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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.

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

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

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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 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 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), Romano et al. (2008), 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

[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.

[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% 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.

[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).

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. (2014) 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, 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% 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 1500 m 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]

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%, 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).

[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.

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%, 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 pre-determined, 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 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:

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

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.

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.

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% of respondents ever attended school and 76% of them attended only primary school. The proportion of literate respondents is equally low (20%). 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% of them have access to drinking water from a public tap, 6% have a toilet, 25% own a radio, 95% 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% 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.

[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.

[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 4weeks per year on average. This threshold is based on the findings of Gaudart et al. (2009). See Appendix B.2 for detailed information.

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).

Table 3
Program compliance (self-reported).

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

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 of these poor households.[12] Also, 25% 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.

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 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

[12] NMCP records report that no IRS campaign was conducted in control villages over the 12months 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).

[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.

Table 4
Ownership of mosquito bed nets.

				$E(Y \vert T=1, X) - E(Y \vert T=0, X)$		
		(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).

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), Romano et al. (2008), 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 Instrumental 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:

$$Y_{ij} = \alpha + \beta\ Spray5m_i + X_i^{'}\gamma + V_j^{'}\delta + \epsilon_{ij} \tag{3}$$

$$\Pr(Spray5m_i = 1 \vert T_j, X_i, V_j) = \Phi(\theta_1 + \theta_2 T_j + X_i^{'}\theta_3 + V_j^{'}\theta_4 + v_{ij}) \tag{4}$$

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

[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.

Table 5
Information and knowledge about malaria.

				$E(Y \mid T=1, X) - E(Y \mid T=0, X)$		
		(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).

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).

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% 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 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 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

[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).

[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 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.

organize their households into shifts when it comes to LHM activities. In fact, LHM is more a programmatic 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.

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.

Appendix A. Supplementary data

Supplementary data associated with this article can be found in the online version at http://dx.doi.org/10.1016/j.labeco.2016.11.003.

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