Do Public Health Interventions Crowd Out Private Health Investments? Malaria Control Policies in Eritrea[☆]

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Abstract

Engaging in indoor residual spraying in areas with high coverage of mosquito bed nets may discourage net ownership and use. This paper analyses new data from a randomized control trial conducted in Eritrea, which surprisingly shows the opposite: indoor residual spraying encouraged net acquisition and use. One possible explanation for this finding is that there is imperfect information about the risk of malaria infection. The introduction of indoor residual spraying 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.

Keywords: Malaria, Bed nets, Indoor Residual Spray, Information, Beliefs, Behavior.
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1. Introduction

Most public programs induce behavioural responses in their target population. These responses are often perverse, making programs less effective than what was originally intended. This is a central concern in the design of public interventions across a variety of contexts, in rich and poor countries alike. In the particular case of malaria control programs, the introduction of indoor residual spraying[1] (IRS) could have a negative impact on the use of insecticide treated mosquito bed nets (ITN), if the investment in one technology crowds out the investment on the other.

This paper analyses new data from a randomized control trial conducted in Eritrea, which surprisingly shows the opposite: an IRS campaign implemented in the most malarious region of the country led to increases in ITN ownership and use. Under perfect information about the returns to investment in the two technologies, the extent to which private investments crowd out public investments depends on the degree of substitutability between the two (e.g. Lengeler, 2011). If instead individuals perceive IRS and ITNs as complements, we would expect a positive response in private investment when the public investment is increased, as we observe in the data. However, available data does not allow to identify whether individuals in the sample perceive the technologies as substitutes or complements. In addition, there is no evidence in literature related to the perception of these technologies.[2]

Outside the scope of a perfect information model exist situations where the introduction of a program changes the information set of individuals. For example, by introducing a health program in a community, the public health authorities may be perceived to be especially concerned about that particular health problem. This may indicate to individuals that the issue may be more serious than what they had initially perceived it to be and induce a change in their beliefs about the returns to private health investments. A program could also have an implicit information component even when it does not include an explicit information campaign. In this context, the standard crowding-out intuition breaks down and an increase in public health investments can lead to an increase in private health investments even when the technologies are perceived as substitutes.[3] Our analysis suggests that, in parallel to an increase in private health investments, the introduction of IRS caused a change in beliefs about the importance of the disease in these areas.

An additional channel through which IRS could influence ITN ownership is related to changes

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

[2] Kleinschmidt et al. (2009) provide evidence that combined use of IRS and ITNs reduces the probability of malaria infection more than their individual use. However, this is not per se evidence of complementarity, which implies that the combined use of the two technologies generates larger impacts than the sum of the impacts of using them individually.

[3] Some public reaction in the US to the recent Ebola outbreak has some similarities with the situation we just described. There is limited public information about Ebola, which means that public perceptions of the disease may be easier to change than in cases where there is a higher level of knowledge. The perception of massive government investments towards the prevention of Ebola in the US (both in the countries where the outbreak originated from and in the US), may have lead some individuals to become very worried about the possibility of an Ebola outbreak in the US. This change in perceptions lead individuals to act accordingly, either through their own health behaviours or by putting pressure on the politicians who represent them.

in net prices. This could occur if, for example, the intervention not only provides IRS, but also increases the supply of nets. A reduction in net prices and a subsequent increase in ownership could follow. In our setting, no nets were distributed together with the IRS campaign and, therefore, the supply of nets is unlikely to have changed as a result of the intervention.

The data used in the study come from an experimental evaluation of the impact of an IRS program organized by the Government of Eritrea in the most malarious region of Eritrea (Gash Barka). Fifty-eight (58) villages were randomly assigned to treatment and 58 villages were randomly assigned to control. Between June and 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[4]. Households in control villages did not receive publicly provided IRS and, at the same time, IRS was not privately provided in this region. A household survey and malaria rapid diagnostic tests (RDT) were administered during the malaria season that followed (October 2009).

Although the prevalence of malaria parasite infections was found to be low in this area, villagers were still actively engaging in different malaria prevention activities. Gash Barka is characterized by environmental features that are favourable, particularly during the rainy season, to mosquito proliferation and that have been relatively constant over the last ten years.[5] In this setting, Keating et al. (2011) focus explicitly on the effect of the IRS campaign on malaria prevalence and on the extensive margin of ITN ownership (i.e. whether households own at least one ITN), documenting no difference between treatment and control group for both indicators. Our aim is instead to quantify the impact of the intervention on individual and household malaria prevention behaviours. Our data shows that the intervention led to higher ownership and use of ITNs on the intensive margin. This means that the extensive margin of ownership does not explain all the increase in the number of nets owned/used that is observed in the treatment group, relative to the control group. In addition, households in treatment villages became more aware of (and concerned with) malaria than those in control villages. Relative to households in control villages, they were more likely to mention mosquitoes as a malaria vector, and to mention children as one of the groups most affected by malaria.

When conducting the analysis, we faced two main challenges. First, even though our data comes from a randomized control trial, we were not able to collect a baseline survey. This means that we were unable to collect pre-program outcomes, and check whether the sample showed balance in these variables. However, we do not expect there to be any imbalance induced by the

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[4] Teams visiting villages for IRS treatment were comprised of social workers. It is unlikely that IRS teams provided information about malaria to the households living in treatment villages, in addition to offering IRS treatment. Within the National Malaria Control Program, information campaigns are managed by a communication team, which did not participate in the IRS campaign.

[5] The area experienced high levels of malaria infections in the past and a steep reduction over the past decade, mainly explained by an increase in prevention activities. For this intervention, less than 1 percent in the sample tested positive to malaria on October 2009 (Keating et al., 2011). A detailed discussion of malaria prevalence in the study area is presented in appendix B.1.

randomization procedure. We show that the data is balanced across essentially all variables that can be safely assumed to be pre-determined and on indicators of pre-intervention infection risk.[6]

Second, we analyse program impacts on a relatively large number of outcomes. Therefore, it is essential to account for the simultaneous testing of multiple hypotheses. For all the outcomes and for each specification, we implement the stepwise multiple testing procedure suggested by Romano and Wolf (2005), which adjusts the critical values used for each hypothesis being tested and therefore controls for the family-wise error rate (FWER). We show that our conclusions are robust to multiple hypothesis testing.

A large literature debates the extent to which a variety of public programs discourages (or crowds-out) private investments in those goods or services that are provided by the public sector. Two examples (among many) are Peltzman (1973), who discusses the case of higher education in the US, and Cutler and Gruber (1996), who study health insurance in the US. Examples of the importance of crowding-out effects for health programs in developing countries are much less common in the literature than for developed countries, perhaps because of lack of data. Some examples include Das et al. (2011), who analyse education subsidies in Zambia and India, and Bennett (2012), who studies the negative effect of the provision of piped water on household sanitary behaviour in the Philippines.

The standard presumption in these papers is that there is substitutability between private and public expenditures, and that individuals have perfect information about the returns to their health investments. However, there is increasing evidence that decision-making by the poor is greatly affected by limited information (e.g. Bertrand et al. 2006; Banerjee and Duflo 2011; Dupas 2011b). 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. This 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. Providing information about the returns from using a technology can be an effective way to promote both take-up and use. Dupas (2011b) reviews several studies that show how the provision of information can effectively influence people's health-seeking behaviour, 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 the new information. In other words, policies may affect people's behaviour if they are able to change their beliefs. In a study of HIV in Malawi, De Paula et al. (2011) do not find strong evidence that HIV testing consistently affects people's beliefs about their own HIV status (see also Delavande and Kohler, 2009), but they also show that downward revisions in beliefs about HIV status increase risky behaviour,

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[6] We complement our dataset with pre-intervention geographic and time variation of the area of intervention's Normalized Difference Vegetation Index (NDVI), a vegetation index obtained from the analysis of the colour spectrum of satellite imagery. NDVI generally measures the overall propensity of an area to harbour mosquito populations (Gaudart et al., 2009; Shililu et al., 2004).

while the opposite occurs with upward revisions. In another study about HIV-related behaviour, Dupas (2011a) shows that providing information on the relative risk of HIV infection disaggregated by gender and age has a significant effect on teenage pregnancy. The role of information in public health programs and health behaviour in developing countries is also key in Madajewicz et al. (2007); Goldstein et al. (2008); Kremer et al. (2009).

It is important to recognize how the availability of information about the benefits of using one technology plays a central role in public health policies. Borrowing from the literature in marketing and psychology, Dupas (2009) analyses 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 (2009) 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.

We also contribute to the understanding of ITN use, which is the main tool available to households to prevent malaria 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. This aspect is further investigated by Tarozzi et al. (2013), who conducted an RCT in Orissa (India) and provided evidence on the effectiveness of micro-loans promoting ITN ownership.

The remainder of the paper is organized as follows. In Section 2 we briefly describe the study area and the malaria eradication activities taking place in that area. In Section 3 we describe our dataset and we present our estimates in Section 4. Section 5 concludes.

2. IRS in Eritrea and the Intervention

Malaria is transmitted to humans from the bite of infected female 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 destroying the habitat of mosquitoes by draining stagnant water; IRS consists of spraying the inside walls of dwellings with insecticide to kill resting mosquitoes.

Eritrea has been successful in greatly reducing malaria prevalence to relatively low levels. 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 (National Malaria Control Program, NMCP). In Eritrea, the costs of IRS are borne almost exclusively by the government, which conducts spraying campaigns (there is no private market for IRS activities). Similarly, LHM campaigns are organized by the government with the active involvement of local populations. In contrast, ITNs must be acquired by individuals, set up above the bed and used regularly to have an effect. There exist periodic massive distribution campaigns for ITNs, but use and care of ITNs is still a private decision. Sleeping under a net is perceived as unpleasant, especially in warm weather, and ITNs also need regular re-impregnation, if they are not coated with long-lasting insecticide.[7]

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 decided to conduct 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 first results of this evaluation are presented in Keating et al. (2011).

The intervention was conducted in the Gash Barka region, one of the six zones that compose the country and the most malarious zone in Eritrea.[8] Between 2007 and 2008, this zone registered more than half of all diagnosed malaria cases and over 60 percent of all related deaths in the country. Gash Barka is mostly a rural/agricultural area, representing one-fifth of the country's population, which is estimated at 3.6 million. Altitudes range between 500 and 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 precipitation, leading to marked differences in vegetation and malaria prevalence. The rainy season is concentrated between July and September, while precipitation is scarce during the rest of the year. As a result, malaria transmission is higher in the period from July to December, with a peak in September and October, following the rainy season.

A two-arm cluster-randomized controlled trial (using a 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 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 ensure that treatment and control villages were at least 5 km apart. The NMCP verified the distance between treatment and control villages, and villages that were within 5 km from 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 could not be found or reached. Again, the closest eligible village was chosen as a replacement.[9]

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[7] There is limited evidence on the barriers to mosquito net use in malaria-endemic regions (Pulford et al., 2011). Discomfort, mainly related to heat, is among the main identified reasons for not using the nets. 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%).

[8] We excluded the sub-zone Logo Anseba since it was deemed to have a very low malaria prevalence attributable to higher altitude.

[9] This procedure is documented in detail in appendix D. The list we originally used to randomly assign villages to treatment or control group 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. Our analysis provides evidence that randomization was effective.

In each treatment village, the intervention involved the control of adult mosquito populations using IRS with the insecticide dichlorodiphenyltrichloroethane (DDT), which is recommended by the Eritrean NMCP. During the months of June to July of 2009, 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 (WHO). Treatment and control villages received similar levels of ITNs, 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).

3. Data

A household survey was conducted in October 2009, which corresponds to the period right after the peak of the malaria season.[10] 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 and microscopy was used to validate positive RDT results. No other additional test, such as anaemia, was collected. 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 (unexpectedly) very low in the area under investigation.

Tables 1 and 2 present means and standard deviations for variables which are essentially predetermined, and mean differences between the treatment and the control groups. Even though some of these variables could potentially respond to the intervention, it is unlikely that any response along these dimensions (household demographics, dwelling and village characteristics) took place between the time of the intervention (June-July 2009) and the time of the survey (October 2009). Table 1 shows individual-level variables and table 2 shows household-level variables. All 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 that 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.

Tables 1 and 2 also show joint tests that check the balance of several variables 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

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[10] A baseline survey was not collected because of budgetary constraints. Appendix C provides a detailed description of the data and of all the variables used in this paper.

Table 1: Randomization checks: Individual Variables

	(1) Control	(2) Treatment	(3) Difference
All household members			
1 - Female	0.521	0.517	-0.004
	(0.500)	(0.500)	(0.011)
2 - Age	21.997	22.343	0.346
	(19.184)	(19.517)	(0.492)
3 - Stayed here last night	0.953	0.967	0.014
	(0.212)	(0.180)	(0.009)
Respondents only			
4 - Female	0.663	0.610	-0.052
	(0.473)	(0.488)	(0.037)
5 - Age	41.431	42.047	0.616
	(15.255)	(15.006)	(0.893)
6 - Ever attended school	0.186	0.193	0.007
	(0.389)	(0.395)	(0.034)
6a - Only primary school	0.782	0.745	-0.037
	(0.414)	(0.437)	(0.053)
7 - Literate	0.196	0.181	-0.015
	(0.397)	(0.385)	(0.032)
8 - Married	0.940	0.928	-0.013
	(0.237)	(0.259)	(0.013)
9 - Muslim	0.779	0.839	0.060
	(0.415)	(0.368)	(0.068)
10 - Tigre tribe	0.401	0.567	0.166*
	(0.490)	(0.496)	(0.084)
11 - Other Afro-Asiatic tribe	0.332	0.227	-0.104
	(0.471)	(0.419)	(0.076)
Observations (All household members)	3774	3899	7673
Observations (Respondents only)	797	799	1596
Joint test on variables: 1-3		p-value	0.242
Joint test on variables: 4-11		p-value	0.233

Note: *** p<0.01, ** p<0.05, * p<0.1. Columns (1) and (2) report sample means in the control and treatment groups, with standard deviations in parentheses. Column (3) reports the difference between (2) and (1) estimated using an OLS regression of the correspondent outcome on the treatment indicator. Standard errors clustered at village level are reported in parentheses. We also present joint tests of balance across variables, by running a probit regression of the treatment indicator on the groups of variables, and reporting p-values of an F-test for the joint significance of the coefficients on the selected variables. Variable 6a is not used in the joint test since it is conditional on having attended school. "Other Afro-Asiatic tribe" includes Tigrinya and Hedareb tribes, while the excluded category "Other tribes" includes Afar, Bilen, Nara, Rashaida, Saho and Kunama tribes.

Table 2: Randomization checks: Household Variables

	(1) Control	(2) Treatment	(3) Difference
12- Household adult members	2.397	2.478	0.082
	(1.036)	(1.092)	(0.063)
13- Household members under 5	0.824	0.845	0.021
	(0.941)	(0.904)	(0.057)
14- Household members 6-18 y.o.	1.575	1.654	0.078
	(1.530)	(1.559)	(0.098)
15- Access to public tap	0.432	0.422	-0.010
	(0.496)	(0.494)	(0.077)
17- Access to unprotected spring	0.140	0.125	-0.015
	(0.347)	(0.331)	(0.038)
16- Access to unprotected well	0.228	0.248	0.020
	(0.420)	(0.432)	(0.054)
18- Has any toilet	0.066	0.054	-0.011
	(0.248)	(0.227)	(0.023)
19- Has radio	0.244	0.252	0.008
	(0.430)	(0.435)	(0.032)
20- Firewood is main fuel	0.956	0.935	-0.021
	(0.204)	(0.247)	(0.018)
21- Has no window	0.319	0.324	0.005
	(0.466)	(0.468)	(0.066)
22- Number of separate rooms	1.833	1.855	0.022
	(1.199)	(1.183)	(0.105)
23- Number of sleeping rooms	1.380	1.382	0.002
	(0.819)	(0.714)	(0.051)
24- Number of sleeping spaces	4.608	4.444	-0.164
	(2.453)	(2.347)	(0.190)
25- High Vegetation (NDVI)	0.400	0.435	0.035
	(0.490)	(0.496)	(0.093)
26- Share of female in the village	0.523	0.519	-0.005
	(0.059)	(0.061)	(0.011)
Observations	775	768	1543
Joint test on variables: 12-26		p-value	0.837
Joint test on variables: 4-26		p-value	0.422

Note: *** p<0.01, ** p<0.05, * p<0.1. Columns (1) and (2) report sample means in the control and treatment groups, with standard deviations in parentheses. Column (3) reports the difference between (2) and (1) estimated using an OLS regression of the correspondent outcome on the treatment indicator. Standard errors clustered at village level are reported in parentheses. We also present joint tests of balance across variables, by running a probit regression of the treatment indicator on the groups of variables, and reporting p-values of an F-test for the joint significance of the coefficients on the selected variables. “High vegetation (NDVI)” is an indicator variable equal to 1 if the village is in an area where, during the period 2000-2009, NDVI exceeded 0.361 for more than 4 weeks per year on average (see appendix B.2 for detailed information).

coefficients in the regressions are jointly equal to zero. Let T_i denote an indicator that takes value 1 if household i belongs to a treatment village and 0 otherwise and let X_i be a vector of variables in each group. Then we estimate:

$$\Pr(T_i = 1 | X_i) = \Phi\left(X_i' \beta\right) \tag{1}$$

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 village level. We do not reject the null hypothesis of no difference between treatment and control 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.

To control for pre-intervention differences in risk of infection (or exposure to malaria) we compare treatment and control villages using a NDVI index.[11] This index has been shown to be correlated with the species of malaria called *Plasmodium falciparum*, which accounts for more than 80% of malaria infections in Eritrea (Shililu et al., 2004), and generally measures the overall propensity of an area to harbour mosquito populations. We observe no significant difference between treatment and control villages on this dimension, supporting randomization balance.

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 an average age of 22, and an average age among respondents of 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 (20 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 living in these villages are very poor: only 43 percent of them have access to drinking water from a public tap, 6 percent have a toilet, 25 percent own a radio, 95 percent use firewood as the main source of fuel, and the average number of rooms per house is well below 2.

Compliance with treatment was high, but not perfect. Table 3 shows that 6 percent of households living in control villages reported having their dwelling sprayed in the 5 months prior to the survey, which is roughly the period of time between the treatment and the interviews. The spraying in control villages was not carried out by the government. Most likely, households used simple insecticide sprays purchased from local shops, which have low effectiveness when compared to IRS, since the cost of replicating the IRS provided by the government would be too high for any

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[11] We always include in the controls a "High vegetation" indicator variable equal to 1 if the village is in an area where, during the period 2000-2009, NDVI exceeded 0.361 for more than 4 weeks per year on average. This threshold is based on the findings of Gaudart et al. (2009). See appendix B.2 for detailed information.

of these poor households.[12] Also, 25 percent of households in treatment villages reported not having received IRS or not recalling it. This may have occurred because all household members were absent at the time of the intervention. Since participation was voluntary, it could also have happened because the residents did not authorize spraying inside their home. 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 centre of the village so they were not reached by the IRS campaign. Nevertheless, spraying activity targeted all households in the village, to guarantee that at least 80% of the village was covered, in line with the World Health Organization guidelines.

Table 3: Program compliance (self-report)

	Control group	Treatment group	Total
Dwelling was sprayed in past 5 months	49 (7.5%)	604 (92.5%)	653
Dwelling was not sprayed in past 5 months	679 (84.6%)	124 (15.4%)	803
Missing information	80 (49.7%)	81 (50.3%)	161
Total	808	809	1617

Note: this table shows the number of respondents reporting whether someone sprayed the interior walls of their dwelling against mosquitoes (without specifying whether it was carried out by IRS teams) in the 5 months prior to the survey, distinguishing between control and treatment groups. In parenthesis we report the corresponding population shares for each answer. Five months corresponds approximately to the period of time between the IRS intervention and the survey. When the respondent doesn't know whether the dwelling was sprayed, we report it as missing information.

Throughout the paper, we report not only simple comparisons between treatment and control villages, but also instrumental variable estimates to correct for imperfect compliance with the IRS campaign. The reason why we focus on both sets of estimates is that the intervention is likely to affect the beliefs and behaviours of all residents in the community, even those who did not have their house sprayed, and therefore the intent to treat estimate is as interesting as the instrumental variables estimate. We further develop this issue below.

4. Data Analysis

4.1. Main Results

In this section we analyse the impact of the IRS campaign on a set of behavioural and socio-economic outcomes. In particular, we start by looking at the effect of spraying on the ownership of mosquito bed nets, by making use of both self-reported and observed information.[13] We then

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[12] NMCP records report that no IRS campaign was conducted in control villages over the 12 months prior to the survey. We can also exclude that other organizations conducted an IRS campaign in the region. Since the question did not specify "with DDT" or "by spraying teams", respondents may have plausibly answered yes if they had engaged in personal spraying with commercially bought insect repellent. The effect of such sprays is very limited compared to that of IRS.

[13] The interviewer first asked the respondent "How many mosquito nets does your household have?" and then asked the respondent to show each net in the dwelling. For each observed net, a series of questions are asked and some observational data is collected (e.g. whether the net is an ITN).

discuss possible mechanisms for this effect by looking at the impact on the level of information and awareness of malaria among the people of Gash Barka and other preventive behaviours. The impact of IRS on malaria prevalence was found to be zero in our earlier work (Keating et al., 2011).

In tables 4-6 we compare treatment and control villages across a variety of dimensions (ownership and use of mosquito bed nets, concern and knowledge of malaria, and participation in LHM). The first two columns of each table present means and standard deviations for each variable, for control and treatment 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 specification includes a set of control variables which includes all the variables we analysed in the randomization checks[14] (which we call X_i in the equations below) and village level characteristics V_j. Village level controls include a set of regional dummies, an indicator whether the village is in an area with high vegetation during the 10 years prior to the intervention and the share of women living in the village.

We estimate the program impact using least squares regression (2) of the outcome for individual/household i living in village j (we indicate it by Y_{ij}) on a treatment indicator T_j and control variables X_i:

$$Y_{ij} = \alpha + \beta T_j + X_i'\gamma + V_j'\delta + \epsilon_{ij} \tag{2}$$

where ϵ_{ij} is an individual-specific error term. Standard errors are clustered at village level.[15] Furthermore, since we measure program impacts on a relatively large number of outcomes, it is essential to account for the simultaneous testing of multiple hypotheses. In order to do so, for all the outcomes we implement the stepwise multiple hypothesis testing procedure suggested by Romano and Wolf (2005), which adjusts the critical values used for each hypothesis being tested and corrects the p-values for the family-wise error rate.[16] We highlight in bold those coefficients for which we can reject the null that they are equal to zero after implementing this adjustment.

Across tables, in the first two columns we rely on intent-to-treat estimates by comparing outcomes between treatment and control groups, independently from actual participation in the spraying campaign. Given that compliance with spraying was not perfect, we additionally report Instru-

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[14] Our estimates are almost identical for models with and without controls (see appendix B.4.1), therefore we will refer in the paper to the estimates with controls. We exclude from the list of controls the dummy variables indicating whether the respondent slept in the house due to potential endogeneity. Results are unaffected by its inclusion.

[15] For binary outcomes, the coefficients are robust to estimating the treatment effect using a probit and bivariate probit models, instead of OLS and IV, respectively. See appendix B.4.

[16] We repeat the test separately for each specification presented in the paper, i.e. OLS without controls, OLS with controls and IV. The procedure is presented in appendix A. We consider simultaneously all hypotheses tested in the main outcome tables in the paper.

mental Variable estimates of the impact of IRS in column 5 of each table, where each household's participation in the IRS campaign is instrumented by the village level treatment indicator. In particular, we estimate the coefficient β in the following equation using a linear regression model augmented with an endogenous binary-treatment variable estimated by full maximum likelihood:

$$\begin{aligned} Y_{ij} &= \alpha + \beta \, Spray5m_i + X_i'\gamma + V_j'\delta + \epsilon_{ij} \qquad (3) \\ \Pr(Spray5m_i = 1 \mid T_j, X_i, V_j) &= \Phi\left(\theta_1 + \theta_2 \, T_j + X_i'\theta_3 + V_j'\theta_4 + \nu_{ij}\right) \qquad (4) \end{aligned}$$

where $Spray5m_i$ 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, and where Φ is the cumulative density function of the standard normal. Using linear probability models and linear IV estimators gives us essentially the same results. Also including households who reported not knowing whether the dwelling has been sprayed does not affect the results (see appendix B.4.2).

Table 4 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 include only those nets that were properly treated with insecticide at the time of the survey, while we use the latter term to additionally include those nets that had not been properly re-treated. We include in the ITN definition 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 and all ITNs either acquired in the 3 years prior to the survey or re-treated in the 12 months before the survey. On average, 0.91 nets per household were used the previous night and 0.59 nets were left unused in the control group villages. Furthermore, in the same villages, there were about 1.58 nets (1.22 ITNs) per household. 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 treated with insecticide at the time of the survey.[17]

Table 4 also presents the estimated program effects on ownership and use of bed nets.[18] The number of nets used the night before the survey was 0.24 higher in treated villages, but there was no discernible difference in the number of unused nets between treatment and control. Households living in treated villages own 0.25 more nets and 0.24 more ITNs than households from control villages. We jointly test and reject (at the 1 percent level of significance) that there is no difference in these four variables between treatment and control villages. These results show a clear difference in net ownership and use between treatment and control villages that is robust to multiple hypothesis testing. When looking at the effect of IRS on the extensive margin of net ownership, we observe an increase of 5.5 percentage points in the share of households owning at least one

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[17] We do not study explicitly households' participation in net re-impregnation activities because LLINs have progressively replaced traditional ITNs since the NMCP discontinued its distribution in 2006.

[18] We focus on the intensive margin of net ownership, as we refer to the total number of nets owned or observed per household. In all estimations where controls are included household size is added as regressor to control for potential unbalances. Estimating models 2 and 3 using the per capita number of nets leads to the same conclusions (see appendix B.8).

Table 4: Ownership of mosquito bed nets

				$E(Y\|T=1,X) - E(Y\|T=0,X)$		
		(1) Control	(2) Treatment	(3) OLS	(4) OLS	(5) IV
1. Number of nets owned by household		1.575	1.795	**0.220****	**0.248*****	**0.278*****
		[1.210]	[1.277]	(0.111)	(0.082)	(0.104)
	N =	763	762	1525	1525	1382
2. Number of observed nets		1.503	1.721	**0.218****	**0.246*****	**0.285*****
		[1.124]	[1.190]	(0.106)	(0.075)	(0.097)
	N =	748	745	1493	1493	1350
2a. Used the night before		0.914	1.165	**0.251****	**0.237*****	**0.302*****
		[1.051]	[1.230]	(0.102)	(0.082)	(0.113)
	N =	748	745	1493	1493	1350
2b. Unused the night before		0.588	0.556	-0.033	0.009	-0.018
		[0.944]	[0.933]	(0.066)	(0.062)	(0.088)
	N =	748	745	1493	1493	1350
3. Number of observed ITNs		1.217	1.411	**0.194****	**0.244*****	**0.299*****
		[1.118]	[1.208]	(0.098)	(0.081)	(0.109)
	N =	756	754	1510	1510	1368
3a. Used the night before		0.753	0.966	**0.213****	**0.221*****	**0.280*****
		[0.980]	[1.164]	(0.087)	(0.079)	(0.106)
	N =	756	754	1510	1510	1368
3b. Unused the night before		0.464	0.446	-0.019	0.024	0.016
		[0.858]	[0.853]	(0.063)	(0.059)	(0.084)
	N =	756	754	1510	1510	1368
Controls				No	Yes	Yes
Joint test on variables: 1-2-3			p-values	0.103	0.002	-
			N	1489	1489	-
Joint test on variables: 2a-3a			p-values	0.030	0.009	-
			N	1480	1480	-

Note: *** p<0.01, ** p<0.05, * p<0.1. We use one observation per household. Variables 2 and 3 are observed by the interviewer, while variable 2 is self-reported. “Nets” refers to any bed nets, irrespective of their treatment status, “ITNs” includes only LLINs and properly treated ITNs. Columns (1) and (2) report sample means in control and treatment groups, with standard deviations in brackets. Columns (3) and (4) report the difference between treatment and control groups estimated using OLS regression (model 2). Column (5) estimates the difference between households who report to have received the spraying campaign in the last 5 months and those who didn’t by instrumenting program participation with the treatment group indicator (model 3). In columns (3)-(5), standard errors clustered at village level are reported in parentheses. Controls include gender, age, education, household size, tribe and religion, information about access to water, dwelling characteristics, regional dummies and village characteristics (share of women living in the village and a dummy for pre-intervention high vegetation areas). To control for joint significance, we run a probit regression of the treatment indicator on the selected groups of variables and we report p-values of an F-test for the joint significance of the coefficients on the selected variables. We highlight in bold coefficients for which we cannot reject at 10% of significance level the null hypothesis of no effect of IRS when adjusting the critical values for multiple hypothesis testing (see appendix A).

net and an increase of 5.8 percentage points in the share of households owning at least one ITN, which is only significant at 10% (see table B12 in appendix B.8). These estimates are not significant when we distinguish between used and unused nets and when we exclude control variables from the estimation. These results are partially in line with Keating et al. (2011) and suggest that the extensive margin do not explain all of the increase that is observed on the intensive margin.

IRS may affect bed net ownership through an increase in malaria awareness. We build an index of awareness and knowledge of malaria using all available information on whether respondents believe malaria is a problem in the community, whether they are aware of the main channel of transmission, and whether they are informed of the categories of individuals that are most affected by the infection.[19] We limit our analysis to information and awareness about malaria, since data about subjective expectations about the efficacy of different technologies are not available in this survey.[20] Table 5 shows that concern and knowledge of malaria is high in both treatment and control villages.

Despite the fairly low levels of parasite prevalence in the region, malaria is still (correctly) perceived as a problem in the community by a large majority of the population and there is widespread knowledge that mosquitoes are an important transmission vector. This can be related to the fact that while the number of cases identified through RDTs in October 2009 are low, the area experienced high levels of malaria prevalence in the past and a steep reduction over the past decade. Furthermore, about half of the respondents were aware of information campaigns conducted during the 6 months prior to the interview, concerning ITNs, early seeking behaviour (seeking timely treatment and proper diagnostic of malaria symptoms) and environmental management. However, there was no difference in this set of variables between treatment and control villages.

Table 5 presents the estimated effect of the IRS campaign on concern and knowledge of malaria. Our estimates suggest that treatment increased the index by 0.03. There is more concern with malaria transmission in treatment than in control villages, suggesting that IRS provision led individuals to update their beliefs about the importance of malaria in their communities. The increased concern and knowledge of malaria may have changed the expected returns to malaria prevention behaviours such as ITN use[21]. Past exposure, as proxied by the 2000-2009 average

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[19] The exact questions are “Is Malaria a problem in this community?”, “How does one get malaria?” and “Who is most affected by malaria?”. We average 16 dummy variables representing answers to these questions. For each variable, the respondent scores 1 if the answer is in line with concern or correct knowledge of malaria and 0 if the answer indicates wrong (or absent) knowledge of malaria. The index is equal to 1 if the respondent is concerned and fully aware of malaria. R^2 of a regression of the index on all village dummies is equal to 0.148, showing that there exist a significant within-village variation in concern about and knowledge of malaria. We discuss the construction of the index in detail in appendix B.3.

[20] To our knowledge there is no study documenting subjective expectations in areas with low malaria prevalence in the present, but high prevalence in the past. Mahajan et al. (2009) provide evidence of subjective expectations of contracting malaria in an area where prevalence was high at the time of the study (Orissa, India) under three scenarios (no net, net and ITN). They show that respondents report on average 9 chances out of 10 to contract malaria when no net is used versus 4.6 when sleeping under a net and 0.6 when sleeping under a ITN. No data is available for the use of IRS technology.

[21] It is important to note that an independent increase in salience about malaria would induce an increase in net

Table 5: Information and knowledge about malaria

				$E(Y\|T=1,X) - E(Y\|T=0,X)$		
		(1) Control	(2) Treatment	(3) OLS	(4) OLS	(5) IV
1. Concern and knowledge of malaria		0.805	0.843	**0.038*****	**0.032*****	**0.038*****
		[0.193]	[0.143]	(0.012)	(0.011)	(0.013)
	N =	755	760	1515	1515	1376
2. Heard or saw messages about:						
2a. ITNs		0.464	0.482	0.018	-0.012	-0.007
		[0.499]	[0.500]	(0.042)	(0.034)	(0.044)
	N =	761	764	1525	1525	1383
2b. Early seeking behaviour		0.499	0.538	0.039	-0.001	-0.004
		[0.500]	[0.499]	(0.042)	(0.033)	(0.045)
	N =	760	764	1524	1524	1383
2c. Environmental management		0.382	0.449	0.067	0.023	0.035
		[0.486]	[0.498]	(0.044)	(0.035)	(0.049)
	N =	762	764	1526	1526	1384
Controls		-	-	No	Yes	Yes
Joint test on variables: 2a-2c			p-values	0.450	0.841	-
			N	1521	1521	-

Note: *** p<0.01, ** p<0.05, * p<0.1. We use one observation per household. Variable 2 refers to the 6 months previous to the interview. Columns (1) and (2) report sample means restricted to control and treatment group, standard deviations in brackets. Columns (3) and (4) report the difference between treatment and control groups using OLS regression (model 2). Column (5) estimates the difference between households who report to have received the spraying campaign in the last 5 months and those who didn't by instrumenting program participation with the treatment group indicator (model 3). In columns (3)-(5), standard errors clustered at village level are reported in parentheses. Concern and knowledge of malaria is an index computed by averaging 16 dummy variables representing information on whether respondents believe malaria is a problem in the community, are acknowledged of the malaria vector and are informed of the categories of individuals that are most affected by the infection. The index is equal to 1 if the respondent is concerned and fully aware of malaria. We discuss the construction of the index in detail in appendix B.3. Controls include gender, age, education, household size, tribe and religion, information about access to water, dwelling characteristics, regional dummies and village characteristics (share of women living in the village and a dummy for pre-intervention high vegetation areas). To control for joint significance, we run a probit regression of the treatment indicator on the selected groups of variables and we report p-values of an F-test for the joint significance of the coefficients on the selected variables. We highlight in bold coefficients for which we cannot reject at 10% of significance level the null hypothesis of no effect of IRS when adjusting the critical values for multiple hypothesis testing (see appendix A).

NDVI, is positively correlated with higher concern and knowledge of malaria and, at the same time, the treatment effect of providing IRS is unaffected by introducing controls on average past exposure (see appendix B.3). It is also important to report that, during the 6 months preceding the survey, respondents in treatment villages did not receive significantly more information on ITNs, early seeking behaviour and environmental management, compared to those in the control group. These variables are not statistically different in treatment and control groups, either when we look at them individually or jointly. Any changes in information and knowledge are likely to be a direct consequence of the IRS campaign. Relative to the magnitude of the effect of the program on net ownership (table 4), the effect on knowledge and concern is relatively small. While it is true that these variables are probably imperfect measures of the true level of knowledge and concern in these villages, this result also suggests that the change in knowledge and concern can only partly explain the overall change in net ownership.

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. More generally, 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 intervention, 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, an introduction of information in this manner may lead individuals to update their beliefs and, as a result, change their behaviours. These changes are generally not expected by those designing the program, while this section shows that they can be quite important. While our results on information can be seen as a bit tentative, they are certainly suggestive of the possible importance of the mechanism we emphasize.

Individuals can engage in other activities that can reduce the risk of malaria infection in response to the IRS campaign. For example, they can increase prevention by participating in environmental management campaigns, such as LHM.[22] Table 6 focuses on participation in these campaigns and shows that it is fairly low across a variety of measures, as also pointed out in Keating et al. (2011). Table 6 also report estimates of the impact of IRS on participation to LHM campaigns. We find no significant impact.[23] It is important to note that LHM is a rather different preventive policy compared to IRS, since it often requires coordination within the community in order to be implemented. This is definitely the case in Eritrea, where villages organize their households into shifts when it comes to LHM activities. In fact, LHM is more a programmatic

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ownership. However, available data does not allow to differentiate between salience and knowledge.

[22] We also look at activities that are indirectly leading to a reduced risk of malaria infection, such as keeping livestock away from the dwelling or taking action to avoid mosquito bites. We do not find evidence that IRS affected private investment in any of those behaviours. See appendix B.6.

[23] Standard errors are relatively small in table 6, so we would have been able to detect a small impact of IRS on participation to LHM, had there been any. Most coefficients have a positive sign, whereas a negative sign would hint to the presence of crowd-out.

intervention with localized benefits, while ITN can be seen as a personal protection.

5. Conclusions

The concern that government intervention crowds out desirable private behaviour is common to several areas of public policy. The standard perfect information model predicts that this would 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, and in particular they are considering the introduction of regular IRS campaigns to achieve this goal, whereas IRS has so far been chiefly used in 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 elimination. 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 to consistently make use of the available preventive technologies to ensure that malaria elimination can be achieved in the medium run.

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 behaviours in which villagers are engaged. We show instead that IRS increased average ownership and use of ITNs.

Although the prevalence of malaria parasite infections was found to be low in this area, we observe 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. Public health interventions may act as marketing campaigns, capable of promoting take-up of existing preventive technologies, and as an information campaign that fosters active use of 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.

Table 6: Participation in Larval Habitat Management (LHM)

			$E(Y \mid T=1,X) - E(Y \mid T=0,X)$		
	(1) Control	(2) Treatment	(3) OLS	(4) OLS	(5) IV
1. Respondent participated in LHM	0.276	0.323	0.047	0.015	0.019
	[0.447]	[0.468]	(0.045)	(0.036)	(0.046)
N =	695	694	1389	1389	1376
2. Days spent by household in LHM	0.602	0.651	0.048	0.000	-0.009
	[1.965]	[2.850]	(0.190)	(0.158)	(0.212)
N =	757	753	1510	1510	1367
3. Any household member participated in LHM					
3a. All members	0.387	0.449	0.062	0.022	0.020
	[0.902]	[0.932]	(0.077)	(0.067)	(0.096)
N =	768	764	1532	1532	1389
3b. Male members >15 y.o.	0.121	0.166	0.045	0.025	0.030
	[0.385]	[0.458]	(0.031)	(0.025)	(0.031)
N =	768	764	1532	1532	1389
3c. Female members >15 y.o.	0.219	0.212	-0.007	-0.017	-0.029
	[0.483]	[0.452]	(0.038)	(0.033)	(0.047)
N =	768	764	1532	1532	1389
3d. Members <15 years old	0.047	0.071	0.024	0.014	0.015
	[0.380]	[0.445]	(0.026)	(0.027)	(0.043)
N =	765	760	1525	1525	1382
Controls	-	-	No	Yes	Yes
Joint test on variables: 1,2,3b-3d		p-values	0.235	0.496	-
		N	1365	1365	-

Note: *** p<0.01, ** p<0.05, * p<0.1. We use one observation per household. Variable 1 refers to the 6 months previous to the interview, while variables 2 and 3 refers to the month previous to the interview. Columns (1) and (2) report sample means restricted to control and treatment group, standard deviations in brackets. Columns (3) and (4) report the difference between treatment and control groups using OLS regression (model 2). Column (5) estimates the difference between households who report to have received the spraying campaign in the last 5 months and those who didn't by instrumenting program participation with the treatment group indicator (model 3). Standard errors clustered at village level are reported in parentheses. Controls include gender, age, education, household size, tribe and religion, information about access to water, dwelling characteristics, regional dummies and village characteristics (share of women living in the village and a dummy for pre-intervention high vegetation areas). To control for joint significance, we run a probit regression of the treatment indicator on the selected groups of variables and we report p-values of an F-test for the joint significance of the coefficients on the selected variables. The joint test omits the variable 3a since it is just the sum of 3b, 3c and 3d. In columns (3)-(5), standard errors clustered at village level are reported in parentheses. We highlight in bold coefficients for which we cannot reject at 10% of significance level the null hypothesis of no effect of IRS when adjusting the critical values for multiple hypothesis testing (see appendix A).

Regarding the external validity of our findings, it is not possible to argue that we will find similar effects in other settings. After all, we are studying a very small experiment in a very special location. Nevertheless, we believe that the principles we uncovered are fairly general and could be at work in many other settings. Observing such a change in beliefs was likely dependent on malaria prevalence being relatively low in the study region. In such environments, populations may be more prone to changing beliefs and behaviours concerning health when they notice any potential causes for alarm, and especially when they are very visible, as in the case of IRS treatments.

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Appendix to "Do Public Health Interventions Crowd Out Private Health Investments? Malaria Control Policies in Eritrea"
FOR ONLINE PUBLICATION ONLY

A. Multiple hypothesis testing

This section presents the procedure for multiple hypothesis testing for the coefficients in tables 4-6. We repeat the test separately for each specification, i.e. OLS without controls, OLS with controls and IV. We follow the Studentized k-StepM Method for Two-Sided Setup (Romano and Wolf, 2005; Romano et al., 2008). Our data is represented by a data matrix X_N, where N is the number of observations, which is generated from some underlying (unknown) probability mechanism P. Interest focuses on the parameter vector $\theta = (\beta_1, ..., \beta_S)'$, where each β_s is the parameter on the treatment indicator T in equation (2) (with or without controls) or on $Spray5m$ in equation (3). S corresponds to the number of hypothesis tested. Individual hypotheses concern the elements of θ and are two-sided: $H_s : \beta_s = 0 \; vs \; H_s' : \beta_s \neq 0$. We implement the following procedure:

1. Let $\hat{\theta}_N$ denote an estimator of θ (with standard error $\hat{\sigma}_{N,s}$) computed from the original data matrix X_N using the specifications presented in Section 4.
2. For each hypothesis H_s, $1 \leq s \leq S$, we compute a studentized test statistics $z_{N,s} = \hat{\beta}_{N,s}/\hat{\sigma}_{N,s}$ from the data matrix X_N. We relabel $z_{N,s}$ in descending order of the absolute studentized test statistics $|z_{N,s}|$: r_1 corresponds to the largest absolute studentized test statistic and strategy r_S to the smallest one, e.g. $z_{N,r_1} \geq z_{N,r_2} \geq ... \geq z_{N,r_S}$.
3. Generate M bootstrap data matrices $X_N^{*,m}$ with $1 \leq m \leq M$ clustered at village level. Romano et al. (2008) suggest to use at least $M \geq 1000$, we use $M = 2000$. We exclude 0.1 percent of iterations where at least one estimation does not converge in the IV specification.
4. From each bootstrap data matrix, we compute estimates $\hat{\beta}_{N,1}^{*,m}, ..., \hat{\beta}_{N,S}^{*,m}$ and standard errors $\hat{\sigma}_{N,1}^{*,m}, ..., \hat{\sigma}_{N,S}^{*,m}$ using the same specifications as in Step 1. Then set $j = 1$ and $R_0 = 0$.
5. For $1 \leq m \leq M$, we compute $max_{N,j}^{*,m} = max_{R_{j-1}+1 \leq s \leq S} \left(\left| \hat{\beta}_{N,r_s}^{*,m} - \hat{\beta}_{N,r_s} \right| / \hat{\sigma}_{N,r_s}^{*,m} \right)$.
6. Compute $\hat{d}_j$ as the $1 - \alpha$ empirical quantile of the M values $max_{N,j}^{*,m}$. For $R_{j-1} + 1 \leq s \leq S$, if $|z_{N,r_s}| > \hat{d}_j$, reject the null hypothesis H_{r_s}.
7. If no further hypotheses are rejected, the procedure stops. Otherwise, denote by R_j the number of hypotheses rejected so far, let $j = j + 1$ and return to Step 5.

We implement the procedure for three different levels of significance: 0.01, 0.05 and 0.1. Application of the procedure leads to the following adjusted critical t-statistics for $j = 1$ ($\hat{d}_1$):

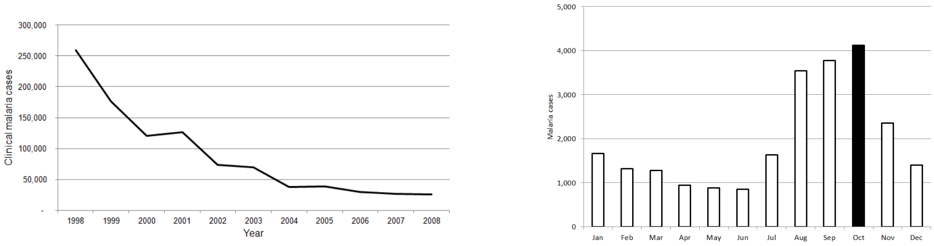
α	**OLS without controls**	**OLS with controls**	**Instrumental Variable**
0.01	2.69	2.90	2.91
0.05	2.18	2.42	2.47
0.10	1.95	2.18	2.25

B. Additional Data Analysis

B.1. Malaria prevalence

Malaria prevalence was extremely low in the area under investigation, but the study was conducted in an area where malaria prevalence was drastically reduced over the past decade. The number of clinical malaria cases declined sharply in Eritrea over the past decade, from 260,000 in 1998 to 26,000 in 2008 (figure B1, Panel A).

Figure B1: Clinical malaria cases in Eritrea and Gash Barka



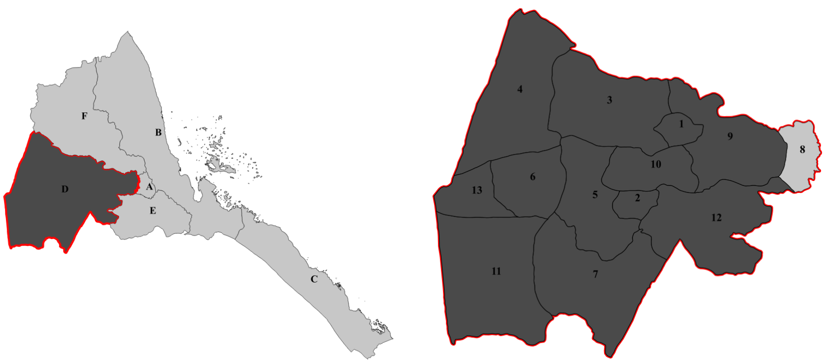
A. Yearly cases, Eritrea (1998-2008) *B. Monthly cases, Gash Barka (2002-2007)*

Note: Panel A presents the number of yearly cases of malaria in Eritrea in the period 1998-2008. Panel B presents the monthly average number of malaria cases in Gash Barka for the period 2002-2007. Sources: Eritrea Malaria Five Year Strategic Plan; NMCP Eritrea Annual Report 2008.

The area selected for this study, Gash Barka (see figure B2) is the zone where most cases are concentrated. It witnessed a similar trend, recording 110,000 cases in 1998 and 18,000 cases in 2008. Secondly, malaria transmission is typically seasonal: it extends from July until November-December and it reaches a peak between September and November, period during which the survey was conducted (October). This pattern is shown in panel B of figure B1, which presents the average number of malaria cases (including both in-patient and out-patient departments malaria cases) over the year in Gash Barka over the period 2002-2007.

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. Keating et al. (2011) shows that 5,502 people were tested with RDT, and among those 13 individuals tested positive in the control group and 17 tested positive in the treatment group. The difference in the share of positive RDTs between the two groups is very small (and positive) and not significant. These figures are in line with those provided by the NMCP of Eritrea. The total number of malaria cases registered by NMCP in Gash Barka in 2008 was 20,320, which is about 3% of the estimated population living in the region (670,000). We tested 5,502 people in the survey, therefore the expected number of malaria cases among them over the whole year is 166, i.e. 3% of 5,502. Due to seasonality of malaria, the yearly share of malaria cases occurred in September between 2002-2007 was 15%. Positive RDTs indicate a malaria infection that occurred in the month prior to the test. September is roughly the month before the survey. Therefore the expected number of positive RDTs at the beginning of October was 25, i.e., 15% of 166. The number of positive RDTs in our sample is a bit larger than this, possibly because not all malaria patients report to health facilities.

Figure B2: Location of Zone Gash Barka in Eritrea and selected sub-zones

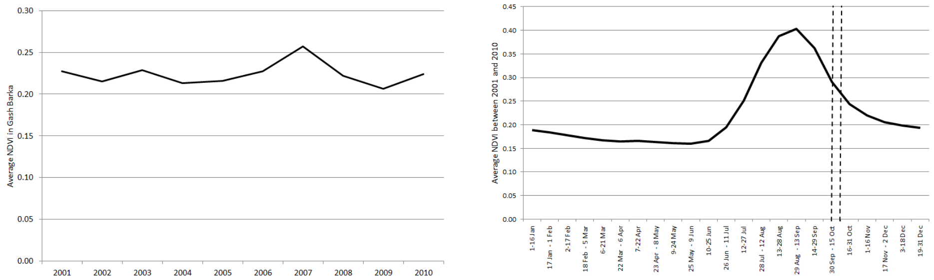

A. Zone selected *B. Sub-zones selected*

Note: Panel A shows the administrative division of Eritrea in the following Zones: A. Anseba, B. Derub, C. Debubawi Keyih Bahri, D. Gash Barka, E. Makel, F. Semenawi Keyih Bahri. The zone selected for the study (Gash Barka) is highlighted in darker color. Panel B presents the division of Gash Barka into its administrative sub-zones: 1. Akordat, 2. Barentu, 3. Dghe, 4. Forto, 5. Gogne, 6. Haykota, 7. La'Elay Gash, 8. Logo Anseba, 9. Mansura, 10. Mogolo, 11. Omhajer (Guluj), 12. Shemboko (Shambuko) and Molki, 13. Tesseney. The sub-zones highlighted in darker color were the one selected for the study.

B.2. *Normalized Difference Vegetation Index (NDVI)*

Vegetation data was retrieved from the website of the International Research Institute for Climate and Society of Columbia University. In the absence of water surfaces or snow, it ranges between 0 and 1, where 1 means most vegetation and 0 stands for least vegetation. Over the period July 1981-December 2009, the NDVI in Gash Barka ranged between 0.073-0.714 and varies widely across sub-zones. The vegetation level remained fairly stable (figure B3), suggesting that policies of the NMCP may have been crucial to fight malaria. Vegetation starts increasing in July, following the inception of the rainy season, peaks in September and declines by the end of October.

Figure B3: NDVI in Gash Barka (2001-2010)


A. Average yearly NDVI *B. Seasonality of NDVI*

Note: Panel A shows the yearly average NDVI in Gash Barka. Panel B presents the average NDVI in Gash Barka by week. The time in between of the dotted lines shows the period in which the survey was implemented. Source: International Research Institute for Climate and Society (IRI), Columbia University. The dashed vertical lines show the period when the survey was conducted, i.e., the second week of October.

For each sub-zone and year we counted the number of 2-week periods in which NDVI exceeded 0.361 during the period 2000-2009 (table A in figure B4). We also tried a lower threshold of 0.3 to allow for a possibly lower threshold (table B in figure B4). Cells are coloured from red (arid) to green or blue (more vegetation).

Figure B4: Classification of sub-zones of Gash Barka by vegetation level

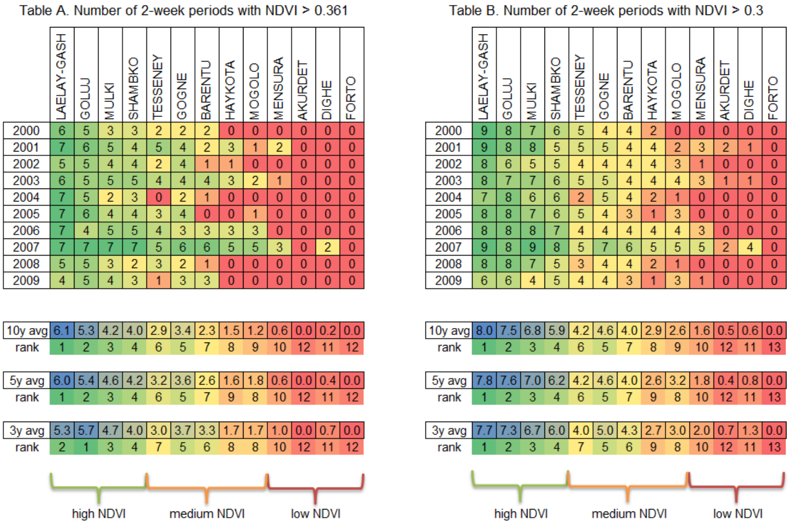


Table A. Number of 2-week periods with NDVI > 0.361

	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

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

Note: for each sub-zone, 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.", "5y avg." and "3y avg." is the column average respectively for the last 10, 5 and 3 years. Sub-zones are sorted from left to right according to their rank in 10-year average number of 2-week periods with NDVI above the threshold. Source: International Research Institute for Climate and Society (IRI), Columbia University.

B.3. Concern and knowledge of malaria

To build this index we made use of three set of dummies. The first set of dummy variables concerns knowledge about the vector (or the cause) of malaria. Table B1 presents the share of respondents in the control and treatment groups who mentioned each vector/cause of malaria (with the respondent allowed to report multiple answers) and the estimated effect of the IRS campaign. While there is widespread knowledge that mosquitoes are an important transmission vector, there is quite large share of respondents mentioning wrong causes such as unhygienic surroundings, poor diet or fatigue.

A second set of variables indicates whether the respondent believes malaria is a problem in the village. Table B2 shows that, in spite of the fairly low levels of parasite prevalence in the region, 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, notice that around 30 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. The Global Malaria Action Plan of the Roll Back Malaria initiative 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". It can lead the public to reduce use of available preventive and treatment measures.

Table B1: Knowledge about the cause of malaria

	(1) Control	(2) Treatment	(3) OLS	(4) OLS	(5) IV
			$E(Y\|T=1,X)-E(Y\|T=0,X)$		
Mosquitoes	0.854	0.919	0.065***	0.064***	0.089***
	[0.354]	[0.273]	(0.021)	(0.019)	(0.025)
Contaminated water/Unhygienic surroundings	0.212	0.220	0.008	0.010	0.021
	[0.409]	[0.415]	(0.024)	(0.024)	(0.032)
Fatigue/Too much time in the sun	0.157	0.169	0.012	-0.002	-0.013
	[0.364]	[0.375]	(0.025)	(0.021)	(0.028)
Poor diet/Eating dirty food	0.254	0.262	0.009	0.000	0.005
	[0.435]	[0.440]	(0.034)	(0.030)	(0.038)
Tall grass/Wet areas	0.133	0.114	-0.019	-0.010	-0.008
	[0.340]	[0.318]	(0.023)	(0.020)	(0.027)
From person to person	0.022	0.018	-0.004	-0.003	-0.008
	[0.148]	[0.134]	(0.010)	(0.007)	(0.010)
During outbreaks	0.003	0.003	0.000	0.001	0.003
	[0.051]	[0.051]	(0.003)	(0.002)	(0.003)
Other reasons	0.071	0.031	-0.039***	-0.037***	-0.050***
	[0.256]	[0.175]	(0.014)	(0.014)	(0.018)
Respondent doesn't know	0.067	0.033	-0.034***	-0.028**	-0.037**
	[0.250]	[0.178]	(0.013)	(0.012)	(0.015)
Controls	-	-	No	Yes	Yes
Observations	765	763	1528	1528	1386

Note: *** p<0.01, ** p<0.05, * p<0.1. One observation per household, data available for respondents only. Columns (1) and (2) report sample means restricted to control and treatment group, standard deviations in brackets. Columns (3) and (4) report the difference between treatment and control groups using OLS regression (model 2). Column (5) estimates the difference between households who report to have received the spraying campaign in the last 5 months and those who didn't by instrumenting program participation with the treatment group indicator (model 3). Standard errors clustered at village level are reported in parentheses. Controls include gender, age, education, household size, tribe and religion, information about access to water, dwelling characteristics, regional dummies and village characteristics (share of women living in the village and a dummy for pre-intervention high vegetation areas). To control for joint significance, we run a probit regression of the treatment indicator on the selected groups of variables and we report p-values of an F-test for the joint significance of the coefficients on the selected variables.

A third set of variables focus on whether the respondent believes a certain category of individuals is most affected by malaria. 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.

To check whether the index captures pre-existent differences in exposure to malaria, we counted the number of 2-week periods in which NDVI exceeded 0.361 during the period 2000-2009 and we divided villages into three different groups: "very limited vegetation (low past exposure)" , "some vegetation (middle past exposure)" and "significant vegetation (high past exposure)". Past exposure is positively correlated with higher concern and knowledge of malaria, but the treatment effect is robust to this control (see table B3).

B.4. Estimation method

B.4.1. Robustness to the inclusion of different controls

This section presents evidence on the robustness of the estimates of the effect of IRS to the the inclusion of different sets of controls. We focus here on net use, but results are similar across all the outcomes considered. In the paper, we consider four different sets of controls. *Village-level* controls includes regional dummies, an indicator variable for high vegetation and the share of women

Table B2: Concern about malaria

				$E(Y\|T=1,X) - E(Y\|T=0,X)$		
		(1) Control	(2) Treatment	(3) OLS	(4) OLS	(5) IV
1. Malaria is a problem in the village						
Yes		0.654	0.709	0.056	0.042	0.058
		[0.476]	[0.454]	(0.047)	(0.038)	(0.052)
	N =	768	764	1532	1532	1389
Respondent doesn't know		0.023	0.021	-0.002	0.002	0.001
		[0.151]	[0.143]	(0.009)	(0.008)	(0.011)
	N =	768	764	1532	1532	1389
2. Most affected by malaria:						
Children		0.807	0.867	0.060**	0.047**	0.054*
		[0.395]	[0.339]	(0.025)	(0.022)	(0.031)
	N =	756	761	1517	1517	1378
Pregnant women		0.369	0.367	-0.002	-0.012	-0.032
		[0.483]	[0.482]	(0.041)	(0.032)	(0.041)
	N =	756	761	1517	1517	1378
Adult men		0.029	0.037	0.008	0.001	-0.007
		[0.168]	[0.188]	(0.011)	(0.010)	(0.012)
	N =	756	761	1517	1517	1378
Adult women		0.036	0.030	-0.005	-0.004	-0.004
		[0.186]	[0.171]	(0.009)	(0.008)	(0.011)
	N =	756	761	1517	1517	1378
Respondent doesn't know		0.111	0.068	-0.043*	-0.034*	-0.033
		[0.314]	[0.252]	(0.023)	(0.019)	(0.027)
	N =	756	761	1517	1517	1378
Controls				No	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. One observation per household, data available for respondents only. Columns (1) and (2) report sample means restricted to control and treatment group, standard deviations in brackets. Columns (3) and (4) report the difference between treatment and control groups using OLS regression (model 2). Column (5) estimates the difference between households who report to have received the spraying campaign in the last 5 months and those who didn't by instrumenting program participation with the treatment group indicator (model 3). Standard errors clustered at village level are reported in parentheses. Controls include gender, age, education, household size, tribe and religion, information about access to water, dwelling characteristics, regional dummies and village characteristics (share of women living in the village and a dummy for pre-intervention high vegetation areas). To control for joint significance, we run a probit regression of the treatment indicator on the selected groups of variables and we report p-values of an F-test for the joint significance of the coefficients on the selected variables.

in the village. *Tribe* controls include dummy variables for the tribe of the household. *Respondent-level* controls include gender, age and other demographics of the respondent. *Household-level* controls include information about household structure, dwelling characteristics and access to water. Table B4 presents estimates of treatment effect using model 2 on the household net use. We can observe that the coefficient is robust to the inclusion of different sets of controls.

B.4.2. Endogenous participation and missing values

To measure take up we rely on self-reported participation in the program. The self-reported participation is however affected by households who reported that they didn't know whether their dwelling had been sprayed. We can construct endogenous participation by computing the share of households within each village who have reported to have participated in the spraying campaign out of the overall population. Table B5 presents first stage regressions using both variables measuring endogenous participation. Table B6 shows that using endogenous participation at individual or at village level is not significantly affecting the coefficients.

Table B3: Concern and knowledge of malaria and past exposure

	Dependent variable: Concern and knowledge of malaria			
	(1) OLS	(2) OLS	(3) IV	(4) IV
Treatment	0.032*** (0.011)	0.026** (0.010)	0.038*** (0.013)	0.034** (0.013)
Some vegetation (middle past exposure)		0.046*** (0.016)		0.030* (0.017)
Significant vegetation (high past exposure)		0.077*** (0.024)		0.065*** (0.024)
Controls	Yes	Yes	Yes	Yes
Observations	1515	1515	1376	1376

Note: *** p<0.01, ** p<0.05, * p<0.1. One observation per household. Concern and knowledge of malaria is an index computed by averaging 16 dummy variables representing information on whether respondents believe malaria is a problem in the community, are acknowledged of the malaria vector and are informed of the categories of individuals that are most affected by the infection. The index is equal to 1 if the respondent is concerned and fully aware of malaria. Columns (1) and (2) report the difference between treatment and control groups using OLS regression (model 2). Columns (3) and (4) estimate the difference between households who report to have received the spraying campaign in the last 5 months and those who didn't by instrumenting program participation with the treatment group indicator (model 3). Some vegetation and Significant vegetation are dummy variables indicating the vegetation level at sub-zone level in the period 2000-2009. Standard errors clustered at village level are reported in parentheses. All specifications include controls for gender, age, education, household size, tribe and religion, information about access to water, dwelling characteristics, regional dummies and a dummy for pre-intervention high vegetation areas.

Table B4: Effect on IRS on net use with different sets of controls

	Dep.Variable: Number of observed nets used the night before				
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS
Treatment	0.251** (0.102)	0.246** (0.097)	0.241*** (0.083)	0.236*** (0.083)	0.237*** (0.082)
Village controls	No	Yes	Yes	Yes	Yes
Household controls	No	No	Yes	Yes	Yes
Tribe controls	No	No	No	Yes	Yes
Respondent controls	No	No	No	No	Yes
Observations	1493	1493	1493	1493	1493

Note: We use one observation per household. Dependent variable is the number of observed nets used the night before the interview. "Nets" refers to any bed nets, irrespective of their treatment status, "ITNs" includes only LLINs and properly treated ITNs. The table reports the difference between treatment and control groups estimated using OLS regression (model 2) and using different sets of control variables. Controls include gender, age, education, household size, tribe and religion, information about access to water, dwelling characteristics, regional dummies and village characteristics (share of women living in the village and a dummy for pre-intervention high vegetation areas). Standard errors clustered at village level are reported in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

B.4.3. Non-linear methods for binary outcomes

For binary outcomes, in order to show robustness of the coefficients to non-linear models, we present estimates of the treatment effect using a probit model and, for IV estimation, a maximum-likelihood two-equation probit model. In other words, we estimate the following model

$$\Pr(Y_{ij} = 1 \mid T_j, X_i, V_j) = \Phi\left(\alpha + \beta\, T_j + X_i'\gamma + V_j'\delta + \epsilon_{ij}\right) \tag{B.1}$$

where T_j is the treatment indicator, X_i is a vector of individual and household characteristics, V_j is a vector of village characteristics, ϵ_{ij} is an individual specific error term and Φ is the cumulative distribution function of a standard normal distribution. When considering the imper-

Table B5: First stage regression of program participation on treatment status

	Dwelling sprayed		Share of dwellings sprayed	
	(1) Probit	(2) Probit	(3) OLS	(4) OLS
Treatment	0.762*** (0.0325)	0.773*** (0.0305)	0.758*** (0.0346)	0.765*** (0.0320)
Controls	No	Yes	No	Yes
Observations	1456	1389	1617	1532

Note: *** p<0.01, ** p<0.05, * p<0.1. One observation per household. In Columns (1) and (2) the dependent variable is a dummy variable equal to 1 if the household reported that their dwelling has been sprayed with IRS in the last 5 months and zero otherwise. In Columns (3) and (4) the dependent variable is the share of households in the village who reported their dwelling has been sprayed. Independent variable is equal to one if the household is in the treatment group or zero otherwise. Columns (1) and (2) report marginal effects. Standard errors clustered at village level are reported in parentheses. Controls include gender, age, education, household size, tribe and religion, information about access to water, dwelling characteristics, regional dummies and village characteristics (share of women living in the village and a dummy for pre-intervention high vegetation areas).

Table B6: Ownership of mosquito bed nets and alternative IV estimation

Endogenous regressor:	Dwelling sprayed			Share of dwellings sprayed
Assumption:	A (1) IV	B (2) IV	C (3) IV	D (4) IV
1. Number of nets owned by household	0.278*** (0.104)	0.395*** (0.107)	0.338*** (0.119)	0.324*** (0.105)
N =	1382	1525	1525	1525
2. Number of observed nets	0.285*** (0.097)	0.391*** (0.100)	0.341*** (0.111)	0.321*** (0.096)
N =	1350	1493	1493	1493
3. Number of observed ITNs	0.299*** (0.109)	0.379*** (0.111)	0.365*** (0.122)	0.319*** (0.105)
N =	1368	1510	1510	1510
Controls	Yes	Yes	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. One observation per household. "Nets" refers to any bed nets, irrespective of their treatment status, "ITNs" includes only LLINs and properly treated ITNs. Columns (1)-(3) is estimated using a linear regression model augmented with an endogenous binary variables (model 3). Column (4) is estimated using 2SLS. Standard errors clustered at village level are reported in parentheses. Controls include gender, age, education, household size, tribe and religion, information about access to water, dwelling characteristics, regional dummies and village characteristics (share of women living in the village and a dummy for pre-intervention high vegetation areas). In Assumption A missing values for the question whether the household reported to have their house sprayed in the previous 5 months are removed. In Assumption B, missing values are taking value 1 (sprayed). In Assumption C, missing values are taking value 0 (not sprayed). In Assumption D, missing values are kept for the computation of the village average, i.e. the share of dwellings sprayed is defined as the share of respondents in the village who reported to have their dwelling sprayed in the previous 5 months out of the village population.

fect compliance to the program, we estimate the following two-equation model using maximum-likelihood:

$$\Pr(Y_{ij} = 1 \mid Spray5m_i, X_i, V_j) = \Phi\left(\alpha + \beta\, Spray5m_i + X_i'\gamma + V_j'\delta + \epsilon_{ij}\right) \tag{B.2}$$

$$\Pr(Spray5m_i = 1 \mid T_j, X_i, V_j) = \Phi\left(\theta_1 + \theta_2\, T_j + X_i'\theta_3 + V_j'\theta_4 + \nu_{ij}\right) \tag{B.3}$$

where $Spray5m_i$ 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. Tables B7 present the results for the binary outcomes presented in table 5 in the main text, but using non-linear estimation methods. Results provide evidence on the robustness of the coefficients for binary outcomes to non-linear estimations methods.

Table B7: Information and knowledge about malaria (non-linear estimation)

				$E(Y \vert T=1, X) - E(Y \vert T=0, X)$		
Sub-sample		(1) Control	(2) Treatment	(3) Probit	(4) Probit	(5) IV-BP
2. In the previous 6 months, heard or saw messages about:						
2a. ITNs		0.464	0.482	0.018	-0.013	-0.006
		[0.499]	[0.500]	(0.042)	(0.036)	(0.043)
	N =	761	764	1525	1525	1383
2b. Early seeking behaviour		0.499	0.538	0.039	-0.003	-0.006
		[0.500]	[0.499]	(0.042)	(0.036)	(0.044)
	N =	760	764	1524	1524	1383
2c. Environmental management		0.382	0.449	0.067	0.021	0.032
		[0.486]	[0.498]	(0.043)	(0.038)	(0.048)
	N =	762	764	1526	1526	1384
Controls				No	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. One observation per individual in the selected sub-sample. The outcome variable Y is an indicator variable equal to 1 if the individual, in the 6 months prior to the interview, heard about different interventions and zero otherwise. Columns (1) and (2) report sample means restricted to control and treatment group, standard deviations in brackets. Columns (3) and (4) report the difference between treatment and control groups using probit regression (model B.1). Column (5) estimates the difference between households who report to have received the spraying campaign in the last 5 months and those who didn't by instrumenting program participation with the treatment group indicator and using a 2-equation probit model (model B.2). Standard errors clustered at village level are reported in parentheses. Controls include gender, age, education, household size, tribe and religion, information about access to water, dwelling characteristics, regional dummies and village characteristics (share of women living in the village and a dummy for pre-intervention high vegetation areas).

B.5. Stock of nets over time

The information about how and when the observed bed nets have been acquired was not directly observable by the enumerators conducting the interviews. We have to rely on self-reported information to provide evidence that net ownership is recent. For each observed bed net, the question "How long ago (in months) did your household obtain the mosquito net?" was asked to the respondent. We need to note that self-reported information might present very large measurement error in this case. Firstly, the information is reported by one person only within the household, the respondent, who might have limited information about the time in which the bed net was acquired. Secondly, we request information about the acquisition for each observed net, which means recalling information for multiple nets. Thirdly, we find evidence of rounding for the responses "6 months ago", "12 months ago" and "24 months ago". Fourthly, we ask only about the nets that are currently observed in the household and we don't ask information about nets that were used in the past and are currently not observed in the dwelling.

We make use of the reported information to construct the stock of nets (conditional on having the net being observed at the time of the interview) for each household for each month before the interview. This allows comparing the average stock of nets for the treatment and control group to check for significant differences. Table B8 presents the average number of nets for the control and treatment group 3, 6 and 12 months before the interview and the estimated difference using models (2) and (3). Results show evidence that bed nets were acquired recently, but we cannot draw clear conclusions due to the weaknesses of the information.

Table B8: Self-reported stock of (currently observed) nets over time

			$E(Y\|T=1,X) - E(Y\|T=0,X)$		
	(1) Control	(2) Treatment	(3) OLS	(4) OLS	(5) IV
Current stock	1.575	1.795	0.220**	0.248***	0.278***
	[1.210]	[1.277]	(0.111)	(0.082)	(0.104)
N =	763	762	1525	1525	1382
1 month before interview	1.961	2.141	0.180*	0.185***	0.234***
	[1.036]	[1.103]	(0.091)	(0.069)	(0.086)
N =	595	624	1219	1219	1209
3 months before interview	1.835	1.995	0.160*	0.177**	0.223**
	[1.098]	[1.134]	(0.088)	(0.072)	(0.090)
N =	595	624	1219	1219	1209
6 months before interview	1.714	1.875	0.161*	0.177**	0.223**
	[1.132]	[1.152]	(0.091)	(0.074)	(0.093)
N =	595	624	1219	1219	1209
12 months before interview	1.311	1.362	0.051	0.108	0.138
	[1.160]	[1.180]	(0.103)	(0.091)	(0.114)
N =	595	624	1219	1219	1209
Controls			No	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. One observation per household. “Nets” refers to any bed nets, irrespective of their treatment status, “ITNs” includes only LLINs and properly treated ITNs. Stock over time is built using self-reported information about how many months before the interview the household acquired the net. Columns (1) and (2) report sample means restricted to control and treatment group, standard deviations in brackets. Columns (3) and (4) report the difference between treatment and control groups using OLS regression (model 2). Column (5) estimates the difference between households who report to have received the spraying campaign in the last 5 months and those who didn’t by instrumenting program participation with the treatment group indicator (model 3). Standard errors clustered at village level are reported in parentheses. Controls include gender, age, education, household size, tribe and religion, information about access to water, dwelling characteristics, regional dummies and village characteristics (share of women living in the village and a dummy for pre-intervention high vegetation areas).

B.6. Behaviours conducive to malaria elimination

Individuals can engage in additional activities whose primary aim is not directly related to malaria prevention, but that are indirectly conducive of a reduced risk of malaria infection. Table B9 focuses on this type of behaviour, such as whether the household keeps livestock away from the dwelling and whether they take action to avoid mosquito bites. These behaviours refer to the question “What do you do to stop mosquitoes from biting you?” and do not refer directly to malaria. Therefore, some respondents might have focused on general mosquito bites rather than infectious bites, especially because 15% of the sample in the control group do not know that mosquitoes are the vector of malaria. Households engage in a wide variety of activities other than ITN use and LHM, but we do not find evidence that IRS affected private investment in any of those behaviours.

B.7. Heterogeneous treatment effects

It is possible that the impact of IRS varied across groups of individuals or households. 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. We analysed this possibility for the case of the malaria awareness and net ownership. Table B10 reports in columns 1 and 3 the estimates of heterogeneous treatment effects obtained from OLS regressions where the treatment status is interacted with dummy variables indicating the NDVI category.

Table B9: Behaviors conducive to malaria elimination, other than LHM

			$E(Y\|T=1,X) - E(Y\|T=0,X)$		
	(1) Control	(2) Treatment	(3) OLS	(4) OLS	(5) IV
1. Household keeps livestock >100m from home	0.780	0.804	0.024	0.040	0.048
	[0.414]	[0.397]	(0.032)	(0.030)	(0.037)
N =	410	460	870	870	860
2. Household covers stored water	0.573	0.564	-0.009	-0.011	-0.015
	[0.495]	[0.496]	(0.052)	(0.043)	(0.054)
N =	702	690	1392	1392	1378
3. Respondent does anything to prevent mosquito bites	0.798	0.839	0.041	0.033	0.038
	[0.402]	[0.368]	(0.032)	(0.029)	(0.037)
N =	694	695	1389	1389	1378
4. Respondent mentions burning coils	0.211	0.226	0.016	0.006	0.001
	[0.408]	[0.419]	(0.036)	(0.027)	(0.033)
N =	693	689	1382	1382	1371
5. Respondent mentions using net	0.642	0.679	0.037	0.051	0.064
	[0.480]	[0.467]	(0.040)	(0.034)	(0.044)
N =	693	689	1382	1382	1371
6. Respondent mentions using spray	0.022	0.026	0.004	0.012	0.018*
	[0.146]	[0.160]	(0.009)	(0.008)	(0.010)
N =	693	689	1382	1382	1371
7. Respondent mentions burning animal dung	0.048	0.061	0.013	0.006	0.008
	[0.213]	[0.239]	(0.015)	(0.014)	(0.017)
N =	693	689	1382	1382	1371
8. Respondent mentions burning herbs	0.053	0.049	-0.004	-0.023	-0.030
	[0.225]	[0.217]	(0.018)	(0.020)	(0.027)
N =	693	689	1382	1382	1371
9. Respondent mentions draining stagnant water	0.118	0.110	-0.008	-0.016	-0.027
	[0.323]	[0.313]	(0.022)	(0.019)	(0.025)
N =	693	689	1382	1382	1371
Controls			No	Yes	Yes

Note: outcomes 3-9 refers to the question "What do you do to stop mosquitoes from biting you?" and do not refer directly to malaria. We use one observation per household. Columns (1) and (2) report sample means restricted to control and treatment group, standard deviations in brackets. Columns (3) and (4) report the difference between treatment and control groups using OLS regression (model 2). Column (5) estimates the difference between households who report to have received the spraying campaign in the last 5 months and those who didn't by instrumenting program participation with the treatment group indicator (model 3). Controls include gender, age, education, household size, tribe and religion, information about access to water, dwelling characteristics, regional dummies and village characteristics (share of women living in the village and a dummy for pre-intervention high vegetation areas). Standard errors clustered at village level are reported in parentheses (*** p<0.01, ** p<0.05, * p<0.1). We highlight in bold coefficients for which we cannot reject at 10% of significance level the null hypothesis of no effect of IRS when adjusting the critical values for multiple hypothesis testing, using the procedure of Romano and Wolf (2005) as described in appendix A.

Workers may have been impacted by IRS campaign differently compared to unemployed adults, because the marginal cost of being infected might be higher. Similarly, for malaria awareness, columns 2 and 4 in table B10 reports estimates of heterogeneous treatment effects obtained from OLS regressions where the treatment status is interacted with the employment status of the respondent (the variable *work* is an indicator variable equal to 1 if the respondent is employed or self-employed and zero otherwise). Estimates show a significant 12% increase among workers (column 2). However, we don't observe any heterogeneous pattern in net ownership if the respondent is working.

We present heterogeneous treatment effects estimates on net ownership looking at other individual characteristics: literacy status, tribe, gender of household head, household size. Table B11 shows that households with unemployed respondents did not significantly differ from the ones with an employed respondent. Literate respondents[2] acquired more nets than those with an illiterate head (even if the difference is not statistically significant). We don't observe significant difference among tribes different than the Tigre tribe. The treatment effect was only slightly larger in male-headed households than in female-headed ones. We observe a larger effect in households in the third tercile of household size distribution. To conclude, we estimate heterogeneous treatment effects depending on household wealth[3]. Column 4 of table B11 shows the coefficient on the interaction between the treatment status and the dummy variables indicating whether the household in the $x-th$ tercile of the asset distribution. We don't observe a significant difference across different asset terciles, but we do observe a significant treatment effect for the second and the third tercile. This reinforces the finding that there is a relationship between net ownership and household wealth even if nets are distributed for free.

Table B10: Heterogeneous treatment effect on malaria awareness

Dependent variable:	Y = 1(Malaria is a problem)		Number of observed nets used the night before	
	(1)	(2)	(3)	(4)
	OLS	OLS	OLS	OLS
Sub-sample	All	All	Male	Female
Treatment	0.046	-0.009	0.398**	0.242**
	(0.076)	(0.047)	(0.157)	(0.113)
Treatment x ndvi=1	0.006		-0.232	
	(0.095)		(0.193)	
Treatment x ndvi=2	-0.005		-0.287	
	(0.096)		(0.234)	
ndvi=1	-0.022		0.131	
	(0.073)		(0.126)	
ndvi=2	0.065		-0.227	
	(0.087)		(0.224)	
T x work=1		0.120*		0.061
		(0.061)		(0.135)
Work		-0.056		-0.023
		(0.050)		(0.089)
Observations	1498	1277	1493	1269

Note: *** p<0.01, ** p<0.05, * p<0.1. One observation per household. The dependent variables are an indicator variable equal to 1 if the respondent reported that malaria is an issue in their community and zero otherwise (columns 1 and 2) and the number of observed nets used the night before (columns 3 and 4). Columns (1)-(4) report the difference between treatment and control groups using OLS regression (model 2) and the coefficients on interactions between the treatment status and vegetation index dummies in column (1) and (3) and between the treatment status and the employment status in columns (2) and (4). Standard errors clustered at village level are reported in parentheses. All specifications include controls for gender, age, education, household size, tribe and religion, information about access to water, dwelling characteristics, regional dummies and village characteristics (share of women living in the village and a dummy for pre-intervention high vegetation areas).

B.8. *Extensive versus intensive margin*

In the main text we refer to the number of nets as the total number owned or observed per household. Instead, table B12 presents estimates for the IRS effect by looking at new ownership on the extensive margin. Each dependent variable is a dummy variable equal to 1 if the households owns at least one net and zero otherwise.

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[2] This information is available for all respondents, but not for all households heads.

[3] We computed a wealth index with Principal Component Analysis using information on household asset ownership.

Table B11: Heterogeneous treatment effects on net ownership

	Dependent variable: Number of observed nets used the night before				
	(1)	(2)	(3)	(4)	(5)
	OLS	OLS	OLS	OLS	OLS
Treatment	0.187**	0.189*	0.093	0.079	0.169*
	(0.083)	(0.105)	(0.073)	(0.089)	(0.101)
Treatment x literate	0.254*				
	(0.145)				
Literate	0.067				
	(0.131)				
Treatment x male household head		0.070			
		(0.126)			
Male household head		0.087			
		(0.088)			
Treatment x 2nd household size tercile			0.141		
			(0.122)		
Treatment x 3rd household size tercile			0.412**		
			(0.171)		
2nd household size tercile			0.000		
			(0.108)		
3rd household size tercile			-0.310		
			(0.191)		
Treatment x 2nd wealth tercile				0.218	
				(0.143)	
Treatment x 3rd wealth tercile				0.231	
				(0.157)	
2nd wealth quintile				-0.080	
				(0.105)	
3rd wealth quintile				0.098	
				(0.138)	
Treatment x tigre tribe					0.150
					(0.145)
Tigre	-0.013	-0.034	-0.032	-0.018	-0.100
	(0.102)	(0.101)	(0.102)	(0.106)	(0.112)
Observations	1493	1493	1493	1493	1493

Note: *** p<0.01, ** p<0.05, * p<0.1. One observation per household. The dependent variable is the number of observed nets used the night before. Columns (1)-(5) report the difference between treatment and control groups using OLS regression (model 2) and the coefficients on interactions between the treatment status and literacy status (column 1), gender of the household head (column 2), household size (column 3), asset ownership (column 4) and tribe (column 5). Standard errors clustered at village level are reported in parentheses. All specifications include controls for gender, age, education, household size, tribe and religion, information about access to water, dwelling characteristics, regional dummies and village characteristics (share of women living in the village and a dummy for pre-intervention high vegetation areas).

In all estimations where controls are included household size is added as regressor to control for potential unbalances. We check whether results differ when we look at per capita nets instead of total number of nets. We divide the total number of nets by the number of household members to the power of 0.6 to account for externalities in bed net use within the household. Table B13 presents estimates of treatment effect using model 2 and model 3 on the per-capita ownership of mosquito bed nets. We can observe that, similarly to analyzing the total number of nets, a significant effect is found for the number of nets owned (both for self-reported and observed data). The results are consistent to different assumptions about the effect of externalities of bed net use.

To look at heterogeneity of the effect in terms of number of nets owned, we look at whether the effect is driven by household owning fewer nets or already owning a larger number. In table B14, for each number of owned net *n*, the dependent variables are dummy variables equal to 1 if the household owns *n* nets or more and 0 otherwise. We observe that the effect on net ownership

Table B12: Ownership of mosquito bed nets, extensive margin

				$E(Y \mid T=1, X) - E(Y \mid T=0, X)$		
		(1) Control	(2) Treatment	(3) OLS	(4) OLS	(5) IV
1. Household owns any nets		0.793	0.832	0.039	0.055**	0.043
		[0.405]	[0.374]	(0.031)	(0.026)	(0.031)
	N =	763	762	1525	1525	1382
2. Household has any observed nets		0.789	0.828	0.039	0.055**	0.042
		[0.408]	[0.377]	(0.032)	(0.027)	(0.031)
	N =	748	745	1493	1493	1350
2a. Used the night before		0.541	0.603	0.061	0.059	0.061
		[0.499]	[0.490]	(0.041)	(0.036)	(0.050)
	N =	748	745	1493	1493	1350
2b. Unused the night before		0.349	0.325	-0.024	0.001	-0.012
		[0.477]	[0.469]	(0.033)	(0.029)	(0.041)
	N =	748	745	1493	1493	1350
3. Household has any observed ITNs		0.679	0.715	0.036	0.058*	0.050
		[0.467]	[0.452]	(0.035)	(0.031)	(0.040)
	N =	756	754	1510	1510	1368
3a. Used the night before		0.468	0.519	0.050	0.055	0.056
		[0.499]	[0.500]	(0.038)	(0.035)	(0.049)
	N =	756	754	1510	1510	1368
3b. Unused the night before		0.286	0.264	-0.022	0.000	-0.011
		[0.452]	[0.441]	(0.032)	(0.029)	(0.039)
	N =	756	754	1510	1510	1368
Controls		-	-	No	Yes	Yes

Note: We use one observation per household. Variables 2 and 3 are observed by the interviewer, while variable 1 is self-reported. "Nets" refers to any bed nets, irrespective of their treatment status, "ITNs" includes only LLINs and properly treated ITNs. Columns (1) and (2) report sample means in control and treatment groups, with standard deviations in brackets. Columns (3) and (4) report the difference between treatment and control groups estimated using OLS regression. Column (5) estimates the difference between households who report to have received the spraying campaign in the last 5 months and those who didn't by instrumenting program participation with the treatment group indicator. Controls include gender, age, education, household size, tribe and religion, information about access to water, dwelling characteristics, regional dummies and village characteristics (share of women living in the village and a dummy for pre-intervention high vegetation areas). Standard errors clustered at village level are reported in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

is driven by households with a larger number of nets. This support our finding in contrast with the null finding of Keating et al. (2011), which do not find any effect of IRS on owning at least one net.

Table B13: Per-capita ownership of mosquito bed nets

			$E(Y \mid T = 1, X) - E(Y \mid T = 0, X)$		
	(1)	(2)	(3)	(4)	(5)
	Control	Treatment	OLS	OLS	IV
1. Number of nets owned by household	0.634	0.705	0.071*	0.098***	0.110***
	[0.460]	[0.469]	(0.042)	(0.032)	(0.040)
N =	763	762	1525	1525	1382
2. Number of observed nets	0.612	0.675	0.063	0.093***	0.106***
	[0.443]	[0.435]	(0.040)	(0.029)	(0.038)
N =	748	745	1493	1493	1350
2a. Used the night before	0.373	0.449	0.076**	0.078**	0.095**
	[0.409]	[0.445]	(0.038)	(0.031)	(0.043)
N =	748	745	1493	1493	1350
2b. Unused the night before	0.239	0.226	-0.013	0.014	0.009
	[0.380]	[0.379]	(0.027)	(0.024)	(0.033)
N =	748	745	1493	1493	1350
3. Number of observed ITNs	0.492	0.553	0.061	0.095***	0.116***
	[0.440]	[0.448]	(0.039)	(0.032)	(0.044)
N =	756	754	1510	1510	1368
3a. Used the night before	0.304	0.372	0.068**	0.077**	0.094**
	[0.380]	[0.425]	(0.033)	(0.030)	(0.041)
N =	756	754	1510	1510	1368
3b. Unused the night before	0.188	0.181	-0.007	0.018	0.019
	[0.345]	[0.347]	(0.026)	(0.023)	(0.031)
N =	756	754	1510	1510	1368
Controls	-	-	No	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. One observation per household. Variables 2 and 3 are observed by the interviewer, while variable 1 is self-reported. Number of nets is normalized by the number of household members. "Nets" refers to any bed nets, irrespective of their treatment status, "ITNs" includes only LLINs and properly treated ITNs. Columns (1) and (2) report sample means restricted to control and treatment group, standard deviations in brackets. Columns (3) and (4) report the difference between treatment and control groups using OLS regression (model 2). Column (5) estimates the difference between households who report to have received the spraying campaign in the last 5 months and those who didn't by instrumenting program participation with the treatment group indicator (model 3). Standard errors clustered at village level are reported in parentheses. Controls include gender, age, education, household size, tribe and religion, information about access to water, dwelling characteristics, regional dummies and village characteristics (share of women living in the village and a dummy for pre-intervention high vegetation areas).

Table B14: Ownership of mosquito bed nets by number of nets

			$E(Y \vert T=1, X) - E(Y \vert T=0, X)$		
	(1)	(2)	(3)	(4)	(5)
	Control	Treatment	OLS	OLS	IV
1. Nets owned by household					
None	0.207	0.168	-0.039	-0.055**	-0.043
	[0.405]	[0.374]	(0.031)	(0.026)	(0.031)
1 net or more	0.793	0.832	0.039	0.055**	0.043
	[0.405]	[0.374]	(0.031)	(0.026)	(0.031)
2 nets or more	0.503	0.573	0.070*	0.082***	0.101**
	[0.500]	[0.495]	(0.040)	(0.030)	(0.040)
3 nets or more	0.191	0.252	0.061*	0.060**	0.076**
	[0.394]	[0.434]	(0.034)	(0.027)	(0.037)
4 nets or more	0.067	0.110	0.043**	0.044***	0.054**
	[0.250]	[0.313]	(0.020)	(0.016)	(0.022)
2. Nets used the night before					
None	0.459	0.397	-0.061	-0.059	-0.061
	[0.499]	[0.490]	(0.041)	(0.036)	(0.050)
1 net or more	0.541	0.603	0.061	0.059	0.061
	[0.499]	[0.490]	(0.041)	(0.036)	(0.050)
2 nets or more	0.263	0.344	0.080**	0.080***	0.112***
	[0.441]	[0.475]	(0.037)	(0.029)	(0.041)
3 nets or more	0.080	0.149	0.069***	0.060***	0.080***
	[0.272)	[0.356)	(0.024)	(0.020)	(0.027)
4 nets or more	0.029	0.070	0.040***	0.038***	0.050***
	[0.169]	[0.255]	(0.015)	(0.013)	(0.018)
3. ITNs owned by household					
None	0.321	0.285	-0.036	-0.058*	-0.050
	[0.467]	[0.452]	(0.035)	(0.031)	(0.040)
1 net or more	0.679	0.715	0.036	0.058*	0.050
	[0.467]	[0.452]	(0.035)	(0.031)	(0.040)
2 nets or more	0.366	0.444	0.078**	0.101***	0.136***
	[0.482]	[0.497]	(0.037)	(0.031)	(0.043)
3 nets or more	0.126	0.182	0.056**	0.060**	0.081***
	[0.332]	[0.386]	(0.027)	(0.023)	(0.031)
4 nets or more	0.046	0.070	0.024	0.025*	0.031*
	[0.210]	[0.256]	(0.015)	(0.013)	(0.018)
4. ITNs used the night before					
None	0.532	0.481	-0.050	-0.055	-0.056
	[0.499]	[0.500]	(0.038)	(0.035)	(0.049)
1 net or more	0.468	0.519	0.050	0.055	0.056
	[0.499]	[0.500]	(0.038)	(0.035)	(0.049)
2 nets or more	0.205	0.280	0.075**	0.084***	0.119***
	[0.404]	[0.449]	(0.032)	(0.029)	(0.039)
3 nets or more	0.057	0.121	0.064***	0.059***	0.078***
	[0.232]	[0.326]	(0.020)	(0.018)	(0.023)
4 nets or more	0.022	0.046	0.024**	0.023**	0.028**
	[0.148]	[0.211]	(0.011)	(0.010)	(0.013)
Controls	-	-	No	Yes	Yes

Note: we use one observation per household. Variables 2-5 are observed by the interviewer, while variable 1 is self-reported. “Nets” refers to any bed nets, irrespective of their treatment status, “ITNs” includes only LLINs and properly treated ITNs. Columns (1) and (2) report sample means in control and treatment groups, with standard deviations in brackets. Columns (3) and (4) report the difference between treatment and control groups estimated using OLS regression. Column (5) estimates the difference between households who report to have received the spraying campaign in the last 5 months and those who didn’t by instrumenting program participation with the treatment group indicator. Controls include gender, age, education, household size, tribe and religion, information about access to water, dwelling characteristics, regional dummies and village characteristics (share of women living in the village and a dummy for pre-intervention high vegetation areas). Standard errors clustered at village level are reported in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

C. Data

The following table presents a detailed description of the variables used in the paper.

Variable	**Level**	**Description**
Female	Individual	Indicator variable equal to 1 if person is a female, and zero otherwise.
Usually lives here	Individual	Indicator variable equal to 1 if person reportedly normally lives in the dwelling where the interview was conducted and zero otherwise.
Stayed here last night	Individual	Indicator variable equal to 1 if person reportedly spent the night before the interview in the dwelling where the interview was conducted and zero otherwise.
Age	Individual	Age in years of the person, zero if less than 1 year old.
Ever attended school	Respondent	Indicator variable equal to 1 if respondent reportedly ever attended school, and zero otherwise.
Only primary school	Respondent	Indicator variable equal to 1 if respondent reportedly has some schooling but did not progress to secondary school; zero 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.
Literate	Respondent	Indicator variable equal to 1 if respondent reportedly can read and write in one language without any difficulty, and zero otherwise.
Muslim	Respondent	Indicator variable equal to 1 if respondent is Muslim, and zero otherwise.
Tigre	Respondent	Indicator variable equal to 1 if respondent belongs to Tigre tribe, and zero otherwise.
Married	Respondent	Indicator variable equal to 1 if respondent is married, and zero otherwise.
Household size	Household	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.
Household members under 5	Household	Number of household members whose age was not greater than 5 years.
Household members under 18	Household	Number of household members whose age was not greater than 18 years.
Public tap	Household	Indicator variable equal to 1 if the main source of drinking water of the household was a public tap, and zero otherwise.
Unprotected well	Household	Indicator variable equal to 1 if the main source of drinking water of the household was an unprotected well, and zero otherwise.
Unprotected spring	Household	Indicator variable equal to 1 if the main source of drinking water of the household was an unprotected spring, and zero otherwise.
Any toilet	Household	Indicator variable equal to 1 if dwelling has a toilet, and zero otherwise.
Radio	Household	Indicator variable equal to 1 if household owns a radio, and zero otherwise.
Firewood is main fuel	Household	Indicator variable equal to 1 if firewood is the main fuel used by the household for cooking, and zero otherwise.
No window	Household	Indicator variable equal to 1 if dwelling has no windows and zero otherwise.
Number of separate rooms	Household	Number of separate rooms that compose the dwelling.
Number of sleeping rooms	Household	Number of separate rooms used for sleeping in the dwelling.
Number of sleeping spaces	Household	Number of sleeping spaces available inside the dwelling.
Dwelling was sprayed in past 5 months	Household	Indicator variable equal to 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; zero if dwelling was reportedly not sprayed or if dwelling was reportedly sprayed beyond the 5 months prior to the survey. Don’t know is recoded as missing.
Mosquitoes mentioned among malaria vectors	Respondent	Indicator variable equal to 1 if respondent mentioned mosquitoes answering the question ”How does one get malaria?” and zero otherwise.
Malaria is a problem in community	Respondent	Indicator variable equal to 1 if respondent answered yes to the question ”Is malaria a problem in this community?” and zero otherwise. Don’t know was recoded to missing.
Children mentioned among most affected by malaria	Respondent	Indicator variable equal to 1 if respondent answered children or children and pregnant women to the question ”Who is most affected by malaria?” and zero otherwise.
Pregnant women mentioned among most affected	Respondent	Indicator variable equal to 1 if respondent answered pregnant women or children and pregnant women to the question ”Who is most affected by malaria?” and zero otherwise.

Heard/saw messages about ITNs	Respondent	Indicator variable equal to 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 zero otherwise.
Heard/saw messages about early seeking behaviour	Respondent	Indicator variable equal to 1 if respondent answered yes to the question ”During the last six months, have you heard or seen any messages about early seeking behaviour for malaria treatment?”, and zero otherwise.
Heard/saw messages about environmental management	Respondent	Indicator variable equal to 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 zero otherwise.
Number of nets owned by household	Household	Number of bed nets reportedly owned by household, including 0 if household had none.
Number of ITNs owned by household	Household	Number of ITNs owned by household, including 0 if household had none.
Reported net use	Individual	Indicator variable equal to 1 if person reportedly slept under a bed net the night before the survey and zero otherwise.
Number of observed nets used the night before	Household	Number of bed nets observed during survey and reportedly used the night before the survey by at least one household member.
Number of observed nets left unused the night before	Household	Difference between the total number of nets observed during the survey and the number of observed nets used the night before.
Respondent participated in LHM	Respondent	Indicator variable equal to 1 if respondent answered yes to the question ”In the past six months, have you participated in environmental management in the village?” and zero otherwise.
Days spent by household in LHM	Household	Number of days spent during the last month in LHM activities.
Household members who participated in LHM	Household	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.
Male household members who participated in LHM	Household	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.
Female household members who participated in LHM	Household	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.
Young Household members who participated in LHM	Household	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.
Household keeps livestock 100m from home	Household	Indicator variable equal to 1 if respondent answered no to the question Are these animals kept 100 meters or less from your house? and zero 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.?).
Household covers stored water	Household	an indicator variable = 1 if respondent answered yes to the question Is the stored water covered?, and zero 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?.
Respondent does anything to prevent mosquito bites	Respondent	Indicator variable equal to 1 if respondent answered yes to the question Do you do things to stop mosquitoes from biting you?, and zero otherwise.

D. Randomization procedures

D.1. Village lists and treatment allocation

We can identify four village lists that were used during the RCT conducted in Eritrea:

1. An initial village list, provided by the NMCP of Eritrea to the research team to conduct the initial random allocation to treatment (2008);
2. A village list provided by the NMCP to the spraying teams that conducted the IRS campaign in Gash Barka (Gash Barka) in June-July 2009. This list includes only the names of treatment villages. The names of control villages were added by hand and was probably done by NMCP staff in Gash Barka;
3. A village list provided by the NMCP to data collectors (October 2009), including both treatment and control villages;
4. A final village list, provided by the NMCP to The World Bank at the end of all field operations (November 2009).

Comparison between List 1 and List 4 reveals some differences. Out of 116 villages, 82 (71%) have the same name in both lists and another 10 (9%) villages have names that can be matched using additional information. Two villages were replaced with two additional ones in one subzone. We are left with 22 (19%) cases of mismatch that we can't explain. Treatment allocation was altered in 5 instances and we explain possible reasons underlying these changes. 87 (95%) of the 92 villages that we can match from List 1 to List 4 have the correct treatment allocations. 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, can be matched using the sub-zone where it is located. 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.

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

1. The initial list was outdated, possibly from the Census of 2002 or 2003. For example, a sub-zone 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 switched from a sub-zone to another (e.g., Hawashait moved from sub-zone Dighe to sub-zone Laelay Gash) and some became part of another country (Sudan or Ethiopia). Existence and location of treatment and control villages were not checked or recorded prior to the beginning of the study. Villages may also have changed name or 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.

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 and missing treatment villages were replaced with the closest available village.
3. The minimum distance between villages had to be >5km. After randomisation some villages were found to be adjacent and were replaced to ensure the minimum distance would be kept. In addition, some treatment and control villages are located in the highlands, where there is no malaria thanks to altitude. Two such instances in sub-zone 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 Gash Barka. We check if preference was given to the Tigre tribe, which is over-represented in the treatment group (the new treatment village is number 43 and the new control is number 46). No Tigre households resides in either village, suggesting that no active effort was put to offer treatment to Tigre villages. 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.
4. New issues arose when enumerators went to the field to conduct the survey. Issues occurred when data collectors could not find some of the control villages. Missing control villages were replaced with the nearest available village. We compare List 3 to List 4 and the problem concerns the following villages: 3 controls in sub-zone Goluj (villages 4, 5, 7); 1 control in sub-zone Tesseney (52), and 2 controls in sub-zone Shambko (93, 95). We analyse the determinants of such changes in Table D16. 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 sub-zone. Notice that we are comparing replacement control villages to all (treatment and control) villages surveyed in the same sub-zone, and treatment villages may have become wealthier following the IRS intervention.

D.1.1. Change in number of villages in each sub-zone

The number of villages by sub-zone was different from List 1 to List 4, as shown in Table D17. This can be explained by the fact that, in recent years, the boundaries of certain sub-zones were changed, so that some villages were allocated to a new adjacent sub-zone. The number of treatment villages was finalised when List 2 was drafted for the spraying teams. The total was reduced from 58 to 57. Although, in 6 of the 13 surveyed sub-zones, the number of treatment villages was left unchanged. Column 5 of Table D17 shows that the largest disparities with respect to List 1 appear in sub-zone Haykota (where 3 extra villages were treated) and in sub-zone Mensura (where 3 villages less were treated). In the other sub-zones, the number of treated villages differs from the original figure by at most 1. The number of treatment villages, both in total and by sub-zone, was not changed in the subsequent lists. The number of control villages was left unchanged at 58,

Table D16: Choice of replacement control villages

Dependent variable:		**Tigre**			**Wealth**	
Sample restricted to sub-zone	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 Columns (4)-(6) Y_i is an asset index for household i. Samples restricted to the sub-zones where listed villages are located, shown in each header. Notice that no Tigre households were surveyed in sub-zone Shambko. Observations clustered at village level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

from List 1 through List 4. However, column 10 of Table D17 shows that the allocation of control villages across sub-zones changed significantly: in the case of sub-zone Akurdet, it was increased by 3, while it was decreased by 3 in sub-zone Haykota. In sub-zone Haykota the problem is severe for both treatment villages (+3) and control villages (-3). The problem is less severe in the other sub-zones, in 5 of which the number of controls was left untouched.

Table D17: Number of villages in Lists 1, 2 and 4

	List 1			**List 2**		**List 4**						
Sub-sample	Total (1)	T (2)	C (3)	T (4)	D1 (5)	Total (6)	D1 (7)	T (8)	D1 (9)	D2 (10)	C (11)	D1 (12)
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. T and C stand for Treatment and Control. For List 1, this Table reports in columns 1-3 the number of villages for each sub-zone, 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 sub-zone 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 sub-zone: (11) = (6) - (8). Column 12 reports the difference between the previous column and the corresponding column for List 1: (12) = (11) - (3).

D.1.2. Altered village names

We investigate the characteristics of altered village names and how these might have affected selection into treatment. In table D18 we investigate the presence of any systematic differences between villages whose names were not changed during the operations of the RCT and those villages which instead were changed. We compare villages with altered name or treatment allocation, to all other villages in Gash Barka. Column 1 is analogous to the randomisation checks presented in the paper, while in Column 2 we check if villages with the same name in Lists 1 and 4 differ systematically from those which were changed. We repeat the same analysis in Column 3, where we broaden the definition of *unchanged villages* to include also those villages whose names we were able to match with the original list with the help of information on multiple village names. We find no evidence of systematic differences between villages whose names were the same in List 1 and 4 and villages whose names were different. We find no evidence of any discrimination on grounds of ethnicity or wealth. We only find a significant small age difference between unchanged and replaced villages, but we do not interpret this as a sign of age-based discrimination.

Tables D19-D22 replicate the analysis of homogeneous treatment effects conducted in the main text, checking the effect on the parameter of interest by adding a dummy variable equal to 1 if the name of the village was left unchanged and 0 otherwise. Estimates do not change appreciably in terms of magnitude and statistical significance.

D.1.3. Reallocation of treatment status

The treatment allocation of 5 villages was altered from the original list. We compare List 2 to List 1 to see which control villages were reallocated from control to treatment group. In sub-zone Haykota, this happened for 2 villages, i.e. Biet Hama (56) and Akyeb (59). In sub-zone Laelay Gash, this possibly happened for one village, Amir/Uguma (19), since names do not match perfectly. We cannot identify any other instance in which this problem occurred. We compare List 3 to List 1 to see which treatment villages were reallocated from treatment to the control group. In sub-zone Dighe, one village was re-allocated to serve as control, i.e. Aflanda (72). In sub-zone Forto, the same happened to one village, i.e. Grgr (16). In fact, no household was reportedly sprayed in Grgr and only one was in Aflanda.

We investigate the possibility that preference for treatment was given to villages with better infrastructure or other specific characteristics. In table D23 we investigate the presence of any systematic differences between these villages and those whose treatment allocation was left unchanged. Column 1 reports, for each variable, the estimated difference between villages whose treatment allocation was changed to the ones whose treatment allocation was not changed. We conduct the same randomisation checks used to compare treatment and control villages, but this time to compare villages with altered treatment status to those with unaltered treatment status. Columns 2 and 3 report the same difference, but restricting the sample to the treatment group and the control group respectively. 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

Table D18: Which villages were replaced? - Individual Variables

Sub-sample:	Treatment status (1)	Same name (2)	Matched name (3)
All household members			
1. Female	-0.004	-0.007	-0.006
	(0.011)	(0.012)	(0.014)
2. Usually lives here	0.006	-0.002	-0.003
	(0.005)	(0.006)	(0.007)
3. Stayed here last night	0.014	-0.010	-0.005
	(0.009)	(0.009)	(0.012)
4. Age	0.346	1.414***	1.326**
	(0.492)	(0.487)	(0.556)
Respondents only			
5. Age	0.616	1.834*	1.524
	(0.893)	(0.983)	(0.146)
6. Ever attended school	0.007	-0.024	-0.078*
	(0.034)	(0.037)	(0.043)
7. Only primary school	-0.037	0.051	0.057
	(0.053)	(0.054)	(0.057)
8. Literate	-0.015	-0.029	-0.091**
	(0.032)	(0.037)	(0.042)
9. Muslim religion	0.060	0.064	0.144
	(0.068)	(0.078)	(0.096)
10. Tigre tribe	0.167*	0.039	0.142
	(0.084)	(0.095)	(0.106)
11. Married	-0.013	-0.014	-0.006
	(0.013)	(0.014)	(0.016)
Household level			
12. Household size	0.184	-0.163	-0.138
	(0.156)	(0.162)	(0.173)
13. Household members under 5	0.021	-0.071	0.005
	(0.057)	(0.059)	(0.066)
14. Household members under 18	0.093	-0.184	-0.177
	(0.128)	(0.128)	(0.136)
15. Access to public tap	-0.010	-0.052	-0.146
	(0.077)	(0.089)	(0.102)
16. Access to unprotected well	0.020	0.004	0.043
	(0.055)	(0.057)	(0.061)
17. Access to unprotected spring	-0.015	0.036	0.065
	(0.038)	(0.039)	(0.042)
18. Has any toilet	-0.011	-0.009	0.010
	(0.023)	(0.027)	(0.030)
19. Has radio	0.008	-0.008	-0.007
	(0.032)	(0.035)	(0.042)
20. Firewood is main fuel	-0.021	-0.018	-0.032*
	(0.019)	(0.018)	(0.018)
21. Has no window	0.005	-0.037	-0.062
	(0.066)	(0.071)	(0.077)
22. Number of separate rooms	0.023	-0.143	-0.139
	(0.105)	(0.112)	(0.122)
23. Number of sleeping rooms	0.002	-0.024	-0.027
	(0.051)	(0.052)	(0.053)
24. Number of sleeping spaces	-0.164	-0.058	-0.279
	(0.190)	(0.205)	(0.217)

Note: 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 randomisation 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.

group. In Column 2 we compare villages that were originally allocated to treatment group with the villages that were originally in the control group, but were in fact allocated to treatment. Similarly,

Table D19: 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 among malaria vectors	0.908	0.854	0.054**	0.031*	0.027*
	[0.289]	[0.353]	(0.021)	(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]	(0.044)	(0.035)	(0.035)
3. Children among most affected by malaria	0.863	0.788	0.074***	0.068***	0.069***
	[0.344]	[0.409]	(0.025)	(0.019)	(0.019)
4. Pregnant women among most affected	0.367	0.365	0.002	-0.014	-0.015
	[0.482]	[0.482]	(0.040)	(0.024)	(0.024)
5. In the previous 6 months, heard about:					
5a. ITNs	0.484	0.469	0.015	-0.001	0.005
	[0.500]	[0.499]	(0.042)	(0.038)	(0.039)
5b. Early seeking behaviour	0.537	0.501	0.037	0.019	0.025
	[0.499]	[0.500]	(0.042)	(0.040)	(0.040)
5c. Environmental management	0.450	0.387	0.064	0.029	0.033
	[0.498]	[0.487]	(0.043)	(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 brackets. 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 sub-zone 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 D20: 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. Nets owned by household	1.774	1.575	0.200*	0.214**	0.216**
	[1.279]	[1.207]	(0.110)	(0.010)	(0.099)
2. ITNs owned by household	1.444	1.278	0.166*	0.176*	0.180*
	[1.206]	[1.126]	(0.096)	(0.093)	(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. Observed nets used the night before	1.384	1.164	0.220**	0.186**	0.187**
	[1.214]	[1.054]	(0.099)	(0.088)	(0.086)
5. Observed nets left unused the night before	0.676	0.736	-0.060	0.015	0.025
	[0.993]	[1.001]	(0.076)	(0.063)	(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 brackets. 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 sub-zone 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.

in Column 3 we compare villages that were originally allocated to control group with the villages that were originally in the treatment group but were mistakenly allocated to control group. We would be particularly worried of 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. 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.

Table D21: Robustness checks: Participation in Larval Habitat Management (LHM)

Variables	Treatment	Control	$E(Y \mid T=1, X) - E(Y \mid T=0, X)$		
			No Regressors	Basic Regressors	Same Name
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)
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. Participated in LHM in the last month:					
3a. Household members	0.456	0.39	0.066	0.051	0.035
	[1.007]	[0.898]	(0.077)	(0.071)	(0.068)
3b. Male >15 y.o.	0.167	0.125	0.042	0.025	0.021
	[0.462]	[0.399]	(0.031)	(0.027)	(0.027)
3c. Female >15 y.o.	0.215	0.219	-0.004	-0.001	-0.004
	[0.470]	[0.483]	(0.038)	(0.034)	(0.034)
3d. Household members <15 y.o.	0.075	0.046	0.029	0.027	0.018
	[0.467]	[0.372]	(0.025)	(0.026)	(0.023)

Note: variable 1 refers to 6 months previous to the interview, while variable 2 refers to one month. Columns 1 and 2 report means for treatment and control groups, with standard deviations in brackets. 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 sub-zone 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 D22: Robustness checks: Behaviours conducive to malaria elimination, other than LHM

Variables	Treatment	Control	$E(Y \mid T=1, X) - E(Y \mid T=0, X)$		
			No Regressors	Basic Regressors	Same Name
1. HH 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. HH 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 prevents 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:					
4a. Using net	0.680	0.649	0.029	0.011	0.005
	[0.467]	[0.478]	(0.039)	(0.029)	(0.028)
4b. Burning coils	0.225	0.211	0.015	0.003	0.004
	[0.418]	[0.409]	(0.035)	(0.022)	(0.021)
4c. Using spray	0.025	0.021	0.004	0.010	0.011
	[0.156]	[0.143]	(0.009)	(0.008)	(0.008)
7. Burning animal dung	0.058	0.046	0.012	0.005	0.005
	[0.234]	[0.209]	(0.014)	(0.012)	(0.012)
4d. Burning herbs	0.048	0.054	-0.006	-0.017	-0.018
	[0.215]	[0.226]	(0.018)	(0.014)	(0.014)
4e. 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 brackets. 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 sub-zone 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 D23: Which villages were reallocated across treatments? - Individual Variables

Sub-sample:	All villages (1)	Treatment group (2)	Control group (3)
All household members			
1. Female	0.016	0.020	0.010
	(0.039)	(0.058)	(0.033)
2. Usually lives here	0.015***	0.008	0.025***
	(0.005)	(0.006)	(0.004)
3. Stayed here last night	0.008	0.017***	-0.014
	(0.011)	(0.004)	(0.011)
4. Age	4.142***	3.368***	5.381***
	(0.462)	(0.396)	(0.398)
Respondents only			
5. Age	0.166	2.545	-3.407
	(2.655)	(1.859)	(4.948)
6. Ever attended school	-0.137***	-0.126***	-0.156***
	(0.029)	(0.041)	(0.035)
7. Only primary school	0.240***	0.260***	0.219***
	(0.026)	(0.036)	(0.040)
8. Literate	-0.121***	-0.139***	-0.092
	(0.043)	(0.045)	(0.080)
9. Muslim religion	0.200***	0.170***	0.229***
	(0.035)	(0.047)	(0.053)
10. Tigre tribe	0.039	0.301**	-0.379***
	(0.196)	(0.130)	(0.068)
11. Married	-0.083***	-0.053**	-0.123***
	(0.021)	(0.021)	(0.027)
Household level			
12. Household size	-0.834***	-0.593**	-1.229***
	(0.290)	(0.230)	(0.448)
13. Household members under 5	-0.145	-0.043	-0.299**
	(0.095)	(0.085)	(0.134)
14. Household members under 18	-0.810***	-0.574***	-1.175***
	(0.202)	(0.151)	(0.246)
15. Access to public tap	0.190	0.121	0.292**
	(0.152)	(0.235)	(0.117)
16. Access to unprotected well	-0.203***	-0.184**	-0.236***
	(0.048)	(0.070)	(0.040)
17. Access to unprotected spring	-0.032	0.048	-0.145***
	(0.067)	(0.093)	(0.029)
18. Has any toilet	-0.033	-0.006	-0.068***
	(0.028)	(0.041)	(0.019)
19. Has radio	-0.108*	-0.009	-0.253***
	(0.061)	(0.043)	(0.024)
20. Firewood is main fuel	0.011	-0.010	0.045***
	(0.042)	(0.067)	(0.012)
21. Has no window	0.426***	0.313	0.5853***
	(0.126)	(0.189)	(0.050)
22. Number of separate rooms	-0.518***	-0.567***	-0.456***
	(0.088)	(0.105)	(0.151)
23. Number of sleeping rooms	-0.277***	-0.300***	-0.246***
	(0.047)	(0.063)	(0.066)
24. Number of sleeping spaces	-1.140***	-0.905	-1.444***
	(0.410)	(0.661)	(0.181)

Note: 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.