	0.000	0.198	0.396
Stone cement	0.058	0.052	0.091	0.104	0.068	-0.009
Cement blocks	0.000	0.000	0.000	0.000	0.003	0.062
Coverer adobe	0.000	0.000	0.000	0.000	0.010	0.348
Other	0.049	0.104	0.071	0.117	0.062	-0.025
Window type						
any	0.000	0.078	0.341	0.570	0.513	0.269
shutters	0.000	0.000	0.029	0.227	0.305	0.360
glass	0.000	0.000	0.000	0.006	0.006	0.081
screens	0.000	0.000	0.000	0.000	0.003	0.073
none	0.570	0.518	0.334	0.084	0.097	-0.297
other	0.430	0.405	0.295	0.113	0.075	-0.237
Observations	309	309	308	309	308	

Table 7: Asset ownership, by wealth quintile (continued)

	(1)	(2)	(3)	(4)	(5)	Factor loadings
Electronics						
electricity	0.000	0.000	0.000	0.000	0.049	0.506
radio	0.000	0.155	0.244	0.317	0.539	0.362
TV	0.000	0.000	0.000	0.000	0.023	0.486
phone	0.000	0.000	0.000	0.000	0.023	0.393
fridge	0.000	0.000	0.000	0.000	0.010	0.481
Dwelling						
persons per room	3.935	3.972	3.973	4.055	3.794	-0.003
Vehicles						
bike	0.000	0.000	0.000	0.006	0.097	0.342
moto	0.000	0.000	0.000	0.000	0.006	0.198
car	0.000	0.000	0.000	0.000	0.010	0.165
cart	0.000	0.000	0.000	0.003	0.097	0.425
Other						
livestock	0.550	0.553	0.588	0.602	0.539	-0.011
Observations	309	309	308	309	308	

Table 8: Estimated heterogeneous treatment effect on malaria awareness

	Y=1(Malaria is a problem)				
Subsample:	All	All	Working age	Working age men	Working age women
Treatment	0.035	0.052	-0.026	-0.018	-0.037
	(0.035)	(0.072)	(0.043)	(0.081)	(0.047)
T x $ndvi$=1		-0.027			
		(0.085)			
		[0.5251]			
T x $ndvi$=2		-0.028			
		(0.113)			
		[0.7514]			
Work			-0.034	-0.003	-0.032
			(0.045)	(0.078)	(0.060)
T x work			0.126***	0.100	0.131**
			(0.049)	(0.095)	(0.062)
			[0.0178]	[0.1655]	[0.1385]
Female	-0.0709***	-0.0707***	-0.0564**		
	(0.024)	(0.024)	(0.027)		
Observations	1,567	1,567	1,479	549	918

Note: Treatment effects were estimated using probit regression (1) in model 1, probit regression (2) in model 2, and probit regression (3) in models 3–5. Sample restricted as shown in models 3–5. Marginal effects are reported for all models. Additional controls include: Tigre tribe dummy, Muslim dummy, subzone dummies. We omitted from model (2) the main effects for $ndvi$, to avoid collinearity with subzone dummies. Observations clustered at village level. Robust standard errors in parentheses. P-value for the F-test $interaction + treatment = 0$ in square brackets.
*** p<0.01, ** p<0.05, * p<0.1.

Table 9: Estimated heterogeneous treatment effects on knowledge that mosquitoes are the malaria vector

	Y=1(Mosquitoes are a malaria vector)				
Subsample:	All	All	Working age	Working age men	Working age women
Treatment	0.0302*	0.020	0.0468**	0.111*	0.036
	(0.016)	(0.025)	(0.023)	(0.058)	(0.025)
T x $ndvi$=1		-0.035			
		(0.043)			
		[0.6704]			
T x $ndvi$=2		0.0641***			
		(0.024)			
		[0.0005]			
Work			0.034	0.041	0.039
			(0.024)	(0.040)	(0.031)
T x work			-0.061	-0.154**	0.001
			(0.045)	(0.077)	(0.054)
			[0.7791]	[0.4006]	[0.4213]
Female	-0.025	-0.027	-0.023		
	(0.018)	(0.017)	(0.019)		
Observations	1,597	1,597	1,504	515	937

Note: Treatment effects were estimated using probit regression (1) in model 1, probit regression (2) in model 2, and probit regression (3) in models 3–5. Sample restricted as shown in models 3–5. Marginal effects are reported for all models. Additional controls include: Tigre tribe dummy, Muslim dummy, subzone dummies. We omitted from model (2) the main effects for $ndvi$, to avoid collinearity with subzone dummies. Observations clustered at village level. Robust standard errors in parentheses. P-value for the F-test $interaction + treatment = 0$ in square brackets.
*** p<0.01, ** p<0.05, * p<0.1.

Table 10: Estimated heterogeneous treatment effects on net ownership (Part A)

	Y=Number of bed nets owned				
Treatment	0.1674*	0.3112***	0.2012**	0.3708**	0.3986**
	(0.0890)	(0.1062)	(0.0884)	(0.1783)	(0.1779)
Literate respondent	-0.0455				
	(0.1563)				
Treatment x literate	0.3221**				
	(0.1548)				
	[0.0017]				
Unemployed respondent		0.0281			
		(0.0924)			
Treatment x unemployed		-0.1410			
		(0.1311)			
		[0.1117]			
Respondent ever attended school			-0.0504		
			(0.1639)		
Treatment x ever attended school			0.1524		
			(0.1745)		
			[0.0403]		
Muslim				-0.1361	
				(0.2590)	
Treatment x Muslim				-0.1807	
				(0.1944)	
				[0.0433]	
Treatment x Tigre tribe					-0.1976
					(0.2049)
					[0.1111]
Treatment x Hedarib tribe					-0.0921
					(0.2646)
					[0.1432]
Treatment x Nara tribe					-0.1268
					(0.2522)
					[0.1290]
Observations	1,441	1,441	1,441	1,441	1,441

Note: Respondent was used instead of household head if information was available only for respondents. Controls in all regressions include dummies for: tribes, Muslim, subzones, literacy, employment status, any schooling, gender of household head, household size tertiles, presence of any children under 5, radio ownership, wealth quintiles. Observations clustered at village level. Robust standard errors in parentheses. P-value for the F-test $interaction + treatment = 0$ in square brackets.
*** p<0.01, ** p<0.05, * p<0.1.

Table 11: Estimated heterogeneous treatment effects on net ownership (Part B)

	Y=Number of bed nets owned			
Treatment	0.2075**	0.1933**	0.2780***	-0.0773
	(0.0881)	(0.0869)	(0.0995)	(0.1350)
Male household head	0.2215***			
	(0.0820)			
Treatment x male head	0.0336			
	(0.1197)			
	[0.0268]			
Treatment x 2nd household size tertile		-0.0539		
		(0.1339)		
		[0.2504]		
Treatment x 3rd household size tertile		0.2124		
		(0.1658)		
		[0.0190]		
Household has any kids <5 years old			0.2713***	
			(0.0907)	
Treatment x any kids <5			-0.0857	
			(0.1163)	
			[0.0638]	
Treatment x 2nd wealth quintile				0.4557**
				(0.1853)
				[0.0040]
Treatment x 3rd wealth quintile				0.4027**
				(0.1905)
				[0.0198]
Treatment x 4th wealth quintile				0.2891
				(0.2349)
				[0.2937]
Treatment x 5th wealth quintile				0.4051*
				(0.2223)
				[0.0541]
Observations	1,441	1,441	1,441	1,441

Note: in this table, "nets" refers to any bed nets, irrespective of their treatment status. Controls in all regressions include dummies for: tribes, Muslim, subzones, literacy, employment status, any schooling, gender of household head, household size tertiles, presence of any children under 5, radio ownership, wealth quintiles. Observations clustered at village level. Robust standard errors in parentheses. P-value for the F-test $interaction + treatment = 0$ in square brackets.
*** p<0.01, ** p<0.05, * p<0.1.

Table 12: Information and knowledge about malaria

			$E(Y\|T=1,X)-E(Y\|T=0,X)$		
Variables	Treatment	Control	No Regressors	Basic Regressors	All Regressors
1. Mosquitoes mentioned among malaria vectors	0.908	0.854	0.0709***	0.0529**	0.0556**
	(0.289)	(0.353)	(0.0264)	(0.0222)	(0.0239)
2. Malaria is a problem in community	0.726	0.670	0.0689	0.0394	0.0502
	(0.446)	(0.471)	(0.0614)	(0.0432)	(0.0446)
3. Children mentioned among most affected by malaria	0.863	0.788	0.0811***	0.0803***	0.0645***
	(0.344)	(0.409)	(0.0332)	(0.0247)	(0.0227)
4. Pregnant women mentioned among most affected	0.367	0.365	-0.00180	-0.0254	-0.0175
	(0.482)	(0.482)	(0.0546)	(0.0309)	(0.0335)
5. In the previous 6 months, heard/saw messages about:					
5a. ITNs	0.484	0.469	0.0301	0.00213	0.0129
	(0.500)	(0.499)	(0.0554)	(0.0461)	(0.0415)
5b. Early seeking behavior	0.537	0.501	0.0531	0.0213	0.0239
	(0.499)	(0.500)	(0.0559)	(0.0521)	(0.0442)
5c. Environmental management	0.450	0.387	0.102*	0.0526	0.0516
	(0.498)	(0.487)	(0.0585)	(0.0487)	(0.0459)

Note: one observation per household (data available for respondents only). Columns 1 and 2 report means for treatment and control groups, with standard deviations in parentheses. Columns 3–5 report the difference between treatment and control groups, estimated using two-stage least-squares regression (4). The specification in column 3 does not include any controls. The specification in column 4 includes controls for: Tigre tribe, Muslim religion and subzone dummies. The specification in column 5 additionally includes all controls used in the randomization checks, i.e.: gender, age, household size, number of household members under age five, number of household members under age 18, number of rooms in dwelling, number of sleeping rooms, number of sleeping spaces; and dummy variables for whether respondent: usually lives in dwelling, was there the night before, ever attended school, is literate, is married; and dummy variables for whether main water source is: public tap, unprotected well, unprotected spring; and dummy variables for whether household owns a toilet, household owns a radio, firewood is main fuel for cooking, dwelling has no windows. In all regressions, observations are clustered at village level and robust standard errors are reported in parentheses. We also run a probit regression of treatment on sets of variables in the table plus the controls listed above and we test if these sets of coefficients are jointly We report p-values at the bottom of the table. *** p<0.01, ** p<0.05, * p<0.1.

Table 13: Ownership and use of mosquito bed nets

			$E(Y\|T=1,X)-E(Y\|T=0,X)$		
Variables	Treatment	Control	No Regressors	Basic Regressors	All Regressors
1. Number of nets owned by household	1.774	1.575	0.285**	0.253*	0.216**
	(1.279)	(1.207)	(0.136)	(0.131)	(0.109)
2. Number of ITNs owned by household	1.444	1.278	0.247**	0.209*	0.181
	(1.206)	(1.126)	(0.125)	(0.124)	(0.110)
3. Reported net use (of each household member)	0.429	0.380	0.0747	0.0354	0.0542
	(0.495)	(0.486)	(0.0465)	(0.0436)	(0.0393)
4. Number of observed nets used the night before	1.384	1.164	0.285**	0.246**	0.224**
	(1.214)	(1.054)	(0.130)	(0.116)	(0.106)
5. Number of observed nets left unused the night before	0.676	0.736	-0.0858	0.0101	-0.0116
	(0.993)	(1.001)	(0.0992)	(0.0815)	(0.0803)

Note: one observation per household for variables 1,2,4,5. One observation per individual for variable 3. In this table, "nets" refers to any bed nets, irrespective of their treatment status, whereas "ITNs" includes only LLINs and properly treated ITNs, following the definition presented in the paper. Columns 1 and 2 report means for treatment and control groups, with standard deviations in parentheses. Columns 3–5 report the difference between treatment and control groups, estimated using two-stage least-squares regression (4). The specification in column 3 does not include any controls. The specification in column 4 includes controls for: Tigre tribe, Muslim religion and subzone dummies. The specification in column 5 additionally includes all controls used in the randomization checks, i.e.: gender, age, household size, number of household members under age five, number of household members under age 18, number of rooms in dwelling, number of sleeping rooms, number of sleeping spaces; and dummy variables for whether respondent: usually lives in dwelling, was there the night before, ever attended school, is literate, is married; and dummy variables for whether main water source is: public tap, unprotected well, unprotected spring; and dummy variables for whether household owns a toilet, household owns a radio, firewood is main fuel for cooking, dwelling has no windows. In all regressions, observations are clustered at village level and robust standard errors are reported in parentheses. We also run a probit regression of treatment on sets of variables in the table plus the controls listed above and we test if these sets of coefficients are jointly significant. We report p-values at the bottom of the table. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 14: Participation in Larval Habitat Management (LHM)

			$E(Y\|T=1,X)-E(Y\|T=0,X)$		
Variables	Treatment	Control	No Regressors	Basic Regressors	All Regressors
Over the six month before the survey:					
1. Respondent participated in LHM	0.322	0.282	0.0562	0.0238	0.0302
	(0.468)	(0.450)	(0.0574)	(0.0477)	(0.0447)
During the month before the survey:					
2. Days spent by household in LHM	0.632	0.618	0.0297	0.0310	0.120
	(2.774)	(1.978)	(0.258)	(0.232)	(0.250)
3. Household members who participated in LHM	0.456	0.39	0.106	0.635	0.0599
	(1.007)	(0.898)	(0.107)	(0.0996)	(0.0912)
4. Male household members >15 years old	0.167	0.125	0.0667	0.0375	0.0398
who participated in LHM	(0.462)	(0.399)	(0.0433)	(0.0372)	(0.0356)
5. Female household members >15 years old	0.215	0.219	-0.00180	-0.00651	-0.0181
who participated in LHM	(0.47)	(0.483)	(0.0533)	(0.0480)	(0.0469)
6. Household members <15 years old	0.075	0.046	0.0415	0.0328	0.0382
who participated in LHM	(0.467)	(0.372)	(0.0359)	(0.0376)	(0.0344)

Note: one observation per household. Columns 1 and 2 report means for treatment and control groups, with standard deviations in parentheses. Columns 3–5 report the difference between treatment and control groups, estimated using two-stage least-squares regression (4). The specification in column 3 does not include any controls. The specification in column 4 includes controls for: Tigre tribe, Muslim religion and subzone dummies. The specification in column 5 additionally includes all controls used in the randomization checks, i.e.: gender, age, household size, number of household members under age five, number of household members under age 18, number of rooms in dwelling, number of sleeping rooms, number of sleeping spaces; and dummy variables for whether respondent: usually lives in dwelling, was there the night before, ever attended school, is literate, is married; and dummy variables for whether main water source is: public tap, unprotected well, unprotected spring; and dummy variables for whether household owns a toilet, household owns a radio, firewood is main fuel for cooking, dwelling has no windows. In all regressions, observations are clustered at village level and robust standard errors are reported in parentheses. We also run a probit regression of treatment on sets of variables in the table plus the controls listed above and we test if these sets of coefficients are jointly significant. We report p-values at the bottom of the table. *** p<0.01, ** p<0.05, * p<0.1.

Table 15: Behaviors conducive to malaria eradication, other than LHM

			$E(Y\|T=1,X)-E(Y\|T=0,X)$		
Variables	Treatment	Control	No Regressors	Basic Regressors	All Regressors
1. Household keeps livestock >100m from home	0.807	0.776	0.0360	0.0803**	0.0793**
	(0.395)	(0.417)	(0.0405)	(0.0387)	(0.0369)
2. Household covers stored water	0.942	0.953	-0.0113	-0.0431	-0.0370
	(0.234)	(0.212)	(0.0264)	(0.0289)	(0.0302)
3. Respondent does anything to prevent mosquito bites	0.834	0.804	0.0365	-0.00287	0.0139
	(0.372)	(0.397)	(0.0402)	(0.0332)	(0.0332)
4. Respondent mentions using net	0.680	0.649	0.0366	0.0119	0.0306
	(0.467)	(0.478)	(0.0507)	(0.0358)	(0.0367)
5. Respondent mentions burning coils	0.225	0.211	0.0183	0.00451	-0.00126
	(0.418)	(0.409)	(0.0458)	(0.0303)	(0.0309)
6. Respondent mentions using spray	0.025	0.021	0.00549	0.0116	0.0128
	(0.156)	(0.143)	(0.0119)	(0.0109)	(0.00993)
7. Respondent mentions burning animal dung	0.058	0.046	0.0165	0.00865	0.0121
	(0.234)	(0.209)	(0.0190)	(0.0163)	(0.0175)
8. Respondent mentions burning herbs	0.048	0.054	-0.00733	-0.0296	-0.0256
	(0.215)	(0.226)	(0.0236)	(0.0262)	(0.0258)
9. Respondent mentions draining stagnant water	0.106	0.120	-0.0201	-0.0320	-0.0274
	(0.309)	(0.325)	(0.0279)	(0.0246)	(0.0233)

Note: one observation per household. Columns 1 and 2 report means for treatment and control groups, with standard deviations in parentheses. Columns 3–5 report the difference between treatment and control groups, estimated using two-stage least-squares regression (4). The specification in column 3 does not include any controls. The specification in column 4 includes controls for: Tigre tribe, Muslim religion and subzone dummies. The specification in column 5 additionally includes all controls used in the randomization checks, i.e.: gender, age, household size, number of household members under age five, number of household members under age 18, number of rooms in dwelling, number of sleeping rooms, number of sleeping spaces; and dummy variables for whether respondent: usually lives in dwelling, was there the night before, ever attended school, is literate, is married; and dummy variables for whether main water source is: public tap, unprotected well, unprotected spring; and dummy variables for whether household owns a toilet, household owns a radio, firewood is main fuel for cooking, dwelling has no windows. In all regressions, observations are clustered at village level and robust standard errors are reported in parentheses. We also run a probit regression of treatment on sets of variables in the table plus the controls listed above and we test if these sets of coefficients are jointly significant. We report p-values at the bottom of the table. *** p<0.01, ** p<0.05, * p<0.1.

Table 16: Intra-household allocation of bed nets

	Y = 1(Net Use)				
			$E(Y\|T=1,X) - E(Y\|T=0,X)$		
Subsamples	Treatment	Control	No Regressors	Basic Regressors	All Regressors
Children under 5	0.5292	0.4970	0.0402	-0.0083	-0.0014
	(0.4995)	(0.5004)	(0.0615)	(0.0495)	(0.0488)
Youth aged 5–20	0.4107	0.3623	0.0742	0.0351	0.0614
	(0.4921)	(0.4808)	(0.0519)	(0.0516)	(0.0426)
Adult male workers	0.3520	0.2697	0.1266**	0.1142*	0.1392**
	(0.4781)	(0.4443)	(0.0576)	(0.0617)	(0.0584)
Adult female workers	0.5000	0.4026	0.1562**	0.0983	0.1501**
	(0.5013)	(0.4915)	(0.0737)	(0.0713)	(0.0702)
Adult male unemployed	0.3000	0.2409	0.0737	0.0726	0.0849
	(0.4594)	(0.4286)	(0.0760)	(0.0766)	(0.0705)
Adult female unemployed	0.4714	0.4408	0.0590	0.0134	0.0136
	(0.4996)	(0.4969)	(0.0591)	(0.0524)	(0.0544)

Note: The outcome variable Y is an indicator variable =1 if individual reportedly slept under a bed net the night before the survey, and =0 otherwise. For each subsample, columns 1 and 2 report average bed net use in treatment and control villages, with standard deviations in parentheses. Columns 3–5 report the difference between treatment and control groups, estimated using two-stage least-squares regression (4). The specification in column 3 does not include any controls. The specification in column 4 includes controls for: Tigre tribe, Muslim religion and subzone dummies. The specification in column 5 additionally includes all controls used in the randomization checks, i.e.: gender, age, household size, number of household members under age 5, number of household members under age 18, number of rooms in dwelling, number of sleeping rooms, number of sleeping spaces; and dummy variables for whether respondent: usually lives in dwelling, was there the night before, ever attended school, is literate, is married; and dummy variables for whether main water source is: public tap, unprotected well, protected unprotected spring; and dummy variables for whether household owns a toilet, household owns a radio, firewood is main fuel used for cooking, dwelling has no windows. In all regressions, observations are clustered at village level and robust standard errors are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

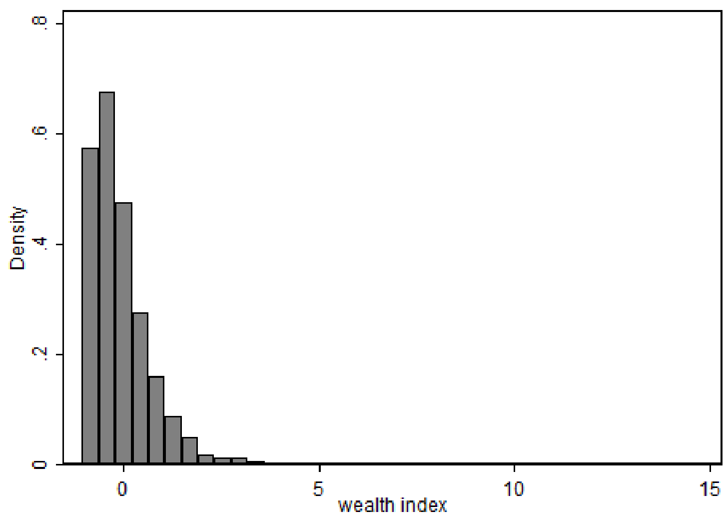

Figure 2: Distribution of wealth in Gash Barka

Appendix 4: Description of the Study Area

May 9, 2012

For Online Publication

This appendix describes the area studied in "Do Public Health Interventions Crowd Out Private Health Investments? Malaria Control Policies in Eritrea" by P. Carneiro, A. Locatelli, T. Ghebremeskel and J. Keating.

1 Zone Gash Barka, Eritrea

1.1 Area under investigation

The survey was conducted in Eritrea in Zone Gash Barka (GB), one of the six zones that compose the country. GB was chosen because it is the most malarious zone in Eritrea. The location of the zone is shows in Figure 1.

GB is a mostly rural/agricultural area, inhabited by one fifth of the country's population. Altitudes range between 500–1,500 meters and temperatures are generally associated with hot and dry climatic conditions. Significant variation can be observed across the region in terms of precipitations, leading to marked differences in vegetation and malaria prevalence. The rainy season is concentrated between July–September and precipitations are scarce during the rest of the year. GB is composed of 14 subzones, as shown in Figure 2. We surveyed only 13 of those subzones because one (Logo Anseba) was deemed to have a very low malaria prevalence (Logo Anseba is the black area in Figure 2).

1.2 Malaria in Gash Barka

Malaria transmission is seasonal and it extends from July until November/December. A peak is reached between September–November, following the rainy season. Our survey was conducted in the first half of October. This period corresponds to the malaria peak and it is highlighted in black in Figure 3. The average number of malaria cases[1] in GB, over the period 2002–2007, is shown in Figure 3.

Figure 4 shows that the number of clinical malaria cases declined sharply in Eritrea over the past decade, from 260 thousand in 1998 to 26 thousand in 2008. Most cases are concentrated in GB, and this zone witnessed a similar trend over the same time period: The number of clinical malaria cases registered in GB declined from 110 thousand in 1998 to about 18 thousand in 2008.

1.3 Vegetation in Gash Barka

The Normalized Difference Vegetation Index (NDVI) is an index of the vegetation level of a region, which we introduced in Section 3.1 of Appendix 3. Over the period July 1981–December 2009, the NDVI in GB ranged between 0.073–0.714. The index varies widely across subzones, which we classified in Table 4 of Appendix 3, depending on their average vegetation level in the decade before 2009.

The average value of NDVI in the 13 surveyed[2] subzones of GB is represented in Figure 5. This figure shows that vegetation starts increasing in July, following the inception of the rainy season. The NDVI peaks in September and declines sharply by the end of October. A slow decline in vegetation is observed between then and

[1]Figures include both IPD (in-patient department) and OPD (out-patient department) malaria cases.

[2]Logo Anseba not included, because this subzone was excluded from the survey.

June. The dashed vertical lines show the period when the survey was conducted, i.e., the second week of October.

Figure 6 shows that – in spite of a general sense that vegetation declined in GB in the recent past – the vegetation level recorded by satellites remained fairly stable. This suggests that policies of the NMCP may have been crucial to fight malaria, and that efforts to fight the disease must be sustained because the environment remains hospitable for the vector.

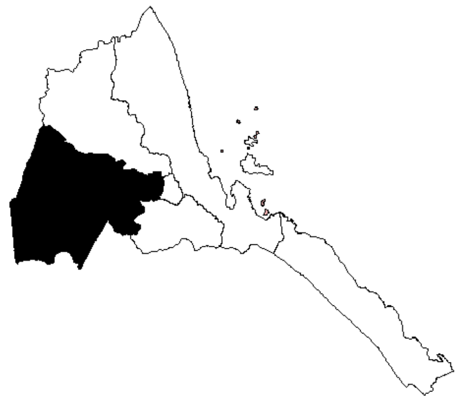

Figure 1: Location of Zone Gash Barka in Eritrea

Figure 2: Subzones of Zone Gash Barka

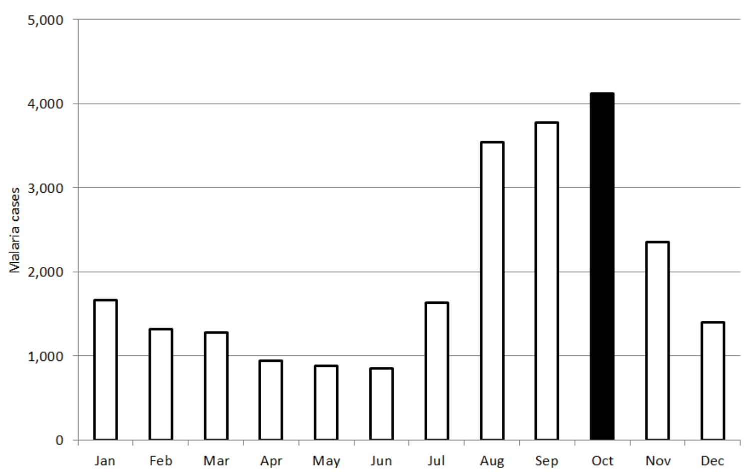

Figure 3: Monthly malaria cases in Gash Barka (2002–2007)

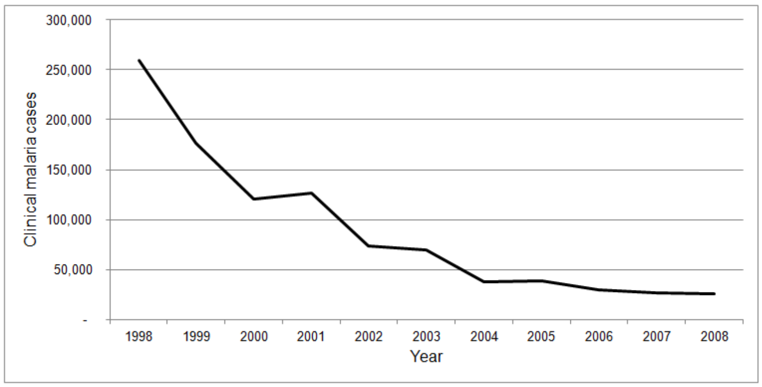

Figure 4: Clinical malaria cases in Eritrea (1998–2008)

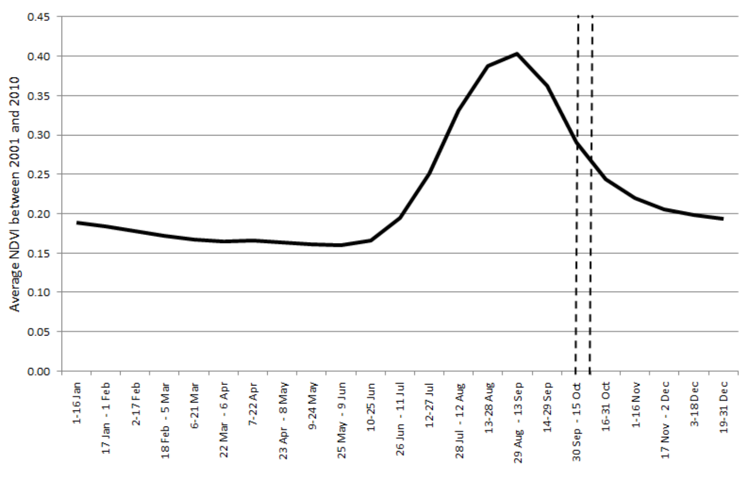

Figure 5: NDVI in Gash Barka (2001-2010)

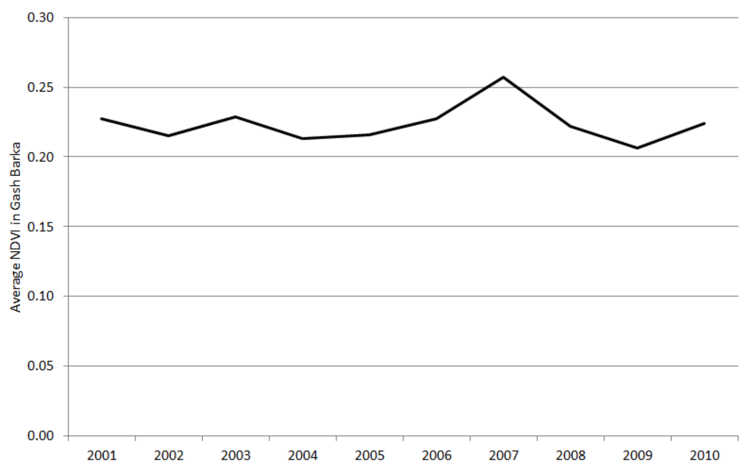

Figure 6: Yearly average NDVI in Gash Barka (2001-2010)

Appendix 5: Checks on Village Lists

May 9, 2012

For Online Publication

This appendix describes the problems encountered in the implementation of the randomization protocol and it presents robustness checks to complement the data analysis conducted in “Do Public Health Interventions Crowd Out Private Health Investments? Malaria Control Policies in Eritrea” by P. Carneiro, A. Locatelli, T. Ghebremeskel and J. Keating.

1 Introduction

Four village lists were used in the RCT under investigation. Comparison reveals some differences across the lists, and we attempt to identify precisely how these lists differ. About 70% of the villages have the same name in the first and last list, and another 10% can be matched using supplementary information. Two villages were arbitrarily replaced. The remaining 20% of village names do not match between the first and last list. Robustness checks suggest that the identified name changes did not alter our estimates of the treatment effects.

Treatment allocations were altered in 5 instances, and we explain possible reasons underlying these changes. Villages included in the RCT, despite not being in the initial list, do not differ significantly from villages initially listed. We find evidence suggesting that some Tigre villages received preferential treatment, which underlines the importance of controlling for this ethnic group in all our regressions (which we do in our analysis).

2 Four village lists

Four village lists were used for the RCT conducted in Eritrea, and we have a copy of each. Several differences exist between these tables. This section aims to keep track of what happened, to allow us to account for any problems in our analysis. The following are the village lists under investigation:

1. Initial village list, provided by the NMCP of Eritrea to J. Keating, to conduct the initial random allocation to treatment (2008);

2. Village list provided by the NMCP to the spraying teams that actually conducted the IRS campaign in Gash Barka (GB) in June–July 2009. This list includes only the names of treatment villages, because spraying teams need not visit the other villages. (Names of control villages were added by hand[1]; this was probably done by NMCP staff in GB);

3. Village list provided by the NMCP to data collectors (October 2009), including both treatment and control villages;

4. Final village list, provided by the NMCP to The World Bank, at the end of all field operations (November 2009).

3 Initially identified issues

Differences between village lists may have arisen from a variety of situation-specific problems. Those issues were discussed at length with the NMCP and analyzed with the help of local staff. The following are the main issues that we identified for each village list:

[1]Hand written names are often very hard to read.

1. The initial list was outdated, possibly from the census of 2002 or 2003: e.g., a subzone had changed name since then, from Omhajer to Goluj, and village sizes do not correspond to the current situation; e.g., Omhajer had only 70 household at the time, while some 1,200 households lived there in 2009. Some villages moved from a subzone to another, e.g., Hawashait moved from subzone Dighe to subzone Laelay Gash. Some even moved abroad, to Sudan or Ethiopia, making it impossible to reach them.

   Location data for the villages is not available from the Government of Eritrea, and existence and location of treatment and control villages were not checked or recorded prior to the beginning of the study. Notice however that, even if this had been properly done, it would still be possible to miss some migrant villages, so this problem could be expected in a setting like ours. Tracking or following those villages may at times be hard or even impossible, e.g., if they have moved abroad.

   Due to a sustained process of villagization, several villages may have merged into a new one. Villages may also have changed name. Villages recorded under similar names are deemed to be the same, because transliteration problems may occur when a different alphabet is used in the study area. Villages may even have several names, so that the same village could be recorded in two lists under very different names; we were able to reconcile some (but not all) of these cases.

   Two major issues, reported by NMCP, are worth pointing out here:

   (a) The minimum distance between villages had to be >5km.[2] After randomization some villages were found to be adjacent, so they were replaced to ensure the minimum distance would be kept. In fact, this issue should have been identified before the random treatment allocation. We do not know which instances were affected by this problem.

[2] A minimum distance was set to avoid spill-overs from treatment to control villages.

(b) Some treatment and control villages are located in the highlands, where there is no malaria[3]. Two such instances in subzone Mulki were reported, whereby one treatment and one control village were replaced with two new villages, located nearby, moving down to the lowlands. The new villages were chosen by NMCP staff in GB. In Section 3.1 we compare the new villages to the other two which were left unchanged.

2. When spraying teams tried to reach the *treatment* villages in List 2, sometimes they could not find one, or a village may have moved abroad and be out of reach. Migrant villages were followed whenever possible. Missing treatment villages were replaced with the closest available village.

3. Once the existence of treatment villages had been ascertained by spraying teams, the table was updated accordingly. The number of villages in List 1 was 116, but this was reduced to 115 in Lists 3 and 4. The reason for this change is unclear. A possible reason could be found in the process of villagization, if two listed villages merged into one. We cannot conclusively answer this question.

   New problems arose when enumerators went to the field to conduct the survey. Issues occurred when data collectors could not find some of the *control* villages, some of which had moved abroad and could not be reached. Missing control villages were replaced with the nearest available village. We compare List 3 to List 4 to try to see how many instances of this problem occurred. This problem concerns: 3 controls in subzone Goluj (villages 4, 5, 7); 1 control in subzone Tesseney (52), and 2 controls in subzone Shambko (93, 95).

   We analyze the determinants of such changes in Table 1. We do not find evidence of differential treatment for Tigre-populated villages. The negative coefficients estimated in models 4 and 6 suggest that replacement control villages were less wealthy than the other villages surveyed in the same subzone.

[3]There is no malaria >1,000 meters of altitude.

Notice that we are comparing replacement control villages to all (treatment and control) villages surveyed in the same subzone, and treatment villages may have become wealthier following the IRS intervention, e.g., if households more aware of malaria invested to improve their dwelling and protect their members from infection. However, we do not compare the new controls to the pre-existing controls because we would have too few observations to conduct this analysis.

4. The final list was drafted by NMCP after all field operations, and it accounts for all problems discussed above.

3.1 Arbitrary village choice in subzone Mulki

Two villages were replaced in subzone Mulki, because they were located in the highlands, where there is no malaria: as discussed in Section 3, one was chosen as a new treatment, and one as a new control. We check if preference was given to the Tigre tribe, which is over-represented in the treatment group. We have no data on the omitted villages. The new treatment village is number 43 and the new control is number 46. No Tigre households resides in either village; in our data there is only one Tigre household in this subzone. This suggests that no active effort was put to offer treatment to Tigre villages. Our estimates are very unlikely to be affected by two villages only.

4 Newly identified problems

4.1 Change in number of villages surveyed in each subzone

The number of villages surveyed by subzone was changed from List 1 to List 4, as shown in Table 2. This can be explained by the fact that, in recent years, the boundaries of certain subzones were changed, so that some villages were allocated to a new adjacent subzone.

The number of treatment villages was finalized when List 2 was drafted for the spraying teams. The total was reduced from 58 to 57. In 6 of the 13 surveyed subzones, the number of treatment villages was left unchanged. Column 5 of Table 2 shows that the largest disparities with respect to List 1 appear in subzone Haykota (where 3 extra villages were treated) and in subzone Mensura (where 3 less villages were treated). In the other subzones, the number of treated villages differs from the original figure by at most 1. The number of treatment villages, both in total and by subzone, was not changed in the subsequent lists.

The number of control villages was left unchanged at 58, from List 1 through List 4. However, column 10 of Table 2 shows that the allocation of control villages across subzones changed significantly: in the case of subzone Akurdet, it was increased by 3, while it was decreased by 3 in subzone Haykota.[4] The problem is less severe in the other subzones, in 5 of which the number of controls was left untouched.

4.2 Reallocation of treatment status

The treatment allocation of 5 villages was altered:

1. We compare List 2 to List 1 to see which control villages were reallocated to the treatment group. Here we report the ascertained cases. In subzone Haykota, this happened for 2 villages, i.e., Biet Hama (56) and Akyeb (59). In subzone Laelay Gash, this possibly[5] happened for one village, i.e., Amir/Uguma (19). We cannot identify any other instance in which this problem occurred.

2. We compare List 3 to List 1 to see which treatment villages were reallocated to the control group. Here we report the ascertained cases. In subzone Dighe, one village was re-allocated to serve as control, i.e., Aflanda (72). In subzone Forto, the same happened to one village, i.e., Grgr (16). In fact, no household

[4] Notice that in subzone Haykota the problem is severe for both treatment villages (+3) and control villages (-3).

[5] Names do not match perfectly.

was reportedly sprayed in Grgr, and only one was in Aflanda. Notice further that those villages are very small, the former with 11 households and the latter with 9 households (We know village size in these instances, because <15 households were surveyed there, which was allowed in villages composed of <15 households).

4.3 Unchanged villages

To conclude this section, we want to answer two questions: How many villages from List 1 are still present in List 4? How many of them have the same treatment status?

1. Out of 116 villages, 82 have the same name (or a similar one) in both List 1 and List 4. Another 10 villages have names that can be matched (if they have multiple names and this information is available) in the two lists. The former group includes 70% of the villages and the latter represents 9.5% of the total. Two villages were replaced in subzone Mulki. So we are left with 22 cases of mismatch that we can't explain, which represents 19% of the total.

2. We check to which of these categories the villages reallocated to treatment or to control belong. Villages 56 and 59 (reallocated to treatment) and 72 and 16 (reallocated to control) have matching names in Lists 1 and 4. Village 19 (reallocated to treatment) may be matched using the subzone where it is located. Therefore, to answer our question, 78 of the 82 villages with identical names have unaltered treatment status, and so do 87 of the 92 with matching names. This is roughly a 95% share.

5 Robustness checks

We want to understand whether changed villages differ from those that were not changed and, if so, along which dimensions. For this purpose, we conduct the same randomization checks used to compare treatment and control villages (see Tables 1 and 2 in the main body of the paper), but this time to compare villages with altered name or treatment allocation to those on the original list. In addition, we include a measure of household wealth (an asset index was introduced in Section 3.2 of Appendix 3).

We investigate the possibility that preference for treatment was given to villages with better infrastructure, where IRS operations could be conducted more easily. In some cases, it may be hardly possible to reach some villages with very little infrastructure, and operators confronted with this problem may have chosen the easiest available alternative.

Notice that we compare villages with altered name or treatment allocation, to all other villages in GB, rather than to those in the respective subzone. We do so because in Section 4.1 we documented evidence of changes in the number of villages per subzone, which hints to a possibility to choose replacement villages across the entire region.

5.1 Altered village names

In Tables 3 and 4 we investigate the presence of any systematic differences between villages whose name was not changed during the operations of the RCT, and those villages which instead were changed. Column 1 is analogous to the randomization checks presented in the paper, and we include it as a benchmark. In column 2 we check if villages with the same name in Lists 1 and 4 differ systematically from those which were not changed. We repeat the same analysis in column 3, where we broaden the definition of *unchanged villages* to include also those villages whose name we were able to re-conduct to the original list, with the help of additional information (e.g., exploiting information on multiple village names).

We find no evidence of systematic differences between changed and unchanged villages. Column 2 suggests that replaced villages are slightly less educated (variable 6), while the opposite appears from column 3 (variable 8). We find no evidence of any discrimination on grounds of ethnicity or wealth. We only find a significant age difference between unchanged and replaced villages, but we do not interpret this as a sign of age-based discrimination.

In Tables 5–8 we replicate the analysis of homogeneous treatment effects conducted in the main body of the paper, adding a dummy =1 if the name of the villages was left unchanged across village lists, and =0 otherwise. Estimates do not change appreciably, either in terms of magnitude or in terms of statistical significance.

5.2 Altered treatment allocations

Comparing the village lists used in the field, we noticed that two villages, originally randomized in the treatment, were used as controls, while three villages initially randomized out, were actually treated. In Tables 9 and 10 we investigate the presence of any systematic differences between these villages and those whose treatment allocation was left unchanged. In column 1 we compare villages whose treatment allocation was changed to all others. In columns 2 and 3 we restrict the sample to the treatment group and the control group respectively[6]. In this way, we can look in turn at the case of the new treatments and of the new controls.

We would be particularly worried if we found opposite signs in columns 2 and 3, which would suggest that some variables were used as grounds for preferential treatment allocation. We find evidence suggesting that Tigre villages were reallocated into treatment and away from the control group, which could possibly explain the imbalance in Tigre presence across treatment groups. We control for the Tigre tribe in all of our regressions. The differences estimated along other dimensions are quite similar in columns 2 and 3, suggesting that treatment allocation was not altered based on those characteristics.

[6]Altered villages in column 2 were moved from the control to the treatment group. Altered villages in column 3 were moved from the treatment to the control group.

Table 1: Choice of replacement control villages

	Tigre			Wealth		
Sample restricted to subzone:	Goluj	Tesseney	Shambko	Goluj	Tesseney	Shambko
village 4	-0.17			-2.45**		
	(0.15)			(0.78)		
village 5	-0.17			-2.23**		
	(0.15)			(0.78)		
village 7	-0.17			-1.71*		
	(0.15)			(0.78)		
village 52		0.38		-0.59		
		(0.20)			(0.41)	
village 93			-			0.25
						(0.13)
village 95			-			-0.68***
						(0.13)
Constant	0.24	0.62**	-	2.22**	0.38	0.09
	(0.15)	(0.20)	-	(0.78)	(0.41)	(0.13)
Observations	73	88	90	72	87	90

Note: one observation per household. This table presents the coefficients β_1 estimated from LS regression $Y_i = \beta_0 + \beta_1 X_i + \epsilon_i$, with standard errors in parentheses. In models (1)–(3), Y_i is an indicator variable =1 if household i belongs to the Tigre tribe, and =0 otherwise. In models (4)–(6) Y_i is an asset index for household i. Samples restricted to the subzones where listed villages are located, shown in each header. Notice that no Tigre households were surveyed in subzone Shambko. Observations clustered at village level. Robust standard errors in parentheses.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2: Number of villages in Lists 1, 2 and 4

	List 1			List 2		List 4						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Subzone	total	treatment	control	treatment	delta1	total	delta1	treated	delta1	delta2	control	delta1
Akurdet	6	3	3	4	1	10	4	4	1	0	6	3
Barentu	2	2	0	2	0	3	1	2	0	0	1	1
Dighe	12	6	6	5	-1	11	-1	5	-1	0	6	0
Forto	9	6	3	5	-1	9	0	5	-1	0	4	1
Gogne	11	5	6	5	0	10	-1	5	0	0	5	-1
Goluj (Omhajer)	7	2	5	2	0	5	-2	2	0	0	3	-2
Haykota	16	9	7	12	3	16	0	12	3	0	4	-3
Laelay-Gash	15	7	8	8	1	15	0	8	1	0	7	-1
Mensura	15	6	9	3	-3	12	-3	3	-3	0	9	0
Mogolo	7	4	3	3	-1	8	1	3	-1	0	5	2
Mulki	4	2	2	2	0	4	0	2	0	0	2	0
Shambko	6	2	4	2	0	6	0	2	0	0	4	0
Tesseney	6	4	2	4	0	6	0	4	0	0	2	0
Total	116	58	58	57	-1	115	-1	57	-1	0	58	0

Note: For List 1, this table reports in columns 1–3 the number of villages for each subzone, divided by treatment allocation. Column 4 reports the number of treatment villages that NMCP included in List 2, to be used by the spraying teams. Column 5 reports the difference between the previous column and the corresponding column for List 1: (5) = (4) - (2). Columns 6–12 refer to List 4. Column 6 shows the total number of villages for each subzone according to the final list. Column 7 reports the difference between the previous column and the corresponding column for List 1: (7) = (6) - (2). Column 8 reports the number of treated villages. The following columns 9-10 report the difference between that and the figure for Lists 1 and 2. Column 11 reports the number of control villages by subzone: (11) = (6) - (8). Column 12 reports the difference between the previous column and the corresponding column for List 1: (12) = (11) - (3).

Table 3: Which villages were replaced? – Individual Variables

	(1)	(2)	(3)
Variables (Y)	Treatment status	Same name	Matched name
ALL HOUSEHOLD MEMBERS			
1. Female	-0.0040	-0.0070	-0.0063
	(0.0113)	(0.0117)	(0.0140)
2. Usually lives here	0.0062	-0.0015	-0.0027
	(0.0049)	(0.0059)	(0.0070)
3. Stayed here last night	0.0137	-0.0096	-0.0046
	(0.0086)	(0.0093)	(0.0115)
4. Age	0.3456	1.4140***	1.3255**
	(0.4924)	(0.4870)	(0.5558)
RESPONDENTS ONLY			
5. Age	0.6157	1.8343*	1.5235
	(0.8926)	(0.9829)	(0.1459)
6. Ever attended school	0.0072	-0.0239	-0.0778*
	(0.0339)	(0.0372)	(0.0426)
7. Only primary school	-0.0373	0.0508	0.0565
	(0.0527)	(0.0544)	(0.0569)
8. Literate	-0.0151	-0.0286	-0.0905**
	(0.0321)	(0.0369)	(0.0422)
9. Muslim religion	0.0601	0.0639	0.1442
	(0.0678)	(0.0780)	(0.0961)
10. Tigre tribe	0.1666*	0.0387	0.1418
	(0.0843)	(0.0951)	(0.1061)
11. Married	-0.0125	-0.0143	-0.0057
	(0.0133)	(0.0135)	(0.0160)

Note. Variables 5–11: sample restricted to respondents only. This table reports, for each variable Y, the coefficient β_1 estimated from LS regression $Y_i = \beta_0 + \beta_1 X_i + \epsilon_i$, with standard errors in parentheses. Column (1) is analogous to the randomization checks, presented in Tables 1 and 2 in the main body of the paper. In column (1), X_i is an indicator variable =1 if village i is in treatment group, =0 otherwise. In column (2), X_i is an indicator variable =1 if village i has same name in village lists 1 to 4, =0 otherwise. In column (3), X_i is an indicator variable =1 if village i has same name in village lists 1 to 4 or if the name of village i was changed but can be matched, =0 otherwise. Observations are clustered at village level.
*** p<0.01, ** p<0.05, * p<0.1.

Table 4: Which villages were replaced? – Household Variables

	(1)	(2)	(3)
Variables (Y)	Treatment status	Same name	Matched name
HOUSEHOLD LEVEL VARIABLES			
12. Household size	0.1844	-0.1634	-0.1378
	(0.1559)	(0.1615)	(0.1734)
13. Household members under 5	0.0214	-0.0711	0.0049
	(0.0566)	(0.0592)	(0.0657)
14. Household members under 18	0.0925	-0.1835	-0.1770
	(0.1279)	(0.1284)	(0.1360)
15. Main source of drinking water:			
15.1.Public tap	-0.0104	-0.0524	-0.1460
	(0.0772)	(0.0887)	(0.1020)
15.2.Unprotected well	0.0195	0.0039	0.0428
	(0.0545)	(0.0571)	(0.0612)
15.3.Unprotected spring	-0.0150	0.0361	0.0646
	(0.0384)	(0.0392)	(0.0423)
16. Has any toilet	-0.0112	-0.0085	0.0096
	(0.0232)	(0.0274)	(0.0300)
17. Has radio	0.0084	-0.0076	-0.0068
	(0.0324)	(0.0348)	(0.0417)
18. Firewood is main fuel	-0.0214	-0.0181	-0.0318*
	(0.0185)	(0.0183)	(0.0178)
19. Has no window	0.0050	-0.0365	-0.0619
	(0.0656)	(0.0712)	(0.0766)
20. Number of separate rooms	0.0225	-0.1434	-0.1389
	(0.1049)	(0.1118)	(0.1215)
21. Number of sleeping rooms	0.0020	-0.0236	-0.0265
	(0.0509)	(0.0523)	(0.0532)
22. Number of sleeping spaces	-0.1641	-0.0582	-0.2794
	(0.1900)	(0.2048)	(0.2172)
23.Asset index	0.0736	-0.0553	-0.1479
	(0.1259)	(0.1417)	(0.1782)

Note. Variables 12–23: one observation per household. This table reports, for each variable Y, the coefficient β_1 estimated from LS regression $Y_i = \beta_0 + \beta_1 X_i + \epsilon_i$, with standard errors in parentheses. Column (1) is analogous to the randomization checks, presented in Tables 1 and 2 in the main body of the paper. In column (1), X_i is an indicator variable =1 if village i is in treatment group, =0 otherwise. In column (2), X_i is an indicator variable =1 if village i has same name in village lists 1 to 4, =0 otherwise. In column (3), X_i is an indicator variable =1 if village i has same name in village lists 1 to 4 or if the name of village i was changed but can be matched, =0 otherwise. Observations are clustered at village level.
*** p<0.01, ** p<0.05, * p<0.1.

Table 5: Robustness checks: Information and knowledge about malaria

			$E(Y\|T=1,X)-E(Y\|T=0,X)$		
Variables	Treatment	Control	No Regressors	Basic Regressors	Same Name
1. Mosquitoes mentioned among malaria vectors	0.908	0.854	0.0541**	0.0305*	0.027*
	(0.289)	(0.353)	(0.0213)	(0.016)	(0.016)
2. Malaria is a problem in community	0.726	0.670	0.0564	0.035	0.026
	(0.446)	(0.471)	(0.0442)	(0.035)	(0.035)
3. Children mentioned among most affected by malaria	0.863	0.788	0.0744***	0.0679***	0.069***
	(0.344)	(0.409)	(0.0248)	(0.019)	(0.019)
4. Pregnant women mentioned among most affected	0.367	0.365	0.002	-0.0143	-0.015
	(0.482)	(0.482)	(0.0403)	(0.024)	(0.024)
5. In the previous 6 months, heard/saw messages about:					
5a. ITNs	0.484	0.469	0.0152	-0.00050	0.005
	(0.500)	(0.499)	(0.0421)	(0.038)	(0.039)
5b. Early seeking behavior	0.537	0.501	0.0365	0.019	0.025
	(0.499)	(0.500)	(0.0420)	(0.040)	(0.040)
5c. Environmental management	0.450	0.387	0.0638	0.029	0.033
	(0.498)	(0.487)	(0.0430)	(0.036)	(0.036)

Note: one observation per household (data available for respondents only). Columns 1 and 2 report means for treatment and control groups, with standard deviations in parentheses. Columns 3–5 report the difference between treatment and control groups, estimated using LS regression (12) for continuous outcomes and probit regression (13) for binary outcomes. The specification in column 3 does not include any controls. The specification in column 4 includes controls for: Tigre tribe, Muslim religion and subzone dummies. In the specification in column 5, controls additionally include a dummy =1 if village name was not changed from List 1 to List 4, and =0 otherwise. In all regressions, observations are clustered at village level and robust standard errors are reported in parentheses.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 6: Robustness checks: Ownership and use of mosquito bed nets

			$E(Y\|T=1,X)-E(Y\|T=0,X)$		
Variables	Treatment	Control	No Regressors	Basic Regressors	Same Name
1. Number of nets owned by household	1.774	1.575	0.200*	0.214**	0.216**
	(1.279)	(1.207)	(0.110)	(0.0996)	(0.099)
2. Number of ITNs owned by household	1.444	1.278	0.166*	0.176*	0.180*
	(1.206)	(1.126)	(0.0963)	(0.0926)	(0.091)
3. Reported net use (of each household member)	0.429	0.380	0.049	0.034	0.028
	(0.495)	(0.486)	(0.035)	(0.033)	(0.030)
4. Number of observed nets used the night before	1.384	1.164	0.220**	0.186**	0.187**
	(1.214)	(1.054)	(0.0990)	(0.0877)	(0.086)
5. Number of observed nets left unused the night before	0.676	0.736	-0.0600	0.0152	0.025
	(0.993)	(1.001)	(0.0763)	(0.0626)	(0.061)

Note: one observation per household for variables 1,2,4,5. One observation per individual for variable 3. In this table, "nets" refers to any bed nets, irrespective of their treatment status, whereas "ITNs" includes only LLINs and properly treated ITNs, following the definition presented in footnote 15 of the paper. Columns 1 and 2 report means for treatment and control groups, with standard deviations in parentheses. Columns 3–5 report the difference between treatment and control groups, estimated using LS regression (12) for continuous outcomes and probit regression (13) for binary outcomes. The specification in column 3 does not include any controls. The specification in column 4 includes controls for: Tigre tribe, Muslim religion and subzone dummies. In the specification in column 5, controls additionally include a dummy =1 if village name was not changed from List 1 to List 4, and =0 otherwise. In all regressions, observations are clustered at village level and robust standard errors are reported in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Table 7: Robustness checks: Participation in Larval Habitat Management (LHM)

			$E(Y\|T=1,X) - E(Y\|T=0,X)$		
Variables	Treatment	Control	No Regressors	Basic Regressors	Same Name
Over the 6 months before the survey:					
1. Respondent participated in LHM	0.322	0.282	0.040	0.012	0.013
	(0.468)	(0.450)	(0.044)	(0.038)	(0.038)
In the last month before the survey:					
2. Days spent by household in LHM	0.632	0.618	0.013	0.025	0.033
	(2.774)	(1.978)	(0.181)	(0.161)	(0.165)
3. Household members who participated in LHM	0.456	0.39	0.066	0.051	0.035
	(1.007)	(0.898)	(0.077)	(0.071)	(0.068)
4. Male household members >15 years old	0.167	0.125	0.042	0.025	0.021
who participated in LHM	(0.462)	(0.399)	(0.031)	(0.027)	(0.027)
5. Female household members >15 years old	0.215	0.219	-0.004	-0.001	-0.004
who participated in LHM	(0.47)	(0.483)	(0.038)	(0.034)	(0.034)
6. Household members <15 years old	0.075	0.046	0.029	0.027	0.018
who participated in LHM	(0.467)	(0.372)	(0.025)	(0.026)	(0.023)

Note: Columns 1 and 2 report means for treatment and control groups, with standard deviations in parentheses. Columns 3–5 report the difference between treatment and control groups, estimated using LS regression (12) for continuous outcomes and probit regression (13) for binary outcomes. The specification in column 3 does not include any controls. The specification in column 4 includes controls for: Tigre tribe, Muslim religion and subzone dummies. In the specification in column 5, controls additionally include a dummy =1 if village name was not changed from List 1 to List 4, and =0 otherwise. In all regressions, observations are clustered at village level and robust standard errors are reported in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Table 8: Robustness checks: Behaviors conducive to malaria eradication, other than LHM

			$E(Y\|T=1,X)-E(Y\|T=0,X)$		
Variables	Treatment	Control	No Regressors	Basic Regressors	Same Name
1. Household keeps livestock >100m from home	0.807	0.776	0.031	0.068**	0.066**
	(0.395)	(0.417)	(0.032)	(0.031)	(0.031)
2. Household covers stored water	0.942	0.953	-0.011	-0.027	-0.020
	(0.234)	(0.212)	(0.020)	(0.018)	(0.016)
3. Respondent does anything to prevent mosquito bites	0.834	0.804	0.030	-0.006	-0.011
	(0.372)	(0.397)	(0.031)	(0.025)	(0.025)
4. Respondent mentions using net	0.680	0.649	0.029	0.011	0.005
	(0.467)	(0.478)	(0.039)	(0.029)	(0.028)
5. Respondent mentions burning coils	0.225	0.211	0.015	0.003	0.004
	(0.418)	(0.409)	(0.035)	(0.022)	(0.021)
6. Respondent mentions using spray	0.025	0.021	0.004	0.010	0.011
	(0.156)	(0.143)	(0.009)	(0.008)	(0.008)
7. Respondent mentions burning animal dung	0.058	0.046	0.012	0.005	0.005
	(0.234)	(0.209)	(0.014)	(0.012)	(0.012)
8. Respondent mentions burning herbs	0.048	0.054	-0.006	-0.017	-0.018
	(0.215)	(0.226)	(0.018)	(0.014)	(0.014)
9. Respondent mentions draining stagnant water	0.106	0.120	-0.014	-0.022	-0.022
	(0.309)	(0.325)	(0.021)	(0.018)	(0.017)

Note: Columns 1 and 2 report means for treatment and control groups, with standard deviations in parentheses. Columns 3–5 report the difference between treatment and control groups, estimated using LS regression (12) for continuous outcomes and probit regression (13) for binary outcomes. The specification in column 3 does not include any controls. The specification in column 4 includes controls for: Tigre tribe, Muslim religion and subzone dummies. In the specification in column 5, controls additionally include a dummy =1 if village name was not changed from List 1 to List 4, and =0 otherwise. In all regressions, observations are clustered at village level and robust standard errors are reported in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Table 9: Which villages were reallocated across treatments? – Individual Variables

	(1)	(2)	(3)
Subsample:	All villages	Treatment group	Control group
Variables (Y)			
ALL HOUSEHOLD MEMBERS			
1. Female	0.0157	0.0201	0.0096
	(0.0391)	(0.0579)	(0.0334)
2. Usually lives here	0.0149***	0.0076	0.0254***
	(0.0046)	(0.0057)	(0.0039)
3. Stayed here last night	0.0079	0.0173***	-0.0139
	(0.0110)	(0.0042)	(0.0106)
4. Age	4.1418***	3.3682***	5.3807***
	(0.4620)	(0.3959)	(0.3977)
RESPONDENTS ONLY			
5. Age	0.1662	2.5454	-3.4066
	(2.6551)	(1.8592)	(4.9482)
6. Ever attended school	-0.1374***	-0.1263***	-0.1556***
	(0.0293)	(0.0411)	(0.0352)
7. Only primary school	0.2397***	0.2603***	0.2192***
	(0.0263)	(0.0356)	(0.0400)
8. Literate	-0.1209***	-0.1390***	-0.0918
	(0.0434)	(0.0450)	(0.0799)
9. Muslim religion	0.1997***	0.1697***	0.2294***
	(0.0353)	(0.0472)	(0.0527)
10. Tigre tribe	0.0386	0.3009**	-0.3789***
	(0.1958)	(0.1298)	(0.0676)
11. Married	-0.0826***	-0.0525**	-0.1232***
	(0.0205)	(0.0210)	(0.0268)

Note: Variables 5–11: sample restricted to respondents only. For each variable Y, we report the coefficient β_1 estimated from LS regression $Y_i = \beta_0 + \beta_1 \Delta_i + \epsilon_i$, where Δ_i is a dummy =1 if person i lives in a village whose treatment status was changed, and =0 otherwise. Sample restricted to treatment group in column (2) and to control group in column (3). Robust standard errors in parentheses. Observations clustered at village level.
*** p<0.01, ** p<0.05, * p<0.1.

Table 10: Which villages were reallocated across treatments? – Household Variables

	(1)	(2)	(3)
Subsample:	All villages	Treatment group	Control group
Variables (Y)			
HOUSEHOLD LEVEL VARIABLES			
12. Household size	-0.8342***	-0.5932**	-1.2288***
	(0.2902)	(0.2295)	(0.4477)
13. Household members under 5	-0.1453	-0.0428	-0.2987**
	(0.0954)	(0.0852)	(0.1343)
14. Household members under 18	-0.8098***	-0.5737***	-1.1750***
	(0.2020)	(0.1506)	(0.2461)
15. Main source of drinking water:			
15.1.Public tap	0.1895	0.1207	0.2919**
	(0.1515)	(0.2349)	(0.1166)
15.2.Unprotected well	-0.2030***	-0.1837**	-0.2362***
	(0.0475)	(0.0699)	(0.0400)
15.3.Unprotected spring	-0.0324	0.0482	-0.1451***
	(0.0674)	(0.0927)	(0.0292)
16. Has any toilet	-0.0325	-0.0060	-0.0680***
	(0.0282)	(0.0409)	(0.0193)
17. Has radio	-0.1080*	-0.0090	-0.2529***
	(0.0607)	(0.0431)	(0.0240)
18. Firewood is main fuel	0.0107	-0.0104	0.0452***
	(0.0419)	(0.0667)	(0.0118)
19. Has no window	0.4261***	0.3127	0.5853***
	(0.1255)	(0.1889)	(0.0496)
20. Number of separate rooms	-0.5183***	-0.5669***	-0.4557***
	(0.0882)	(0.1047)	(0.1507)
21. Number of sleeping rooms	-0.2773***	-0.3001***	-0.2461***
	(0.0472)	(0.0626)	(0.0657)
22. Number of sleeping spaces	-1.1402***	-0.9049	-1.4443***
	(0.4100)	(0.6611)	(0.1808)
23.Asset index	-0.3498***	-0.3021**	-0.4310***
	(0.0994)	(0.1495)	(0.0763)

Note: Variables 12–23: one observation per household. For each variable Y, we report the coefficient β_1 estimated from LS regression $Y_i = \beta_0 + \beta_1 \Delta_i + \epsilon_i$, where Δ_i is a dummy =1 if person i lives in a village whose treatment status was changed, and =0 otherwise. Sample restricted to treatment group in column (2) and to control group in column (3). Robust standard errors in parentheses. Observations clustered at village level.
*** p<0.01, ** p<0.05, * p<0.1.