Micro-loans, bednets and malaria: Evidence from a randomized controlled trial in Orissa (India)

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Abstract

Numerous studies have shown that widespread, regular use of insecticide-treated bednets (ITNs) can successfully reduce the malaria burden. However, in the absence of free provision or heavy subsidization, ITN take-up remains very low in most poor malarious areas. We describe the result of a large-scale cluster randomized controlled trial among micro-finance clients in rural Orissa (India), where we tested the hypothesis that small consumer loans can be used to increase ownership rates of ITNs and consequently to reduce malaria prevalence. Despite the absence of any subsidy, 53 percent of sample households purchased at least one ITN, leading to 16 percent of individuals using a treated nets the previous night, relative to only 2 percent in control areas where nets were not offered for sale. Still, the increase fell significantly short of what achieved in a third group of villages where nets were distributed free of cost, and where we recorded a 47 percent previous-night usage rate. In addition, we find that neither micro-loans nor free distribution led to improvements in malaria and anemia prevalence, measured using blood tests. We examine and rule out several plausible explanations for this latter finding. We conjecture that insufficient ITN coverage levels are the most likely explanation and discuss implications for public health policy.

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1 Introduction

Transmitted by *Anopheles* mosquitoes, malaria represents an enormous burden on global health, with a worldwide incidence of 300-660 million cases every year, of which 80 million cases in India alone (Snow et al. 2005, Korenromp 2005). According to recent estimates, one third of the human population lives in areas exposed to the most severe form of malaria, caused by *Plasmodium falciparum* (Snow et al. 2005). Malaria infection may develop into debilitating febrile episodes and lead to severe health risks, including severe anemia, respiratory problems, hypoglycemia, pregnancy complications, low birth weight, permanent neurologic and developmental impairment for children, and death. Mortality rates are particularly high among young children and pregnant women (Breman 2001). The negative consequences of the disease for growth and for the accumulation of human capital have been long recognized (Sachs and Malaney 2002), although quantifying such causal impacts is difficult, and studies that convincingly document the link are relatively recent within economics.[1]

Numerous studies have shown that high coverage and use rates of insecticide-treated bednets (ITNs) are efficacious at reducing malaria-related morbidity and mortality, as documented in the extensive survey in Lengeler (2004). However, ITN adoption in most malarious areas remains very low and public health interventions frequently have insufficient resources to provide complete ITN coverage for all individuals at risk. The introduction of cost sharing may alleviate budgetary concerns, but may also lead to the exclusion of vulnerable individuals who do not have access to sufficient funds. For instance, Cohen and Dupas (2010) find that demand for ITNs sold through prenatal clinics in rural Kenya dropped dramatically when small positive prices were introduced. This paper describes the findings of a large randomized controlled trial (RCT) in highly malarious areas of rural Orissa (India) whose broad objective was to evaluate the effectiveness of relatively small consumer micro-loans, relative to free distribution or control areas, at increasing ITN ownership and use, and ultimately at reducing the burden of malaria within the study population.

The project was completed in collaboration with BISWA (Bharat Integrated Social Welfare Agency), a rural micro-lending organization with a large presence in rural Orissa. We randomly selected 141 villages from a list of communities with BISWA presence in a broad area covering the five districts of Bargarh, Bolangir, Keonjhar, Kandhamal, and Sambalpur. After a baseline household survey completed in spring 2007, we assigned communities to one of three equally sized groups. A control group received no further interventions, while free nets were distributed to lender clients in a second group ("Free" villages). Clients

[1] Macro-economic studies have determined the existence of a strong negative correlation between malaria incidence and GDP (Gallup and Sachs 2001, McCarthy et al. 2000, Malaney et al. 2004), but it is difficult to infer causality in this context. Microeconomic studies have proposed varying approaches to cost-evaluation (see Chima et al. 2003 for a review on the direct economic impact of malaria in Africa). Shepard et al. (1991)'s estimates of the cost of malaria in four different African settings consist of the sum of medical costs and foregone income, while Cropper et al. (2004) relies on willingness to pay. Bleakley (2010) uses census data from different American states to estimate the long-term consequences of childhood exposure to malaria infection on adult labor productivity. Cutler et al. 2010 examine the effects of malaria on schooling by exploiting geographic variation in malaria prevalence in India prior to a nationwide eradication program in the 1950s. A similar identification strategy is used in Lucas (2010). Barreca (2010), Hong (2007a) and Hong (2007b) use historical data to evaluate the long term impact on schooling and economic outcomes in the United States.

from the third group of villages (Microfinance or "MF" villages) were offered the choice between different contracts for the purchase of ITNs and retreatments, using consumer loans with a one-year repayment period. Buyers could decide between purchasing a treated net or a bundle which also included, in addition, two re-treatments with insecticide. After six and twelve months, field workers went back to all communities except control areas. At this time re-treatment was offered at no cost both in Free villages and to buyers who choose the bundle in MF communities. Buyers who choose to purchase the net only were could re-treat the nets for cash.

The project has several specific aims. First, to test the hypothesis that the offer of small loans for the purchase of ITNs can lead to substantial increases in ITN ownership, even among very poor households. To the best of our knowledge, this is the first large-scale cluster RCT that evaluates the efficacy of a public health program where a health-protecting technology is provided at full cost but allows for repayment over time, as compared to both control conditions or free distribution. Second, we evaluate the impacts of the intervention on health outcomes accurately measured through blood tests. The study included measures of both malaria and anemia prevalence, where the latter is measured by the fraction of individuals with hemoglobin levels below 11 g/dl of blood. The use of blood tests is important, both because ITN ownership and usage are not the true object of interest and because self-reported health outcomes commonly suffer from non-random measurement error (Strauss and Thomas 1998). Third, we analyze (although non-experimentally) whether retreatment rates for bednets can be increased by using particular micro-loan contracts designed to provide appropriate incentives to buyers. Although treatment of nets with insecticide is relatively safe, efficacious and inexpensive, regular retreatment is rare even among bednet users. More generally, sustained compliance with health-protecting behavior is often problematic in public health initiatives, especially when it involves a monetary cost (Kremer and Miguel 2007, Holla and Kremer 2009). Researchers have argued that commitment devices can help poor households to overcome time-inconsistency in their preferences (Ashraf et al. 2006, Duflo et al. 2009). However, while analogous arguments have been often used to study behavior detrimental to health such as addiction, we are not aware of studies that analyze the relationship between commitment devices and health-seeking behavior in a developing country. A fourth objective is to revisit the argument that cost-sharing in public health programs induces more use of the services provided. One view among development experts is that cost sharing should be enforced in public health projects, both to improve targeting (by screening out those with no demand) and to increase usage conditional on take up. One possible reason for the latter is the "sunk cost fallacy", whereby individuals will use a product more because they paid a price for it, thereby letting behavior being driven by "sunk" factors that should be irrelevant (Thaler 1980, Arkes and Blumer 1985). Higher prices may also induce more usage if they are seen as signaling higher quality (Riley 2001). Cohen and Dupas (2010) use data from a RCT to show that, conditional on willingness-to-pay, women in their sample who pay higher prices do not use ITNs more relative to others who received the nets for free. In an earlier RCT, Ashraf et al. (2008) find similarly no evidence of sunk cost effects in the usage of a water purification product in Zambia, although they also find that higher prices screen out individuals who are less likely to use the product.

We find that the interventions successfully increased ITN ownership and (self-reported) use, although

neither increased as much as with free provision. While 53% of BISWA households purchased at least one net in MF villages, mean take up, at 0.24 nets per person, was significantly lower than in areas where nets were distributed for free (0.52 ITNs per head). In the latter areas, total net ownership measured at follow-up was about 1.5 persons per net, a number which often ensures protection for all household members. In MF villages, the ratio was about 2 persons per net, while in control areas it was close to 3 persons per net. Usage rates are consistent with the finding on take up. At follow-up, in villages with Free distribution 47% of individuals were reported as having slept under an ITN the previous night, while the fraction was only 16% in MF villages and 2% in control areas. Usage rates reported to be "usual" during the peak mosquito season in the three experimental arms were respectively 77, 36 and 7%. In MF villages, we also find that re-treatment rates were significantly higher when nets where purchased bundled with a sequence of two-retreatments, even after controlling for a series of household characteristics predictive of contract choice. This is consistent with the implicit inclusion of a "commitment" to re-treat in this contract, although the non-experimental nature of the result does not allow us to interpret the results as conclusively causal. In addition, we find no evidence in favor of the hypothesis that measures of self-reported net usage increased with the price paid, conditional on ownership (indeed, we rather find evidence of the opposite).

The most surprising findings of our study, however, come from the evaluation of the health impacts. We find that the increased usage rates did not translate into health improvements, not even in villages where a large number of ITNs were delivered for free to all BISWA households. At standard significance levels, we cannot reject the null hypothesis of equal malaria prevalence across experimental arms in the post-intervention survey. Hemoglobin levels barely differ across treatment areas, and we can only document a small improvement (11% of a standard deviation) in Free villages, significant only at the 10% level. In addition, the lack of improvements is largely shared by all demographic groups. Our data allow us to rule a number of potentially plausible explanations for the lack of health benefits, including perverse behavioral responses among beneficiaries and measurement error in health outcomes. Ultimately, we conjecture that the most plausible interpretation of our results rests on a comparison between our unique study design (which involved overall low ITN coverage rates and no monitoring of ITN usage) and the earlier literature on the impact of ITNs on health outcomes (which mostly evaluates programs under high coverage rates and close monitoring of health and usage). In this sense, our results should *not* be interpreted as contradicting these earlier seminal studies, whose findings are compelling. Rather, our results complement this literature, and suggest that ITN distribution programs that do not ensure sufficient coverage and regular usage may hardly dent the malaria burden. This interpretation has potentially important implications for the many public health programs of ITN distribution that do not guarantee sufficiently high coverage, and that may therefore fail to provide substantial health benefits to the intended beneficiaries.

The rest of the paper is organized as follows. Section 2 describes in detail the study area, the RCT design as well as the nature of the data recorded. This section also includes baseline summary statistics, an assessment of the success of the randomization and of the extent of attrition between baseline and follow-up survey. Section 3 describes the estimated impacts of the interventions on ITN take-up, (self-reported) usage, re-treatment rates and finally on health outcomes. Section 4 considers several potentially plausible hypotheses that could have helped in explaining the lack of health benefits observed in the data.

In particular, we evaluate our findings in the context of the existing literature on the efficacy of ITNs in reducing the malaria burden. Finally, Section 5 concludes.

2 Location, Study Design and Data

The communities involved in this project are spread across a wide area in the five districts of Bargarh, Bolangir, Keonjhar, Kandhamal, and Sambalpur, all located in the interior of Orissa, India (see Figure 1). Orissa is the most highly malaria endemic state in the country. Official records from the Indian National Vector Borne Disease Control Programme show that, despite accounting for less than four percent of India's population, Orissa accounts for 25% of annual malaria cases, 40% of *P. falciparum* malaria, and 30% of malaria-related deaths in the country (Kumar et al. 2007). According to the 1998-1999 National Family and Health Survey (NFHS), malaria incidence over a three month period was 8.5% in Sambalpur and Bargarh, 8.8% in Balangir, 12.3% in Keonjhar and 17.2% in Phulbani.[2] As we will show, similarly high prevalence rates are also confirmed by our data.[3]

The study was initiated in collaboration with BISWA (Bharat Integrated Social Welfare Agency), a Micro-Finance Institution (MFI) with a large presence in rural Orissa. At the beginning of the study, BISWA provided a list of 878 villages where it had initiated operations, together with rosters of clients as of November 2006. The 878 "BISWA villages" are located in 318 panchayats which in turn are part of 26 blocks in the five study districts. Because panchayats are relatively small administrative units which comprise a limited number of nearby villages, we never include in the study more than one village from each panchayat to limit the extent of contamination.

In completing the baseline randomization, we treated each of the five districts as a separate stratum and selected 33 villages/panchayats from Balangir, 48 from Bargarh, 30 from Keonjhar, 9 from Phulbani and 30 from Sambalpur, for a total of 150 communities.[4] Villages were drawn by means of a pseudo-random number generator, through an algorithm that ensured selection of a multiple of three villages from each block. Within each block, one third of villages were then assigned to each experimental arms, after the completion of the baseline, again by using a pseudo-random number generator. The initial list of blocks excluded areas where the Government of Orissa (GoO) was planning to initiate free distribution in the foreseeable future at the time of the baseline.[5] Despite BISWA's widespread operating network, communities where the micro-lender operate are not a representative sample of all villages in the five study

[2]These figures are calculated using respondent-reported malaria cases among all household members in the three months preceding the survey.

[3]Parasite prevalence varies enormously across the ITN trials reviewed in Lengeler (2004). For instance, in control areas with untreated nets, Luxemburger et al. (1994) reports a 6% prevalence in study location in Thailand, while in D'Alessandro et al. (1995) the figure increases to 39% in The Gambia. In control areas with no net use, ter Kuile et al. (2003) find a 70% prevalence in Kenya, while in Rowland et al. (1996) the rate is only 6% in Pakistan.

[4]The allocation of the sample was approximately in proportion to the number of BISWA communities in each state.

[5]While the study locations were chosen as above to minimize this risk, the sampling scheme was designed to preserve the balanced structure of the sample across treatment groups should the GoO have initiated any unanticipated distribution. Data collected during the post-intervention survey show that indeed distribution of nets from the Government was extremely limited in study areas. We also find virtually no bednet distribution from other NGOs.

districts. In fact, BISWA villages are, on average, larger and with better amenities relative to the overall population.[6] After the baseline survey, but before the intervention, nine of the 150 villages turned out to have no actual BISWA activity and were then later excluded from the intervention as well as from all later data collection efforts. Data from these villages are then excluded from the analysis.

Next, we describe the household-level data collected during the course of the study. The next section describes the pre-intervention data and the nature of the intervention in detail, and briefly lays out the nature and timing of the later data collection efforts. The description of the findings from the post-intervention surveys is left to Section 3 and following.

2.1 Baseline Household Survey

The pre-intervention baseline survey was completed in May-June 2007 for a random sample of 1,844 households with a total of 10,062 members. Because of logistical constraints, the sampling frame at baseline included only households with preexisting BISWA accounts as of November 2006, regardless of whether they had an active loan at the time of the survey. Within each sampled village, we selected randomly 15 households from lists provided by BISWA. In villages where fewer than 15 BISWA households were present, all of them were included.

The baseline survey collected information on a broad range of demographic, socio-economic and health variables. Household-level information included, among other things, dwelling characteristics, ownership of selected assets, borrowing and access to credit and health insurance. The questionnaire also included basic questions about past and expected income, as well as about household expenditure in (or, when relevant, home production of) a comprehensive list of 18 different consumption categories. Individual-level records for all household members included standard information such as age, gender, schooling and occupation, but also included general health status (measured on a 1-to-4 scale) and a complete history of notable health-related problems in the six months before the survey.[7] For each episode, we also recorded all related health expenditures, including any cost for lodging and transportation or loss of income due to missed days of work for the sick person or any care-taker. Although the health history section of the questionnaire was very detailed, the respondent-reported nature of these data should be kept in mind, because there is ample evidence that self-reported health information is often plagued by non-random measurement error (Strauss and Thomas 1998). Because accurate measurement of health impacts was essential for the study, the key health outcomes (malaria infection and hemoglobin levels) were measured in the field with rapid diagnostic tests (RDTs) which require very small blood samples.

Blood tests are essential for the evaluation of malaria prevalence. First, in areas with continuous malaria transmission, adults are often parasitemic but asymptomatic because of acquired partial immunity (Vinetz and Gilman 2002). In addition, fevers of different origins are also often mistakenly attributed to malaria, and symptoms for very young children may be hard to identify. Malaria infection was measured

[6] In Appendix A.1 we document this observation by using village-level characteristics from the 2001 Census of India. Unfortunately, census data do not allow to evaluate differences in terms of exposure to malaria risk or bednet ownership rates.

[7] We recorded health events that satisfied one or more of the following: resulted in loss of one or more days of school or work; required hospitalization or surgery or consultation with health workers; were due to malaria.

via fingerprick blood specimens requiring less than 0.5 ml of blood. Consent was sought to measure malaria infection for all pregnant women, all children under the age of 5 (U5s henceforth), their mothers as well as one randomly selected adult (age 15-60). Malaria infection was determined using the Binax Now® malaria RDT. This RDT is well validated internationally in comparison to blood smears examined through microscopy and provides accurate diagnosis for current or very recent (2 to 4-week) malaria infection (Moody 2002, Farcas et al. 2003, van den Broek et al. 2006, Khairnar et al. 2009).[8] The test is particularly accurate for the most severe form of malaria, caused by *Plasmodium falciparum*, but it can also detect infection from *P. vivax, P. malariae and P. ovale*, although it does not distinguish among them. The test, however, does not indicate the level of parasitemia, so that the result is either negative or positive (to *P. falciparum*, to one of the other three *P.* species or to both). Low hemoglobin level (often referred to as anemia) is a very common health condition in developing countries, where it is often a result of poor nutrition and intestinal parasite infections (Thomas et al. 2006). Malaria can severely worsen this condition, because the parasite destroys red cells and can cause bone marrow dysfunction that can persist for weeks, shortened red cell survival and gastrointestinal haemorrhage. A significant change in anemia rates is one of the most sensitive indicators of changes in malaria prevalence, as demonstrated repeatedly in the literature (Hawley et al. 2003, ter Kuile et al. 2003, Leenstra et al. 2003). Hemoglobin levels were tested with the HemoCue® 201 hemoglobin analyzer (a portable, accurate system for measuring hemoglobin) and the test, like the one used to detect malaria infection, requires less than 0.5 ml of blood and delivered results in approximately 15 minutes. Consent for testing both malaria infection and hemoglobin levels was sought for the same set of individuals.[9] Overall, malaria infection was tested in 2,555 individuals from 1,701 households, and hemoglobin levels were measured for 2,528 from 1,684 households.

In addition to the data described above, we also included questions aiming at gauging respondents' risk aversion and time preferences. Also, separate sections gauged knowledge and practices related to malaria and bednets, including willingness to pay for nets and perceived protective power of nets and treatment with insecticide. Crucial to our analysis, the survey instrument also included a complete "census of sleeping spaces", where surveyors recorded the intra-household sleeping arrangement during the night before the interview, including bednet usage.

2.1.1 Baseline Summary Statistics and Randomization Tests

After the completion of the baseline, the 141 villages were randomly assigned to three groups of 47 communities, each representing a separate experimental arm. We label the three arms (described in detail later) as "Free", when the intervention called for free distribution of ITNs, "MF" (microfinance), when

[8] The test has been shown to have both good *specificity* and *sensitivity*. Both these concepts are defined assuming that the "null hypothesis" of the test is that the individual does not have malaria. The specificity is calculated as the fraction of negative cases correctly diagnosed as such (that is, one minus the probability of a Type-I error). The sensitivity is the fraction of positive cases correctly diagnosed as such (that is, one minus the probability of a Type-II error, or test *power*).

[9] At baseline, but not at follow-up, we also included blood tests to measure the prevalence of Lymphatic filariasis (LF), another mosquito-borne and potentially seriously debilitating tropical disease. Foo et al. (2010) includes the details, and documents the high prevalence of LF in the study area, an unexpected result given that LF was mostly known to be endemic in coastal districts of Orissa.

nets were offered for sale on credit and "Control" when neither intervention was introduced. In Table 1, we report selected summary statistics from the baseline, together with tests for balance across treatment groups. Overall, the null of equality of means across arms is not rejected at standard significance levels in 18 of 20 cases, which suggests that the randomization provided overall good balance across experimental arms.

The figures in the first row of the first three columns of Table 1 show that Scheduled Castes and Tribes and Other Backward Castes represent more than 90 percent of the study population. This proportion is almost identical to the corresponding figure (89%) calculated using data from the study districts from the 1998-99 National Family and Health Survey (NFHS).[10] Households, almost always male-headed, have on average 5.5 members and 0.5 children below age 5. Approximately 50% of members are males and mean age is low, below 28. Literacy rates are low and although about 70% of household heads had at least some schooling, less than 10% overall has a secondary school diploma or above.

Estimates of expenditure per person per day are low and correspond to a figure of just below 1.5USD once purchasing power parity (PPP) is taken into account.[11] For perspective, this figure approximately corresponds to the price of two kilograms of rice in the study areas, or to 1.75 times the official poverty line for rural Orissa.[12] The arm-specific means are very similar and range from 22.3 to 24.2 Rs per person per day, but the estimates are very precise, so that we reject the null of equality at the 10% level (p-value= 0.085). The estimates are approximately 15% lower when we exclude expenditure for dowries and ceremonial expenses, in which case the null of equality is not rejected at standard levels.

At the time of the intervention, bednets were already present in most households, who on average had paid Rs 79 per net. However, bednet coverage was far from universal, with one third of households not owing any nets and an overall mean of one bednet every three persons. The number of treated nets was even lower, ranging from 0.02 ITNs per head in control areas to about 0.05 in Free and MF villages. Note also that despite the generalized low ownership rates, the null of equality in this case is rejected at the 5 percent level (p-value = 0.027). Less than 15% of individuals slept under a net the night before the survey, and less than 3% slept under an ITN. On the one hand, reports about bednets used the night before the interview are unlikely to suffer from significant recall bias. On the other hand, the baseline survey was completed during the hot and dry season, when mosquitoes are less of a nuisance and malaria risk is lower. For this reason, we also asked about bednet use in periods of high mosquito activity. During such periods, more than half of the members were reported as sleeping "regularly" under a bednet. Note, however, that the vast majority of nets in the area were not treated with insecticide, so that even during the mosquito season the protective power of the available nets remained suboptimal. In Figure 2 we show that usage rates are not identical for different ages and genders, although these differences are relatively limited. On average, U5 children appear to be most likely to be protected by nets, while we find a dip in net usage

[10] We could not use data from the latest round of the NFHS (carried out in 2004-05) because district identifiers are not included.

[11] For the most recently revised PPP exchange rates produced by the International Comparison Group see World Bank 2008.

[12] The official rural poverty line for Orissa in 2004-05 was Rs 326 person/month, while the Consumer Price Index for Agricultural Laborers, in this state, increased from 319.5 (mean value between July 2004 and June 2005) to 373 (mean value in May-June 2007).

among teenagers. Women 15 to 30 years old are more likely to use nets than men of the same age, while the sign of the difference is usually reversed for older adults. In any case the vertical distance between gender-specific lines rarely increases beyond 3-4 percentage points.

The results of the blood tests are reported in the bottom rows of Table 1. Twelve percent of tested individuals tested positive for malaria, in almost all cases its most severe form, caused by *P. falciparum*. Almost half of the tested individuals have $Hb < 11$, a threshold often used to define anemia (Thomas et al. 2006). There is also significant heterogeneity in the prevalence of anemia (see Figure 3). Approximately 80% of tested U5, of either gender, are anemic. Anemia rates decline significantly among adults aged 15 to 45, but prevalence remains extremely high (60%) among women, while it is less than 12% among men. Anemia prevalence increases again among older adults, where it characterizes about three-quarters of women and one quarter of men. These patterns for anemia for different ages and genders are common in developing countries (see for instance Thomas et al. 2006). The prevalence of malaria is more balanced between genders and age groups, although women are 3 percentage points more likely to test positive for the parasite (and the difference is significant at the 5% level). Overall, these statistics document the overall poor health status of the study population, and they are consistent with the existence of large potential health gains from measures able to reduce the malaria burden.

2.2 The Intervention

The 141 baseline villages were revisited in September-October 2007, when our field team carried out an information campaign (IC) largely common to all study villages. The IC was carried out publicly, after all BISWA members in a village had been invited. It included a brief presentation about the importance and rationale for ITN use, a demonstration of how to hang nets properly, and advice on proper use and retreatment.[13] The only difference in the IC across experimental arms was that in treatment communities it included an explanation of the intervention assigned to the village. In the 47 "Free" villages, all households with at least one BISWA member (regardless of whether they were included in our baseline sample) received a number of free nets as a function of family composition, with a maximum of four. The nets were treated on the spot by trained personnel, following rules recommended by the World Health Organization (World Health Organization 2002).[14] Individuals were also notified that our team would return after six months to re-treat the nets at no cost. Treatment was completed with a chemical concentration that made re-treatment optimal after six months, using K-Othrine® flow, which contains deltamethrin, a highly effective pyrethroid.[15]

[13] The script of the IC is available upon request from the authors.

[14] While wearing gloves, the field worker dipped the washed net into a bucket where water had been mixed with the appropriate quantity of insecticide. After being soaked for a few minutes, the net is removed from the bucket and is laid flat on a plastic sheet or mat in the shade to dry.

[15] Pyrethroids have been widely used for bednet impregnation with encouraging evidence about the lack of short or long term side-effects on human health (World Health Organization 2005). In Orissa, synthetic pyrethroids have been in use since 1999. Tests performed in 2002-03 in several districts of Orissa (including our study districts Balangir, Kandhamal and Keonjhar) showed high rates of susceptibility to deltamethrin of *Anopheles culicifacies* and *A. fluviatilis*, the two most common malaria vectors in the state (Sharma et al. 2004). The insecticidal efficacy of deltamethrin compound has also been confirmed in

In the 47 "MF" communities, ITNs were offered through micro-loan contracts. Nets could also be purchased for cash but, as we will show, this option was chosen only in a handful of cases. At the time of delivery, nets were treated in a way identical to what was done with free distribution. As in Free communities, only BISWA clients were targeted, although households were free to sell or give away nets purchased during the intervention. In MF villages, the IC also included a detailed explanation of the loan contracts offered by BISWA, and net distribution and recording of loan contracts were to be completed 2-3 days after the IC.[16] The time interval between the IC and the purchase decision was introduced to ensure that the households had an opportunity to consider the offer carefully. In these same MF villages, a second visit was scheduled approximately one month later, and nets were offered again with the same contracts, but no ITNs were offered after this second visit. During both visits, detailed records were taken about attendance and net take up of households included in the baseline survey, together with summary statistics about net distribution at the community level (comparable data were also recorded in villages with free distribution).

Nets were offered for sale with two alternative loan contracts, both at BISWA's standard interest rate, that is, 20% per year. With the first offer, single (double) nets were sold on credit for Rs 173 (223), and repaid with twelve monthly installments of Rs 16 (single) or Rs 21 (double).[17] Survey personnel would then re-visit the villages after six and twelve months and offer retreatment for Rs 15 (single) or Rs 18 (double). With the second contract, the household purchased not only the treated net but also a sequence of two re-treatments. In this case, the price was Rs 203 (single) or Rs 249 (double), to be paid as twelve monthly installments of Rs 19 (single) or Rs 23 (double). With the second option, no cash payment would be required for re-treatment as the loan amount already included the price of the chemicals. To put these prices in perspective, we note that at the time of the intervention daily wages for agricultural labor were around Rs 50, while the price of one kilogram of rice was approximately Rs 10. For sample households, we recorded separately the number and size of the nets received and the contract chosen for the purchase of each net.

BISWA's microcredit operations are based on group lending. Loans, which require no collateral, are offered to borrowers organized in self-help groups (SHGs). The 141 baseline communities hosted a total of 502 SHGs, formed by an average of 12.3 individuals. The number of SHGs per village ranged from one (in about 40% of the communities) to 33 (in one village), with 80% of villages including no more than four SHGs. Each SHG member is responsible for the repayment of all loans granted to the group, which diffuses responsibility to all group members and according to BISWA has been remarkably successful at

Sundargarh, which borders the study district Sambalpur (Yadav et al. 2001, Sharma et al. 2006).

[16] In reality, loan management was not carried out uniformly across the study areas by BISWA personnel. In some locations, especially in Bargarh and Balangir, BISWA officers were less careful, to the extent that in some cases our field team played a central role in loan management and repayment.

[17] The program ITNs were of very good quality and significantly sturdier than most of the pre-existing nets in the study areas. ITNs were composed of white polyester multifilament, mesh size 156, and 75 denier. The nets have bottom reinforcement of 28 cm, and single nets are 180×150×100 cm; double nets are 180×150×160 cm. The nets have been supplied by Biotech International Limited. A total of 6,750 single and 3,250 double nets have been supplied, of which Biotech generously donated 5,000 singles and 2,500 doubles.

ensuring timely repayment. Default is only determined at the end of the loan period, so BISWA clients are allowed some flexibility in the repayment schedule. For instance, a borrower may miss a few monthly repayments during the "lean" agricultural season while paying current and past dues after the harvest, and early repayments are allowed.

2.3 Post-intervention Data and Attrition

Our project team re-visited MF and Free villages in March-April 2008 and in September-October of the same year to complete net re-treatment. Such re-visits had been announced at the time of the intervention and we were successful in ensuring high rates of attendance during both re-treatment sessions. Re-treatment was completed by study personnel in a central location within the village. In Free villages, the service was provided free of cost. The same was done in MF villages for households who purchased nets with the second contract type, where the price included both the net and two re-treatments. Instead, individuals who purchased the nets using the first contract (which did not include re-treatments) were offered the service for cash only. At the time of the first re-treatment, a short questionnaire was filled, with detailed records of re-treatment choices and summary information about bednet ownership and usage. Finally, at the time of the second re-treatment, we only recorded choices and, in MF villages, information about loan repayment up to that moment.

A detailed post-intervention survey was fielded shortly after the second re-treatment, between December 2008 and April 2009. The content of the survey instrument was overall similar to the baseline and considerable effort was devoted to measuring bednet ownership and usage and health status. Biomarkers were again included to measure malaria infection and hemoglobin levels. A longitudinal data set was created by re-contacting all baseline households whenever possible. We also increased significantly the number of biomarkers collected by seeking consent for malaria and hemoglobin RDTs for all household members, rather than for specific demographic groups as done at baseline.[18]

Attrition at follow-up was limited. Of the 1,844 initial households, 1,768 (96%) were re-interviewed. Attrition was mostly due to temporary migration or inability to find eligible respondents despite repeated visits.[19] Refusals accounted for only 13 of the 76 lost households. Attrition in MF and control villages was almost identical, while it was 2 percentage points lower in Free communities. Overall, the null of equal attrition rates among arms is not rejected at standard levels, regardless of whether we use individual or joint tests (see Table 2, column 2). The results in columns 3 and 4 of Table 2 show little correlation between attrition and household characteristics at baseline, including RDT results and bednet ownership and usage. The only regression coefficients that are individually statistically significant indicate that households with

[18] In addition, we also significantly enlarged the sample by including households not interviewed at baseline. First, 10 new households were randomly surveyed from each of the 141 baseline communities. Finally, an additional 25 villages were included in the study, and 15 households were selected from each of these communities. The new villages were selected from the same randomly sorted lists used for the selection of the communities at baseline, by selecting the "next 25 villages" from the list. All these non-baseline households were drawn from census lists regardless of BISWA affiliation, so that both BISWA and non-BISWA households are included. Information from the enlarged sample is not used in this paper and will be used in a separate study.

[19] The survey protocol called for at least three attempts, but a handful of households were re-contacted after 4 or 5 visits.

an older and better educated head are less likely to exit the panel. On the other hand, we cannot reject the joint null that all the included slopes are equal to zero (p-value=0.14).

Finally, we also check whether significant changes in household composition took place between the baseline and the follow-up survey, as well as whether such changes were balanced across experimental arms. This is potentially important for two reasons. First, changes in actual availability of nets may also arise from changes in the number and age of households members. Second, we have documented that malaria and anemia prevalence at baseline differ across age and gender groups, so that changes in the demographic structure of the household may confound, in principle, aggregate changes in such health measures calculated over all household members. We look both at entry into or exit from panel households and to changes in the relative weight of different demographic group. Here we only discuss briefly the results, which are discussed in detail in Appendix A.2. Overall, we find significant entry and exit, with about 10% of baseline members no longer present, and a comparable number of individuals having joined the household over the same period. However, such changes are very similar across arms. The changes in the relative weight of age and gender groups are instead very small and, despite some statistically significant difference across arms, the magnitude of the differences are very small. Overall, we find that none of the results discussed below can be plausibly explained by changes in the composition of panel households.

3 Results

Before describing the results of the intervention, we lay out the basic notation that will be maintained throughout the rest of the paper. First, the village-specific experimental arm relevant for unit i (household or individual) is described by the binary variables $Free_i$ (= 1 if the unit lives in a village where ITNs were distributed free of cost) and MF_i (in villages where ITNs were offered for sale on credit). Control villages are then the omitted category. The index t denotes time and indicates when the relevant variable was recorded. We use the following time subscripts: $t = b$ denotes baseline (spring 2007), while $t = d$ for data gathered during ITN distribution (fall 2007), $t = r_1, r_2$ for data from the 1st and 2nd re-treatment and finally $t = p$ for the final post-intervention survey. Unless noted otherwise, all regressions results describe intent-to-treat (ITT) estimates. In other words, we focus on post-intervention differences in outcomes between experimental arms, regardless of actual program take up.

3.1 ITN Take Up and Ownership

We first evaluate the impact of the intervention on bednet take-up, measured at $t = d$, in fall 2007. Because no distribution took place in control areas, we estimate the following model using only information from the 1,199 panel households residing in the 94 Free and MF villages:

$$y_{id} = \beta_{Free} Free_i + \beta_{MF} MF_i + u_{id}, \tag{1}$$

where y_{id} is a measure of net take-up for household i at the time of the intervention d.[20] We estimate all regressions with Ordinary Least Squares (OLS), clustering standard errors at the community level. The

[20] The results are almost identical if we also include non-panel households that were not re-interviewed after the intervention.

results are displayed in columns 1-4 of Table 3. In communities with free distribution, almost all sample households (96%) received at least one ITN, with an average of 2.7 nets per household received during the intervention (one every two people on average). Of the 610 sample households in Free villages, only 25 did not receive any nets, and in 22 cases this happened because our field teams could not locate any member at the time of the net delivery. In MF villages, take up was significantly lower, at 1.2 nets per household (one every four household members). Still, it is remarkable that 54% of sample households purchased at least one net, despite their non-trivial cost. Note also that, among buyers, mean take up was close to that achieved with free distribution, although the difference is significant at the 10% level: 2.3 nets in MF communities versus 2.8 with free distribution (column 3). In MF villages, take-up was strongly correlated with measures of past malaria exposure at the time of the baseline survey. Such measures included the monetary cost of recent episodes, self-reported incidence and infection detected by our field team through RDTs (see Appendix A.3 for details).

It is also interesting to look at the variation in the number of ITNs received across communities. While we find, predictably, low variation in take-up rates in Free villages, in MF villages there was significant heterogeneity. In Figure 4 we display the distribution of mean per capita ITNs received during the intervention, by experimental arm. On the one hand, most of the variation in take up with free distribution is due to differences in household composition and, in some cases, to the absence of household members during the visit. On the other hand, ITN sales led not only to a lower mean but also to more variation, with no purchases in five of the 47 villages. Note also that the two evident outliers among the MF villages are due to a number of sample households who decided to purchase a large number of ITNs for resale. If we omit these two communities from the analysis, take up in MF areas declines from 1.2 to 0.9 ITNs per household, and from 2.3 to 1.8 if we only include households who purchased at least one net (results not shown).

Next, we turn to the assessment of bednet ownership at the time of the post-intervention household survey, completed in winter 08/09. We report ITT estimates where the dependent variable y_{ip} is the number of bednets owned by the household, either in total (column 5 of Table 3) or per person (column 6). Net ownership was reported by the respondent, but enumerators were instructed to always ask permission to check that the net was present with the household. The presence of the net was confirmed in this way in about 90% of cases so that the estimates are likely to be only marginally affected by reporting errors. Note also that y_{ip} includes all nets owned by the household, regardless of acquisition mode or re-treatment status. The model is then

$$y_{ip} = \beta_0 + \beta_{Free} Free_i + \beta_{MF} MF_i + u_{ip}, \tag{2}$$

where $\hat{\beta}_{Free}$ and $\hat{\beta}_{MF}$ are the estimated differences in net ownership at follow-up relative to control villages. In columns 7 and 8, we also report difference-in-differences (DD) estimates where the dependent variable is the change in net ownership between pre and post-intervention surveys. In this case the estimated regression is then:

$$y_{ip} - y_{ib} = \beta_0 + \beta_{Free} Free_i + \beta_{MF} MF_i + u_{ip}, \tag{3}$$

where ownership is either at the household level or on a per capita base. As expected, given that net

ownership at baseline was overall balanced across treatment arms (see Table 1), the impact estimated with models (2) and (3) are very similar, although they are more precise in the latter models. The relative magnitude of the estimated coefficients is consistent with the take-up results described earlier. The sale of bednets on credit was successful in increasing ownership rates, although not to the point of reaching the close-to-complete coverage achieved with free distribution. Although even in control communities we observe a statistically significant increase of 0.07 nets per person, the increase was twice as large in MF villages and four times as large with free distribution.[21] Indeed, free distribution achieved a coverage of 0.66 nets per person, which is close to the figure of 1.5 persons per net which has been taken to represent full coverage in some contexts (see for instance ter Kuile et al. 2003). However, we also find evidence that the increase in net ownership was markedly lower than the number of nets delivered during the intervention. On average, households received 2.7 nets in Free villages, but post-intervention net ownership was only 1.9 higher than at baseline, and 1.6 higher than in control areas. Similarly, in MF communities an average of 1.2 nets per household were purchased, but at follow-up the difference in ownership relative to controls was only 0.6. These findings are consistent both with the new nets not being retained, or with older nets being disposed of, a point we will return to later in the paper.

3.2 ITN Usage

Next, we move to the analysis of bednet usage among panel households. We re-estimate models (2) and (3) using respondent's reports about individual net usage as dependent variable. Both at baseline and follow-up, we recorded whether each household member had slept under a bednet the night before the interview. In case of an affirmative answer, we also asked whether the net had been treated in the previous six months. Neither wave was completed in late summer/early fall, when mosquito activity peaks, so we also added separate questions about bednet usage during the peak season. The distinction between treated and untreated nets was made only at follow-up, so we estimate only model (2) when the outcome is a dummy for "regular use" of a net during the peak season. The results are in Table 4. Estimates of model (2) are obtained using information from all household members at follow-up. By construction, the DD estimates (model 3) use instead only information about net usage for individuals who were household members at both time periods, which explains the smaller sample sizes in these regressions. Missing values are responsible for the other differences in sample size. In most cases, the respondent did not know whether the net used the previous night was treated, while in others net usage the previous night was not known while regular usage during the peak season was.

The results are largely consistent with the ownership patterns in Table 3: net usage in MF villages is significantly larger than in control areas, but it remains significantly lower than what achieved with free distribution. The difference in usage of "any net" or ITNs between MF and Free villages is always significant at the 1% level. In control areas, 18% of individuals slept under a net the night before the interview (column 1), but only 2% slept under a treated net (column 3). The DD estimates in column 4 show virtually no increase in the usage of treated nets relative to baseline in control villages. In MF

[21] The very small increase in net ownership in control areas also provides suggestive evidence that the short information campaign likely did not change behavior substantially.

villages, we find instead a 14 percentage points increase in ITN usage, which is even larger (46 percentage points) with free distribution. The estimated impacts are overall very similar when we use only information from the follow-up.

A very interesting result is that the newly available ITNs appear to have displaced non-treated nets, especially in areas with free distribution. The figures in column 6 show that usage rates of untreated nets increased by 5 percentage points in control areas and barely changed in MF villages, while the fraction of users *decreased* by about three percentage points in Free communities. These overall patterns are confirmed when we look at "regular usage" during the peak mosquito season, although as expected reports indicate significantly higher usage in this case. In control areas, two thirds of individuals are reported as using nets regularly, but only 6% use ITNs. In MF villages these proportions increase to 83 and 36%, and with free distribution they reach 93% (that is, almost complete coverage) and 77%. Once again, the results for untreated nets suggest replacement of older, untreated nets with new ITNs (column 10). The fraction of individuals sleeping regularly under an untreated net is 0.59 in control areas, 0.47 in MF areas and only 0.15 in Free villages. These findings are also consistent with the observation, discussed earlier, that the increase in the number of nets between pre and post-intervention surveys was significantly smaller than the number of nets distributed during the program. Surely some of the new ITNs were given away or lost, but a number of them apparently replaced some of the lower-quality previously owned ones, which were then likely discarded.

A related question is whether the increase in net usage in program areas favored specific demographic categories disproportionately. In Figure 2 we have shown that, at baseline, usage rates were higher among very young children but were otherwise relatively homogeneous. In Figure 5 we show changes in the fraction of household members in a specific age-gender group who slept protected by a net the night before the interview, by experimental arm. Each column also displays 95% confidence intervals, robust to intra-village correlation. The small increases in usage rates in control areas are consistent with the small changes in ownership documented earlier, and were also to be expected because the post-intervention survey took place during a period of stronger mosquito activity. In what follows we only focus on the changes in usage in MF and Free communities. First, we find that increases in usage rates of ITNs decline with age, especially in Free areas, where, for instance, usage goes up by about 60 percentage points among U5, but only by half as much among members over 60 years old. Second, the increases in ITN use are almost identical between genders (compare graphs A and B). Third, among U5 children, we find that the decline in usage of untreated nets is only found among boys (8 percentage points decline, vs. 1 percentage point increase among girls) although the estimates are noisy and we cannot reject the null of equal changes.

3.2.1 ITN Usage and Cost

The design of our study also allows us to address, to some extent, the question of how the price paid affects usage of health-protecting technologies. A number of development practitioners hold the view that usage, conditional on ownership, may be sub-optimally low with no cost sharing. Broadly, there are three main motivations why users who have paid a positive cost may be more likely to use the product than with free

provision: first, cost-sharing naturally implies self-selection into purchase, so that individuals who care more about the product are more likely to be willing to pay for it and hence to use it; second, positive prices may be interpreted as quality signals; third, the so-called "sunk-cost fallacy" may lead individuals to use a purchased product relative to a free one because they want to rationalize ex-post the purchase. On the other hand, the empirical validity of these arguments has been evaluated and refuted in the context of two recent RCTs, Ashraf et al. (2008) for the case of chlorine in Zambia, and Cohen and Dupas (2010) for the case of ITNs in Kenya. Both studies use a two-stage randomization design. In a first-stage, the willingness to pay is revealed through actual sales at randomly determined prices. In the second stage, a randomized discount implies that individuals with the same revealed WTP actually pay a different price, which allows to rule out self-section into purchase (conditional on price).

Our study, lacking such two-stage randomization, is not ideally designed to study the link between cost and usage. However, we show below that in our sample we find *higher* usage rates conditional on ownership when net were received at no cost. This means that we can exclude that the net effect of self-selection, quality signalling and sunk-cost effect actually work in the direction hypothesized above. An obvious caveat of our results is that our measures of net usage are self-reported. It is easy to hypothesize reasons why free provision of nets may lead to overstated usage rates, perhaps because of gratitude, or because beneficiaries may be afraid of losing the nets if they are found to be sparsely used. To reduce to some extent the risk of reporting bias, we estimate usage rates including only ITNs actually identified by the enumerators as nets distributed through our program. Although this strategy does not eliminate the risk of bias in self-reports of usage, it does reduce considerably measurement error in ownership rates.[22]

The top panel of Figure 6 shows estimates of the fraction of nets (distributed through our program and observed by the enumerator) that were reported as having been used the night before the survey, as a function of the number of nets available in the household. The bottom panel shows estimates of the difference in usage rates between MF and Free villages together with 95% confidence intervals.[23] The results show no evidence that paying a positive price increase usage. We actually find again that when one or two BISWA nets were present usage was 8-19% *higher* in free communities, although the null of no difference is only rejected when one net was present. The difference changes sign when four or five nets were present, but sample size over this range is very small and the estimates become very imprecise, as shown by the large confidence bands.

Overall, our results are then largely consistent with Ashraf et al. (2008) and Cohen and Dupas (2010) in that we do not find evidence of more product usage among users who paid for it. Additional caveats should be mentioned, however, besides the most obvious one that usage was self-reported. First, whether BISWA nets were retained (and then present at the time of the follow-up survey) is likely endogenous. Our estimates should perhaps be repeated taking into account the number of nets delivered at the time of the

[22] In a large majority of cases, enumerators were able to verify the presence of the nets. In any case, the results are very similar when we estimate usage rates for all BISWA nets, regardless of whether the net was actually observed.

[23] For a given number of nets N, let $\hat{d}_N$ denote the difference between the mean fraction of nets used in Free and MF communities. We construct 95% confidence bands for the estimated differences as $\hat{d}_N \pm 1.96\sqrt{\hat{\sigma}^2_{Free} + \hat{\sigma}^2_{MF}}$, where $\hat{\sigma}_{Free}$ and $\hat{\sigma}_{MF}$ are the estimated standard errors of the mean fraction of nets used in Free and MF communities respectively. Standard errors are robust to intra-village correlation.

intervention rather than the number of nets present in the households at the time of the follow-up survey. Second, although most households did eventually pay the nets in full, not everyone did.

3.3 The Decision to Retreat

Periodic retreatment with insecticide is known to increase significantly the protective power of bednets. Although several public health programs worldwide are advocating the use of long-lasting nets (LLINs) which do not require re-treatment, the supply of such devices is still relatively limited and in most developing countries standard nets remain the only option available locally. Non-LLINs are certainly the standard in our study areas but, similarly to what is often observed in poor countries, re-treatment of nets is very uncommon (see Section 2.1.1).

In Free and MF villages, our field workers took care of two re-treatments after approximately six and twelve months from the net delivery, as recommended based on the type and concentration of the insecticide. In villages with free distribution of nets, the re-treatment was offered free of charge. In MF villages, whether re-treatment was offered for cash or at no additional cost depended on the contract chosen by net buyers. As a reminder, BISWA offered all microfinance clients the opportunity to purchase nets through two alternative contracts. The first contract included only the treated net (contract "C1"), while the second was a bundle which also included a sequence of two retreatments (contract "C2"). Contract C2 can therefore be seen as one which financially "commits" the buyer to comply with future retreatments. Buyers could either purchase the nets on loan, with a one year repayment period, or they could pay cash. From a total of 589 panel households, 309 (52%) purchased at least one net, but only 10 (less than 2%) bought any net for cash. Clients could also choose a mix of contracts, but among the 309 buyers in our sample only 19 of them (6%) did.[24] Among the 290 clients who purchased nets with only one type of contract, the choice was almost exactly evenly split, with 144 choosing C1 and 146 choosing the "commitment product" C2.

During re-visits, no additional payment was required if nets had been purchased with C2, but owners of C1 nets had to pay cash in order to obtain retreatment. Fees were Rs 15 for a single net and Rs 18 for a double. Table 5 show the retreatment rates in Free villages relative to those observed among buyers in MF communities, shown separately as a function of the contract chosen. The re-treatment rates are estimated by OLS with standard errors clustered at the village level. The sample comprises all panel households that received nets during the intervention, excluding the handful in MF communities who purchased nets with both contract types. We find that after six months 92% of free nets were re-treated (column 1), but the proportion of treated nets decreased to 83% in the second re-treatment (column 3). Both retreatment rates were then very high, although not equal to 100%. A sizeable fraction of the shortfall appeared to be accounted for by nets being no longer with the household, and retreatment rates increase to 94 and 91% in the first and second re-visit once we exclude households where at least one net was reported as having been sold, stolen, lost or otherwise not available for retreatment (column 2 and 4).

[24] Seven of the 13 were from one of the two "outlier" villages where a large number of nets were purchased with the purpose of resale.

As expected, retreatment rates were similarly high among households who purchased the commitment product C2 in MF communities. When we look at the estimates in columns 1 and 3, re-treatment rates are 8-9 percentage points lower and the difference is not significant at standard levels in the second re-treatment. Once we exclude unlikely re-treaters (columns 2 and 4), the fraction treated is almost identical to what observed in Free villages after six months, and 9 percentage points lower after one year, although the difference is not significant at standard levels. Remarkable differences emerge instead relative to re-treatment rates among buyers who chose contract C1, which did not include a commitment to re-treat. Recall that, for these households, re-treatment was only offered in exchange of an immediate cash payment. During the first re-visit, only 36% of nets purchased with C1 were retreated, with the fraction going further down to 25% during the second re-visit. The difference in retreatment relative to the mean observed in Free villages is statistically significant at all conventional levels. Overall, then, we find a very strong association between re-treatment rates and pre-commitment to re-treat. These results, however, do not necessarily identify a *causal* association between contract type and probability of re-treatment, because households chose the contract, so that contract type is clearly endogenous. This is also confirmed by results from a LPM where we regress a binary variable equal to one if a buyer household in a MF village purchased at least one net with the commitment product C2 on the same set of correlated we used to explain purchase decisions in Table A.13 (the results are available upon request). In fact, several demographic characteristics and measures of past malaria exposure are statistically significant predictors of contract choice.[25] However, we find that the estimated differences in re-treatment rates in Table 5 change only marginally once we include in the regressions all these other correlates (results not shown).

Lastly, we note that retreatment rates were overall very high when offered for free. Although even in Free communities re-treatment was not universal, the fraction of nets distributed who maintained insecticide capability should have therefore remained very high over time, therefore increasing the expected protective power of nets. In MF communities, on the other hand, not only significantly fewer nets were distributed, but the fraction re-treated was also lower, largely due to the low re-treatment rates among households who choose the non-commitment product C1.

3.4 Impact on Health Outcomes

Next, we finally move to the estimation of the main outcomes of interest, that is, the measures of hemoglobin levels (Hb) and malaria infection detected through finger-prick blood RDTs. Our prior hypothesis was that the considerable increase in ITN ownership rates would have led to significant improvements in health outcomes, especially in areas where we implemented free distribution. Unfortunately, we will show that such improvements did not take place.

As a reminder, population targeted for testing was composed of pregnant women, all U5 as well as their mothers and, in addition, a randomly selected adult (age 15-60). In the post-intervention survey, the availability of additional funding allowed us to target all household members. Our testers were able to

[25] This result is opposite to what found in Tarozzi et al. (2009) using the same data: such earlier work focused on a much shorter list of correlates and in that case we could not reject the null that contract choice was uncorrelated with household characteristics.

successfully test 75% of members in panel households, while 19% could not be tested because they were not present at the time of the visits and only 6% because consent was not given. In Appendix A.4 we show that both refusal and absence were balanced across experimental arms, and that testing success was significantly higher (∼90%) for U5 and women 15-45. Adult men were the least likely to be tested, mostly because they were most likely to be off to work during the survey.

The ITT estimates of the program impact on health outcomes are reported in Table 6. We look at three different outcomes. First, malaria prevalence as measured by a binary variable equal to one if the RDT indicated current infection with *Plasmodium*. The tests could also distinguish to some extent the *Plasmodium* species, but because *P. falciparum* was responsible for almost all infections, we simply model malaria using a binary format. Second, we evaluate the impact on hemoglobin (Hb) levels, measured in grams per deciliter of blood. Third, we study the prevalence of anemia, defined here as Hb$<$ 11g/dl. For each one of three outcomes, we estimate the impacts using two alternative samples. In a first set of estimates we include all blood tests completed at follow-up, regardless of whether the individual tested was a new member or a pre-existing one who was not tested at baseline. These regressions include a sample of 7,154 individuals for malaria and 7,149 for anemia/Hb.[26] Next, we show DD estimates using, by construction, only information from individuals tested in both surveys. These estimates include 1,896 observations for malaria and 1,869 for Hb.[27] All regressions are then estimated using individuals as the unit of observations, but the standard errors are as usual clustered at the village level.

In all but one case, the results show no improvement in health. In particular, and regardless of the sample used, we cannot reject the null of no difference in malaria prevalence relative to control areas, regardless of whether we use individual or joint tests. In column 1, where we use information from all individuals tested at follow-up, we see that 18.3% of individuals tested positive for malaria. Prevalence in Free and MF communities is similar but actually about 20% *higher* in intervention communities, and the fraction of positive tests is only marginally lower in Free vs. MF villages.[28] The DD estimates are similar. Relative to baseline, malaria prevalence in control areas increased by 6.3 percentage points, from a baseline of 0.11 (see Table 1). This was to be expected, because the baseline survey was completed during the hot and dry months of spring, when malaria prevalence is lower. However, consistently with the follow-up only estimates, the null of equal change in intervention areas cannot be rejected at standard levels, although the estimated increase in intervention areas was about twice as large. This is all the more surprising for areas where nets were distributed free of cost, where we have documented very large increases in net ownership and (self-reported) usage, as well as very high rates of net re-treatment with insecticide.[29]

[26] Consent was sought to test all individuals for malaria infection and Hb. Both tests were completed for 7,138 individuals, while we have valid data for malaria only for 16 individuals, and for Hb only for 11.

[27] The ITT estimates that include all tests from individuals that were already part of the household at baseline are almost identical to those with the full sample.

[28] Given that ITN ownership and usage are significantly higher in Free and MF villages relative to controls, these results also lead to a *positive* association between malaria prevalence and ITN usage or ownership, when we estimate the relationship with instrumental variables, using treatment status as instrument (results available upon request).

[29] When we calculate mean changes in malaria prevalence within village, we find that malaria declined in only 11 of 47 control, 9 of 47 Free and 8 of 47 MF villages. However, we find increases in prevalence in 20 control, 27 Free and 30 MF communities.

Looking now at hemoglobin levels, when we use only follow-up data the estimated impacts are very close to zero and not significant (column 3). The DD estimates are the one instance where we find some evidence of health improvements, with an average increase in Hb of 0.2 g/dl, which is just above 10% of a (baseline) standard deviation. This increase is statistically significant at the 5% level. It is also interesting to note that, despite the overall increase in malaria prevalence, the mean hemoglobin levels improved relative to baseline. In control villages the mean increase is 0.28 g/dl. This is perhaps due to better nutrition during the follow-up survey, which took place in a period when household income is seasonally higher for many households.[30] Next, we find again no evidence of health improvements when we look at anemia prevalence, which in control areas declined by 11 percentage points relative to baseline (column 6). When we use the complete follow-up sample, we find small and not significant impacts of the interventions. The DD estimate show a 2.4 percentage points decline in anemia relative to control areas, but the difference is not, unlike for Hb, statistically significant. In MF villages, the point estimates of the differences relative to control areas are instead positive in both models (but not significant at standard levels), somewhat in contrast with the mean relative increases in Hb.

An important question is whether the lack of health benefits was shared by all demographic groups. The bars in Figure 7 show malaria and anemia prevalence for each experimental arm by gender and age group, together with 95% confidence intervals. We look at malaria prevalence first, whose relative magnitude across arms we find to vary by gender and age. Among males (panel A), prevalence is ∼15% and almost identical across arms among adults (age 15 or above). However, among U5 the rate in control villages is only about half as large as in treatment areas, where about one child out of five tested positive (although the difference is not significant). Prevalence among males is the highest among 5-14 boys, where the bars look like a vertical shift of those for younger children. In MF villages, one third of boys tested positive. These patterns do not remain the same when we look at females (panel B), where we observe almost identical prevalence across arms among the youngest girls (∼15%) and higher prevalence in intervention villages in older age groups. Overall, then, there are remarkable differences in malaria prevalence, but these are largely concentrated between genders or across age groups rather than across experimental arms. The results for anemia (panels C and D) show again large systematic gaps across gender-age groups, but not across treatments. The most striking differences, also documented at baseline, are the significantly higher prevalence rates among females 5 and older relative to males of the same age.

4 Interpretation and Discussion

Given the overwhelming evidence accumulated to date about the efficacy of widespread use of ITNs in reducing the malaria burden in a variety of areas and conditions (Lengeler (2004)), we expected to observe declines in malaria and anemia prevalence. Indeed, one of the primary objectives of our study was the evaluate to what extent a program of ITN sales on credit could replicate the benefits of free distribution,

[30] In both pre and post-intervention surveys, November and December are the two months which are most frequently indicated by respondents as being associated with the highest seasonal income.

which was included in the study as an important benchmark. We have shown that micro-loans were successful at increasing considerably ITN ownership and (self-reported) usage, although coverage remained significantly below what achieved with free distribution. Despite this, we have shown that health outcomes remained very similar between MF and Free communities and that, more surprisingly, such outcomes did not improve relative to control areas. Why did this happen? In this section we discuss a number of hypotheses which the richness of our data set allows us to explore. We provide evidence against a number of *a priori* plausible hypotheses, and discuss other explanations which we conjecture are likely to be key in explaining the results.

4.1 Changes in Other Prophylactic Behavior

A first hypothesis to explain the lack of health impacts is that the availability of a larger number of ITNs led to unexpected behavioral responses leading to perverse outcomes. On the one hand, we know that resting under an appropriately arranged and intact net provides a mechanical barrier against *Anopheles* mosquitoes, who mostly bite during the sleeping hours.[31] On the other hand, there are other precautions that can be taken to reduce the risk of malaria. Examples are indoor or outdoor wall spraying with insecticide, the use of coils, or the control of drainage pools. Is it possible that the broader availability of nets in Free and MF villages reduced the use of such alternative prophylactic measures? We can test this hypothesis to some extent using data on knowledge and practices collected during the post-intervention survey.

In Table 7 we look at differences among experimental arms in knowledge about causes of malaria (panel A), precautions taken against it (panel B) and wall spraying (panel C). The survey instrument asked respondents, without prompting, to list all possible causes of malaria, and then asked "[w]hat are the best precautions you can take to protect yourself from getting malaria." The means in the top panel A show that households in intervention communities appear to be about as knowledgeable about causes of malaria as those in control areas, although the test of equality is rejected at the 10% level (but not at the 5%) for three of the four causes of malaria. In each case, however, it is one of the experimental arms which records the most accurate responses. Note also that in all groups 85% or more of respondents list mosquito bites as a cause of malaria. If panel A shows that mosquitoes were similarly recognized as a malaria vector across the groups, the figures in the bottom panel B show no remarkable or systemic variation in malaria-avoiding behavior. Bednets are by far the most commonly listed precaution, mentioned in all arms by more than 80% of respondents (with the highest proportions in intervention villages). The next most common precautions are "avoid contaminated environment" (16-21%) and "avoid drinking contaminated water (6-8%). The test of equal means is never rejected at the 5% level, although the joint null of equality for all behaviors is (p-value = 0.0421). But note that the differences in alternative risk-avoiding behavior are not consistent with such behavior being more common in control villages, and indeed in several cases

[31] The late-night biting habits of most anophelines is key for the efficacy of ITNs, although important seasonal and geographical variations have been observed (see Pates and Curtis 2005 for a review). In areas within Keonjhar, one of our study districts, Sahu et al. (2009) find that biting activity of the major malaria vectors was concentrated in the two middle quarters of the night, regardless of the season.

they indicate the opposite (see, for instance, use of smoke, long sleeves, or cleaning of drainage pools).

Next, we analyze difference in residual spraying of indoors or outdoor walls. Spraying, like ITNs, is widely considered an effective tool in the fight against malaria (Mabaso et al. 2004, World Health Organization 2006). In panel C, we show the proportion of baseline households whose inner and outer walls had been sprayed with insecticide in 2008-09, that is, after the distribution of nets, which took place in the fall of 2007. A large fraction of respondents reported that the house walls had been sprayed recently, 36% and 38% for inner and outer walls respectively. However, although the null hypothesis of equal proportion among treatments cannot be rejected at standard levels, the magnitude of the differences between control and intervention areas is large. While 40% of households had the inner walls sprayed after 2007 in control areas, 37% did in Free villages and the fraction declines to 30% in MF communities. The proportions who had the outer walls sprayed in the three groups were respectively 53, 48 and 44%. The reason why the null is not rejected despite the large differences is that spraying is usually done by public officials who intervene at the village level, so that the intra-village correlation for these two variables is very large (0.41 and 0.63 for inner and outer spraying respectively). Our data do not tell us if these differences were mainly the result of household decisions, or if instead they resulted from choices made by public officials who scheduled wall spraying taking into account our intervention. To evaluate whether differences in spraying rates help explain the lack of health benefits in intervention villages, we re-estimate the ITT models in Table 6 including dummies for recent wall spraying among the regressors. In Table 8 we show that this leaves the estimated impacts almost identical. Overall, then, we find no evidence that our results are due to changes in household risk-coping behavior.

Similarly, the absence of relative health improvements cannot be explained by the presence of other programs of net distribution sponsored by the Government or by other NGOs. First, the results on net ownership in Table 3, which showed large relative increases in ITN ownership rates, included nets from all sources. Second, the figures in panel D of Table 7 confirm that the number of nets received from other sources was very small and not significantly different across all arms.[32] Not surprisingly, we find instead that fewer nets were purchased from the market in MF and especially Free villages.

4.2 Measurement Error in Health Measurements

A second hypothesis is that the lack of impacts was due to errors in reading the results of the RDTs. On the one hand, the RDTs we used to detect malaria infection have been shown to have very high specificity (the probability of detecting correctly a positive) and sensitivity (the probability of detecting correctly a negative). On the other hand, the interpretation of the blood test through RDTs presents a higher degree of subjectivity relative to the more accurate but more expensive microscopy. The RDT result is read on a test strip, located on a card, where a reagent is added to the blood sample. The presence of specific *Plasmodium* antigens in the blood is signaled by the appearance of darker lines on the white strip. Although high concurrency between test readers (including non-trained ones) has been documented in clinical trials (Khairnar et al. 2009), a degree of subjectivity is hard to rule out completely, because the

[32] We also find no evidence of sizeable distribution of ITNs among non-BISWA households.

lines can sometimes be hard to detect when parasitemia is low. During the field work, positive RDTs were not confirmed with microscopy, so we cannot gauge directly the nature and extent of measurement error.[33] However, we argue that measurement error is unlikely to explain the lack of estimated health benefits.

First, random misclassification of a binary dependent variable leads, by construction, to negative correlation between the error and the true value of the variable. As long as the true and the mis-measured values are positively correlated (as they likely are in our case) this leads to attenuation bias (Hausman et al. 1998, eq. 15).[34] Since the point estimate for prevalence was *higher* in treatment areas, misclassification of the left hand side would mean that this is an *underestimate* of the effect of the treatment.

Second, a small validation study carried out after the conclusion of the follow-up survey, confirmed high degrees of accuracy of the RDTs. The study was done in July 2009, in collaboration with the Malaria Research Centre (MRC) Field Station in Rourkela (Orissa). The MRC medical team allowed us to use the RDTs to test 205 blood samples independently collected from symptomatic individuals from three villages. The RDT cards were interpreted, in a blind setting, by three different readers, that is, by two of the testers used during the study and by the most senior survey monitor in our research team. The results were then compared with the blood infection status as determined by the MRC team through microscopy, which was accepted as the correct infection status. The results showed very high specificity ($> 90\%$ for each of the three readers).[35] The fraction of correctly identified negatives (sensitivity) was also high but ranged from 74 to 85%. The higher prevalence measured by the RDTs was not surprising, given that these tests often detect the presence of *Plasmodium* antigens even 2-4 weeks after the infection has cleared (Humar et al. 1997). Overall, RDT results were then very similar but not identical between readers (correlations ranged from 0.78 to 0.88). To check whether systematic differences in the interpretation of the malaria RDT played a role in the results, we re-estimate program impacts using tester fixed effects (see columns 5 and 6 of Table 8. The differences among experimental arms become slightly smaller, but they remain positive and not significant at standard levels.

In addition, we note that if parasitemia has been declining in treatment villages, the likelihood of fainter and then harder-to-detect test lines would have likely increases in these areas, possibly *overestimating* the reduction in prevalence. Finally, measurement error was very unlikely to be a problem for the hemoglobin measurements, which also showed little evidence of differential changes across experimental groups. Unlike malaria RDTs, measuring Hb simply requires reading a number from the display of a HemoCue machine. In addition, the reliability of the malaria RDTs is also consistent with the strong cross-sectional correlation between malaria infection status and Hb levels. When we regress Hb on a dummy for a positive malaria test, the slope is significant at a one percent level (slope -0.19, p-value$= 0.000$). Overall, we conclude that the lack of health benefits observed in our data reflect the reality in our sample, and is not the result of imperfect measurements.

[33] However, at the beginning of the study, the reliability of the RDTs was successfully checked by testing a limited number of blood samples with or without malaria infection.

[34] This is unlike the standard case of classical measurement error in a continuous dependent variable, which only affects the variance of OLS estimates, while retaining consistency.

[35] See Table A.15 for details.

4.3 Reliability of Reports on Net Ownership and Usage

An alternative possibility for the lack of health benefits in intervention villages is that respondents systematically overstated the number of program ITNs retained by the household and/or the usage rates, conditional on ownership. Given that the study design did not call for regular nightly checks on net usage, our data do not allow us to rule out conclusively the hypothesis of low usage rates. However, our data provide strong evidence against a large and systematic over-reporting in the number of ITNs owned.

The ITT estimates of usage rates in Table 4 were obtained using information on net usage recorded separately for each household member. However, net usage during the previous night, as well as the actual presence of the net with the household, were also recorded independently in a "census of sleeping patterns" separately included in the questionnaire. Then, we can re-estimate the ITT for ITN usage the night before the survey using this alternative source of information. Surveyors listed all "sleeping spaces" used by household members (including those outside the dwelling) and recorded the identity of the person(s) who slept there the previous night. The surveyor also recorded whether the space was protected by a net and, in such cases, the origin and price of the net and of any recent re-treatment. Finally, for all nets reported as having been used, surveyors asked to see the net and, if allowed to do so, they recorded the net condition, whether it was hanging properly and whether the net was recognizable as one of those distributed by our program. Interviews usually took place during waking hours, when sleeping areas usually double as living spaces, so that we did not expect nets to observe nets hanging properly from hooks or poles.

The results in column 1 of Table 9 show that the ITT estimates of net usage are almost identical to those discussed earlier (compare with column 1 of Table 4). The major difference in the two sets of results is that information from the census of sleeping patterns was only available for about 85% of members (most missing data are due to the temporary absence of the member the previous night). The similar usage rates also reflect very strong concordance about usage at the individual level. The correlation between the two independently measured indicators of net usage is 0.94. While it is possible that respondents misreported similarly on both sets of responses, the remarkable degree of consistency across sections makes it unlikely. Note also that such concordance is not simply due to the respondent reporting every household member as either using or not using nets the night before. Although most of the variation in net usage is between and not within households (the intra-household correlation of usage is about 0.75), the correlation between the two separate reports is still very high (0.87) if we use only information from households where there is intra-family variation in usage. Overall, these findings suggest that net usage the night before the survey was accurately measured. Of course, such measures are very noisy indicator of consistent usage, and there also remains the possibility of systematic over-reports of regular usage during the peak malaria season.

Next, we use information from the census of sleeping patterns to probe further the actual presence of the nets mentioned by the respondent. In a large majority of cases the surveyor was allowed to check the net (column 2): this happened 85% of the times in control areas, and even more frequently in MF (89%) and Free villages (93%). Overall, then, usage rates of nets observed by the surveyors are only slightly lower relative to the reports unconditional on observation (column 3). The estimates in columns 4 and 5 are also consistent with our intervention having improved the availability and quality of bednet protection. In

control areas, only 4.4% of individuals were using a net in good conditions, that is, a net without sizeable holes or tears. The proportion was 9.3 percentage points higher in MF villages and a remarkable 29.3 percentage point higher in Free communities. This was clearly due to usage of program nets, which were of good quality and, at that point in time, relatively new. In column 5 we see indeed that virtually no BISWA nets were present in control areas, which also confirms the absence of cross-arms contamination. In MF villages, 13% of individuals used program nets the previous night, nets that were certainly with the household at the time of the survey. This figure correspond to about half of all nets seen by the surveyors (compare with column 3). In villages with free distribution, 47% of individuals were reported as having used a BISWA net the night before and this accounted for almost all the nets observed.[36]

Finally, the results in column 6 show that a very small fraction of the nets were seen hanging properly within the dwelling. This finding is only apparently at odds with the high usage rates we have just discussed, given that interviews were done during the day, when nets are usually stored away to avoid being damaged and increase living space in what are often small dwellings. In the end, our analysis strongly suggest that the intervention did increase considerably both the availability of nets (especially with free distribution) and net usage, although our data cannot rule out that usage was, in fact, not as consistent as indicated by respondents.[37]

4.4 Coverage of Net Distribution

One more element that could explain the lack of health benefits is that, even in villages with free distribution of nets, the intervention was never close to achieving full coverage of all sleeping spaces in the target villages. This likely limited the extent of externalities which are thought to be key for ITN efficacy (Hawley et al. 2003). In fact, recall that only BISWA clients received the free ITNs or the offer of ITNs for sale on credit. Although BISWA has an important presence in the study area, we estimate that on average only ∼20% of people live in households with at least one BISWA affiliate.[38] Is it possible, then, that the increase in ITN ownership and usage rate in study villages was not sufficiently large to dent the cycle of malaria transmission? To address this question we first look at the relationship between village-level coverage and malaria prevalence and, next, we interpret the results in light of the large existing literature on the relationship between ITNs and malaria burden.

As a first step, we estimate, for each intervention village, malaria prevalence using all tests completed during the follow-up survey. We exclude control villages because no ITNs were distributed there. We then plot the results against the village-specific ratio between the number of ITNs distributed during the

[36] Using the estimates in column 3, the fraction of individuals reported as having slept under a net seen by the surveyor was 0.144+0.127=27.1% in MF villages and 0.144+0.360=50.4% in Free villages.

[37] That a high fraction of nets were retained by households is also confirmed by the high re-treatment rates, especially in Free villages, that we documented in Section 3.3.

[38] We estimate the fraction using village population from the 2001 census of India, together with estimates of the total number of individuals living in households with at least one BISWA member. Let $\hat{s}_v$ and $\hat{b}_v$ denote respectively average household size and average number of BISWA affiliates in BISWA households in village v, both estimated using baseline survey data. Let also m_v be the number of BISWA members in the village, as provided by the micro-lender. Then, if we denote by p_v the village population from the census, our estimate of the fraction who lives in BISWA households is $\hat{s}_v(m_v/\hat{b}_v)/p_v$.

intervention in fall 2007 and total village population. Because the study design did not include a complete village census, we use population figures from the 2001 Indian Census. Such figures are not up-to-date, but should be a very good proxy for current population. Also, demographic growth likely means that in most cases 2001 population will underestimate current population, so that our estimates of ITN coverage (which, we will show, are quite low) may actually overestimate true coverage. The total number of ITNs distributed in each village come from intervention records, and are not estimated from household survey data. The two panels at the top of Figure 8 show the plot of malaria prevalence vs. ITN coverage rates. In the two bottom panels we repeat the exercise for the change in prevalence relative to baseline. Each graph also shows the fitted values of two OLS regressions, one where we include data from all villages (the thicker line) and the other where we exclude the very few villages where the ITN coverage ration was larger that 0.35 (the thinner line with the + pattern).

The graph in panel A shows that the *positive* association between malaria prevalence at follow-up and program coverage is driven by three outlier villages with coverage > 0.35. When we include all villages, the estimated slope is positive (0.56) and significant (p-value = 0.000), but when we exclude the three outliers the slope decreases to 0.12 and is no longer significant at standard levels (p-value = 0.264). When we look at mean changes in malaria prevalence over time (panel C), the results are similar: the slope is positive and significant with the full sample but it becomes smaller and not significant when we exclude the three outliers. When we look at MF villages, where the relative number of ITNs distributed was even lower, the slope of the regressions are never significant at standard levels and in three of four cases they are negative. Of course, the ITN coverage achieved in MF communities was endogenously determined by household purchase decisions, so that the association with malaria prevalence (or its change) should not be interpreted as necessarily causal. On the other hand, in communities with free distribution, the number of nets delivered was decided by our research team based on household size and composition. By construction, this produced variation in ITN coverage resulting only from the distribution of BISWA affiliation and household composition within the community. Even so, BISWA affiliation may be associated with village characteristics related to malaria prevalence.[39] On the one hand, the results of the DD regressions displayed in panel C eliminate any possible spurious correlation due to time-invariant village-level characteristics, regardless of whether they are observed or unobserved. On the other hand, we cannot exclude the presence of other unobserved differences in trends correlated with both ITN coverage and malaria prevalence. Despite these caveats, we can at least say that, overall, the coverage of the intervention does not appear to be systematically correlated with either the levels or the changes in malaria prevalence.

The next question is then whether we can reconcile our results with the several earlier RCTs that have documented large reductions in malaria burden following distribution of ITNs. We find that the unique features of our study make comparisons with other studies difficult, but that perhaps these very differences in features hold the key to explain the different findings. A first and possibly important difference is that our study, to the best of our knowledge, is the only large-scale cluster RCT ever carried out in India to evaluate the impact of ITNs. Of the 22 RCTs considered in detail in the influential survey in

[39] Note, however, that if we regress malaria prevalence *at baseline* on ITN coverage the slope is close to zero (0.03) and not significant (p-value = 0.720).

Lengeler (2004), not one was carried out in India, and only four were located in Asia (one in Iran, one in Pakistan and two in Thailand). It is well-known that optimal strategies for malaria control also depend on characteristics of the local mosquito population such as the prevalence of different *Anopheles* species and their feeding and resting patterns (Pates and Curtis 2005). Such characteristics can differ widely across different geographical areas. On the one hand, our study did not include an entomological component, so that we do not have direct measurements about the local vector population. Similarly, our study is silent about any change in the number, relative frequency or behavior of the local *Anopheles* population that may have resulted from the intervention. On the other hand, recent studies in locations proximate to our study areas have documented both the insecticidal efficacy of the chemical we used for treating nets (Yadav et al. 2001, Sharma et al. 2004, Sharma et al. 2006), and that feeding of local malaria vectors appears to be concentrated during the sleeping hours (Sahu et al. 2009).

The low ITN coverage achieved by our program, even in Free areas, is a second marked difference relative to other studies. Of the 22 RCTs surveyed in Lengeler (2004), fourteen are cluster RCTs. In all but one of them, the number of ITNs distributed was sufficient to ensure close to full coverage for the target population in treatment communities. For instance, in the largest-scale cluster RCT, nets in 113 of 221 communities in Kenya were delivered in number sufficient to cover all sleeping spaces (ter Kuile et al. 2003). In the RCT in Ghana analyzed in Binka et al. (1996), ITNs were supplied in quantities adequate to cover all sleeping spaces in half of 96 communities. Nevill et al. (1996) describes a study where ITNs in half of 56 zones in Kenya were issues to 96% of beds listed during a census. The one exception among the surveyed articles is the cluster RCT described in D'Alessandro et al. (1995). In this study, despite re-treatment with permethrin being attempted with all bednets in half of 104 villages in The Gambia, only ∼60% of children under the age of four slept regularly under ITNs. Interestingly, the program was only unsuccessful in reducing child mortality in one of five strata where coverage and usage rates remained relatively low. The results illustrated in Figure 8 show that our study differs markedly from these earlier studies, because our program never achieved more than 50% coverage and in most cases covered a significantly lower share of the population. This leads us to conjecture that the low coverage may have played an important role in the absence of health benefits. It must be kept in mind, however, that our data are not suited to test this hypothesis formally. In the presence of non-linearities in the relationship between ITN coverage and health benefits, it is possible that such benefits arise only beyond certain thresholds in terms of coverage. The existence of non-linearities is indeed strongly suggested by some of the earlier studies that documented the existence of large externalities of ITNs. Gimnig et al. 2003 and Hawley et al. 2003 found that the number of *Anopheles* mosquitoes as well as rates of parasitemia, anemia and mortality were significantly lower in children who did not use nets but lived within 300 meters of an intervention village with close-to-full ITN coverage. However, no benefits for compounds near intervention areas were found when ITN coverage was less than 25%. It is possible that such thresholds are also important for the protection of ITN users, and not only for non-adopters. Of course, if the threshold lies beyond the range of ITN coverage achieved in our study, even in Free areas, our conjecture is perhaps reasonable but it is still based on an "out-of-sample" prediction.

Perhaps, then, a better comparison for our study could be provided by the eight studies reviewed in

Lengeler (2004) where the impact of ITNs was evaluated using a within-community randomization. In such studies, the design was such that the intra-community coverage rates was always low, and large benefits were observed nonetheless. However, in each of these other studies, the protocol also called for intense monitoring of net usage and/or health outcomes.[40] All these studies included a combinations of nightly surprise visits and frequent (sometimes daily) health checks. It is easy to imagine that such design induced behavioral responses by increasing compliance with regular ITN usage. To sum up, the lack of health benefits of our intervention are not necessarily in contrast with the large benefits documented by others, but may instead complement them.

5 Conclusions

Malaria is one of the world's deadliest infectious diseases, causing an estimated 300-660 million symptomatic cases and over 1 million deaths annually. Insecticide-treated bednets (ITNs) are widely considered an efficacious mean of reducing malaria-related morbidity and mortality, and are therefore a crucial component of most malaria-prevention public-health interventions. Multiple rigorous randomized controlled trials have demonstrated that high coverage and regular use of ITNs can successfully reduce the malarial burden—particularly among pregnant women and children—in a variety of geographical and epidemiological settings. However, despite powerful public and private advocacy, and despite vast progress made in different countries, adoption of ITNs in most malarious areas remains very low. In poor areas, individuals often do not purchase ITNs on the open market, and public health interventions frequently have insufficient resources to provide every individual at risk with a free or heavily subsidized ITN. Even when ITNs are in use, they are rarely retreated with insecticide, although retreatments are recommended every 6-12 months to maintain effectiveness. Long-lasting insecticide nets, which do not require re-treatment, are becoming the product of choice for most public health interventions, but such nets are still rarely available in most local markets.

The primary objective of this study was to evaluate whether sale on credit could increase ITN ownership and usage, and to decrease the burden of malaria in poor rural areas of Orissa (India) where existing markets and public health interventions have been unable to ensure adequate population coverage. With the help of BISWA, a micro-lender with widespread local presence, we completed a randomized controlled trial (RCT) in 141 communities where the micro-lender operated. In one third of villages (selected at random), we offered good-quality ITNs for sale to pre-existing BISWA clients, at full cost but allowing repayment over time. In another third of villages, we completed a distribution of ITNs provided at no cost, reaching a close-to-complete potential coverage among BISWA households in those communities. The records collected from ∼1,800 households at the time of the intervention were overall encouraging and in line with our expectations. The micro-loan program increased ITN ownership and usage rates relative to control areas, but significantly less than in areas with free distribution.

[40] All these eight studies were also carried out within relatively small geographical areas, with the exception of one where the study population was spread across one district.

We also find no evidence in favor of the hypothesis that, conditional on ownership, nets that have been purchased on credit are being used more than others received for free. The self-reported nature of usage data limits somewhat the strength of this result, but the finding is consistent with analogous conclusions found for ITNs in Kenya (Cohen and Dupas 2010) or water disinfectant in Zambia (Ashraf et al. 2008). One more interesting result, in communities where nets were sold on credit, is that regular re-treatment with insecticide was significantly higher among households who decided to "commit" to re-treat by purchasing ITNs bundles with a sequence of two-retreatments. The first re-treatment was more than twice as likely and the second re-treatment more than three times as likely when the bundle was chosen. Although contract choice makes the pre-commitment clearly endogenous, we also find that the gaps in re-treatment rates remain almost identical if we control for a large number of household characteristics which also include proxies for preference parameters. This finding has potentially important policy implications. In situations where public health programs call for cost-sharing and also require compliance with certain behaviors over time—such as re-treatment of non-LLINs—including any monetary costs of such behavior up front may increase compliance. Despite its non-experimental nature, we think this result deserves to be examined further in future research.

Finally, perhaps the most surprising result of the study is that we found no improvements in either malaria prevalence or hemoglobin levels, not even among households which received nets for free. We rule out several potential explanations for the lack of benefits. Specifically, our findings do not appear to be the consequence of behavioral changes among beneficiaries, differential attrition and consent to being tested, poor measurement of health outcomes, or low retention of ITNs. Instead, we conjecture that the findings are likely explained by a combination of two factors. First, low coverage with ITNs at the village level, even in communities with free provision of nets. The low coverage resulted from our program targeting only households affiliated to BISWA, the micro-lender we partnered with. Second, our study design did not include monitoring of ITN usage, and did not call for frequent measurement of health outcomes. Both these features are in stark contrast with the earlier studies surveyed in Lengeler (2004) that have documented large benefits of ITNs on health outcomes. We emphasize that low coverage, coupled with low or no monitoring, are likely to mimic more closely actual public health interventions relative to studies carried out under ideal trial conditions. Unfortunately, although we argue that our conjecture is reasonable, our data do not allow us to establish conclusively whether it is correct, because our study design did not generate sufficient variation along study areas along the two dimensions of monitoring and coverage.

Importantly, the unique features of our study design imply that our results should *not* be interpreted as contradicting earlier studies, whose results on the efficacy of ITNs we find compelling. That our findings may instead be an important complement to the literature is indeed consistent with the view expressed in Lengeler (2004) (p. 10) when he wrote that

> [t]he results presented in this review are from randomized controlled trials where the intervention was deployed under highly controlled conditions, leading to high coverage and use rates. [...] Therefore, the bulk of data in this review describe impact under ideal trial conditions (efficacy) rather than impact under large-scale programme conditions (effectiveness). While the

difference between efficacy and effectiveness is likely to be small for certain medical interventions (such as vaccination or surgery), it can potentially be large for preventive interventions such as ITNs.

Far from suggesting that ITNs are not useful to combat malaria, we take our results to suggest that half-hearted public health interventions that only call for the distribution of a relatively limited number of nets may fail to achieve the desired effects. Much more may be needed, from ensuring close-to-complete coverage, to providing incentives to regular use, to adding complementary interventions such indoor residual spraying, case control and treatment and water management. Otherwise, in the words of Hawley et al. 2003 (p. 126) "low levels of coverage with treated nets or, worse, untreated or poorly treated nets, may do little but fritter away scarce resources".

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A Appendix

A.1 Comparison of Sample Villages and Overall Study Districts

The villages included in our sample at baseline and/or after the intervention were selected from a list of 878 villages where BISWA operated in 2007. It is therefore interesting to evaluate the characteristics of communities in our sample relative to other communities in the five study districts. For this purpose, we use data from the 2001 Census of India, which also includes a broad series of village-level characteristics for the whole population of 8,991 villages in the study districts. The data used in this paper have been collected in 2007-09, but the time gap relative to the 2001 census is short enough that a comparison between sample and non-sample villages should be informative.

Table A.10 show means for a series of village characteristics, calculated for non-sample villages and for each of the three experimental arms. The figures in the last two columns are p-values for the test of equality of means across all the four village groups (column 6) or across the three study groups (column 7). The null of equality between non-sample and sample villages is strongly rejected for most of the listed variables. Sample villages are relatively large (both in terms of area and population), with mean total population more than twice as large as in non-sample villages. Sample villages also appear to be closer to towns, although not to a large extent. Mean distance from the nearest tows is 35 kilometers among non-sample villages and only 1-10 kilometers less in sample villages. Amenities are overall significantly better in sample villages, as reflected, for instance, in the higher proportion of villages with schools, health centers, a post office, a telephone connection and electricity. Interestingly, sample villages are also characterized by significantly larger fractions of land devoted to rice cultivation. This may have implication on malaria prevalence, because rice fields are often an ideal breeding ground for larvae of *Anopheles* mosquitoes.

The results in column 7 are important, because they show that village characteristics are overall balanced among experimental arms. Recall that the randomization tests in Table 1 only evaluated balance in household-level characteristics among villages included at baseline. These result confirm that balance also existed for a large number of community characteristics. In a list of 26 variables, the test of equality across groups is only rejected once, at the 10% level.

A.2 Balance in Changes in Household Composition

The results in Table 2 showed that attrition rates were balanced across experimental arms. Here we analyze whether there is any evidence of differential changes in household composition. This is potentially important for two reasons. First, changes in actual availability of nets may also arise from changes in the number and age of households members. Second, we have documented that malaria and anemia prevalence at baseline differ across age and gender groups, so that changes in the demographic structure of the household may confound, in principle, aggregate changes in such health measures calculated over all household members. Our enumerators filled a complete household roster both at baseline and at follow-up, so that we can separately identify new members as well as individuals who left the household because of death or relocation.

We look first at entry into and exit from households. The tabulation in Table A.11 show that significant changes took place between baseline and follow-up survey. We find that 1,000 of 9,675 individuals are no longer present in the household, but we also find that new members are in similar numbers (916). About one-third of new members are temporary visitors. Overall, the fractions of members who are matched, new or no longer present are similar across treatment groups, and we cannot reject the null of equality (see the table notes for the details of the test). Next, in Table A.12 we analyze changes in the demographic structure of baseline households, again by experimental arm. Each row displays coefficients of a separate OLS regression estimated at the household level, where the dependent variable is the change—between baseline and follow-up—in the fraction of household members that belong to the row-specific age-gender group, while the regressors are dummies for the two intervention groups. The figures in column 1 show relatively small changes in control villages, with the coefficient largest in magnitude equal to -0.011 for

the proportion on males 45 and older. Overall, we find small but statistically significant increases in the mean fraction of U5s, counter-balanced by declines in individuals 45 years old and above. This pattern is broadly consistent with the presence of a relatively small number of births coupled with deaths of older members. The estimates in columns 2 and 3 show that changes were largely similar in intervention villages, although in some cases the differences in changes are statistically significant. When we look at significant coefficients we find that, relative to control communities, the decline in the proportion of older members is about one percentage point larger for males in MF communities and one percentage point smaller for females in areas with free distribution. We also find smaller increases in the fraction of U5 in MF villages, where actually the fraction of girls declined on average by 0.2 percentage points over the study period. Overall, the results in Tables A.11 and A.12 show that changes in baseline household structure were fairly balanced across arms. Even in cases where we can reject the null of equal differences in changes among experimental groups, the differences are always small enough that none of the results described in the paper should depend on differential changes in household composition.

A.3 Correlates of Purchase Decisions

This section looks at correlates of ITN purchase among baseline households in MF villages. Table A.13 shows the results of a Linear Probability Model where the binary dependent variable = 1 if the household purchased at least one ITN (marginal effects calculated from a probit model are almost identical). Sample size is smaller than the 589 panel households in MF villages because 76 observations (13%) have at least one regressor missing. These results are obviously to be interpreted as descriptive, while they do not necessarily imply causal associations between the predictors and the decision to purchase. All covariates were recorded in the baseline survey which, as a reminder, was carried out approximately 4-5 months before the net sales took place. We include measures of expenditure, indebtedness with the micro-lender BISWA, demographic structure, ownership and usage of nets, proxies of risk aversion and time preferences, perceived protective power of nets and measures of past exposure to malaria. To reduce the influence of outliers, variables in Rs are transformed either in logarithms or, if zeros are present, using quartic roots.[41] Note that the study design does not allow the estimation of price elasticities, because the same products were offered at the same conditions in all communities. We omit some of the estimated coefficients from Table A.13 for brevity, but none of the non-reported slopes (listed in the table caption) are significant at standard levels.

Most of the predictors are not statistically significant at standard levels, and overall the model explains only 11% of the variance of purchase decisions. Demographic structure and head characteristics are not significant, either individually or jointly (p-value 0.6276). In particular, presence of U5s does not increase the probability of purchase. Interestingly, richer households (defined by higher monthly expenditure per head) are *less* likely to purchase nets, despite the fact that we control for the number and usage of pre-existing nets. A 10% increase in per capita expenditure predicts a 1.2% decrease in the probability of purchase, and the slope is significant at the 5% level. Also interesting is that usage of nets the night before the interview is one of the strongest predictors of purchase: everything else being the same, households where everyone used a net are 20 percentage points *more* likely to purchase nets relative others where no one did. None of the preference-related measures is significant. Somewhat surprisingly, the same is true for measures of respondent's perceived benefits of treated nets (indeed these measures enter the regression with the "wrong" sign). Also noteworthy is that measures of past exposure to malaria are strongly associated with the decision to purchase. An increase in the monetary cost of malaria episodes in the 6 months before the baseline interview from 0 to the median non-zero value (Rs 590), increase demand by 9 percentage points ($0.019 \times 590^{1/4}$). Households with malaria-related deaths in the previous five years are 10 percentage points more likely to purchase, although such events were rare (only nine respondents report any) and the coefficient is not significant. Finally, although anemia levels do not appear to influence demand, both self-reported malaria episodes and prevalence measured by our blood tests are instead among the strongest

[41] The quartic root has shape similar to the logarithm, but it is defined over zero.

predictors. The similarity of the coefficients is somewhat surprising, given that the two measures of malaria exposure are not only non-comparable but also virtually uncorrelated.[42] Moving from a household with no self-reported past malaria cases to one where every member had been sick increases the probability of purchase by 27 percentage points. Similarly, an increase from 0 to 100% in the fraction of blood tests positive for malaria increases predicts a 20 percentage points increase.

A.4 Post-intervention RDT Success Rates

In the post-intervention survey, all members of households re-contacted after the baseline were targeted for blood tests. Our testers were able to successfully test 75% of members in panel households, while 19% could not be tested because they were not present at the time of the visits and only 6% because consent was not given. (see columns 1 and 4 in Table A.14). The figures in columns 2 and 5 of the same table show that absence and refusal were almost identical across experimental arm, which is reassuring. However, we also find important differences in testing success across different age groups (columns 3 and 6). Almost one third of adult (15-45) males (the omitted category) could not be tested because of absence during the visits, most probably because prime-age males were more likely to be off to work. Testing rates among all other demographic groups were significantly higher, especially among U5 of either gender and among women 15 years old and above. For these groups testing rates were close to 90%. The testing rates are very close between boys and girls, and the null of equality between genders cannot be rejected for both U5 and 5 to 15 year old children. We find instead some evidence of gender differences across age groups in refusal rates, which are highest among women over 45 (8%) and girls U5 (9%). Refusal rates are 3 percentage points lower among U5 boys, but the null of equality between gender cannot be rejected at standard significance levels.

[42] The correlation is actually negative (-0.02), although close to zero and not significant at standard levels (p-value= 0.5732). Recall that self-reported malaria cases were measured for all members, while blood was drawn only from a subset of them.

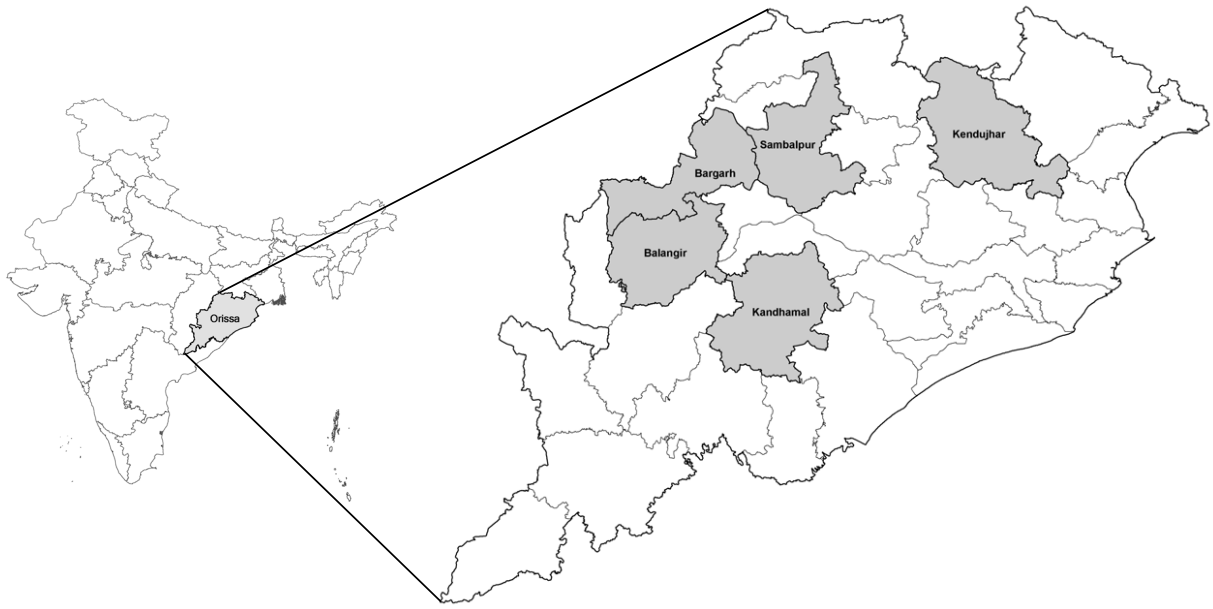

Figure 1: Study Areas

Notes: Study communities include 30 villages in Sambalpur, 9 in Kandhamal (Phulbani), 30 in Keonjhar, 33 in Balangir and 48 in Bargarh.

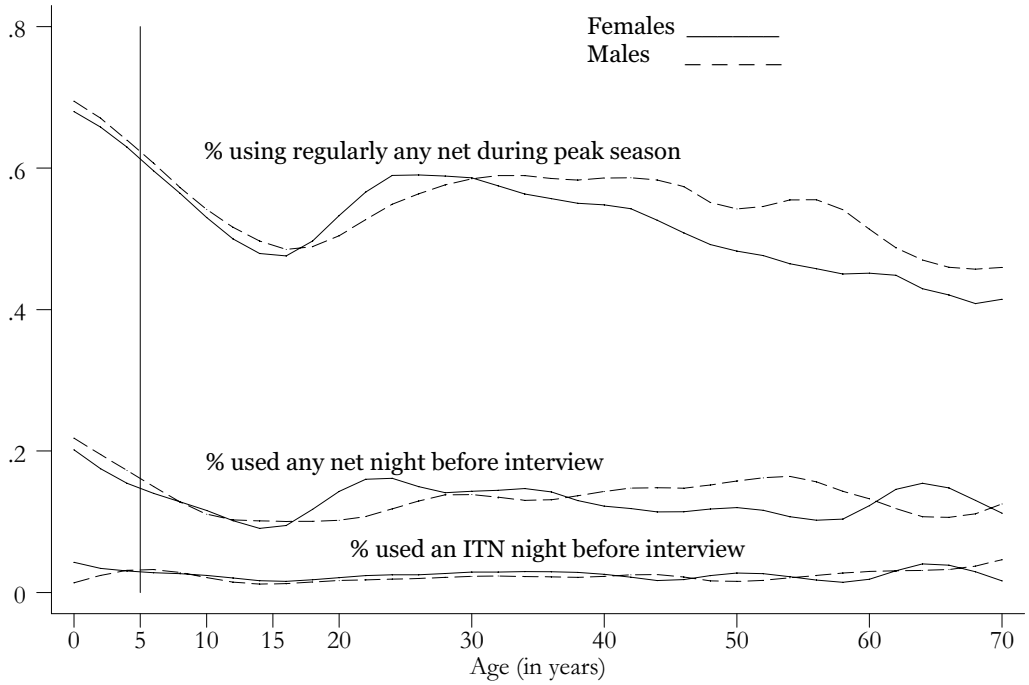

Figure 2: Net Usage at Baseline, by Age and Gender

Notes: Each line is a locally linear non-parametric regression of usage on age (in years). Regressions are estimates using a bi-weight kernel with bandwidth equal to 7. The sample includes information from all members at baseline of the 1,768 panel households. Sample sizes for females and males respectively are 4,738 and 4,747 for usage in peak season, 4,529 and 4,459 for usage of any net the previous night, and 4,507 and 4,439 for usage of ITNs the previous night.

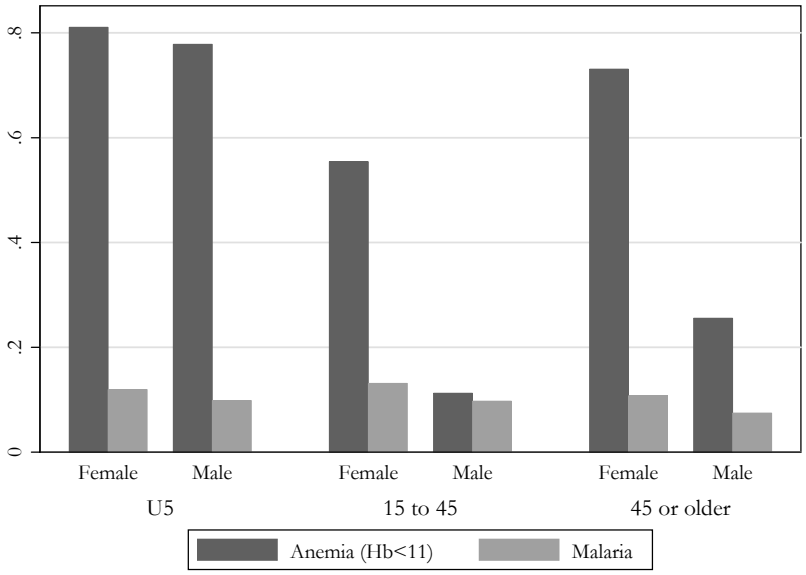

Figure 3: Biomarkers, by Demographic Group

Notes: The columns represent the results of RDTs for anemia ($n = 2,532$), malaria ($n = 2,561$) and lymphatic filariasis ($n = 1,590$) prevalence.

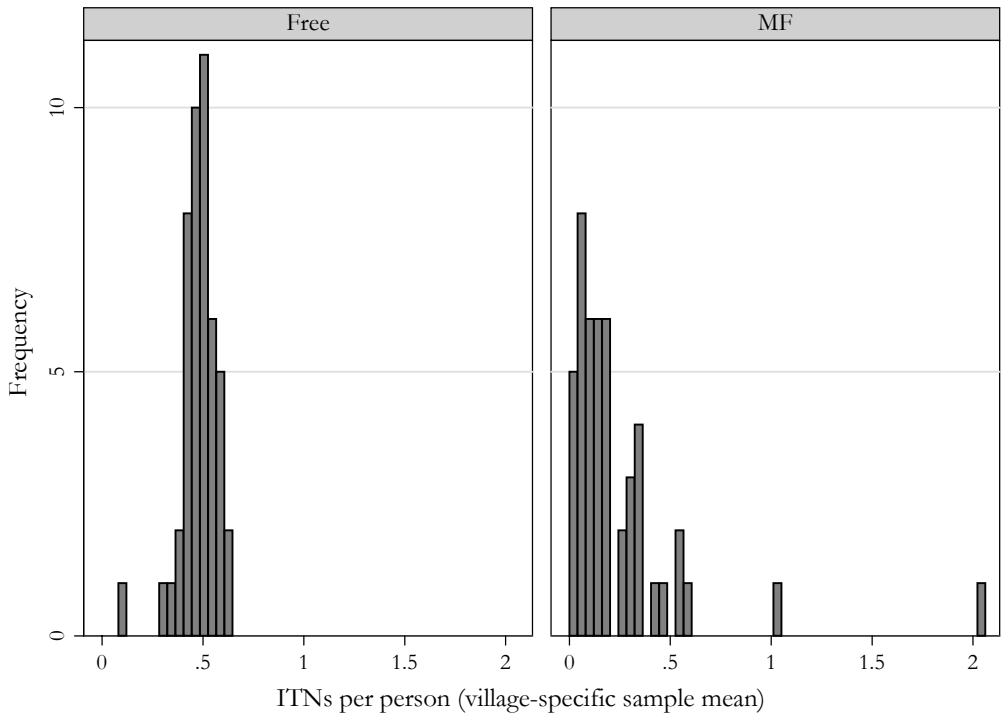

Figure 4: Mean Number of ITNs delivered per Head.

Note: Data from fall 2007. Each histogram represents the distribution across villages of the mean number of ITNs delivered per capita by our intervention. Each intervention group includes 47 villages. The lowest take-up in Free villages is due to a relatively large number of sample households not present during the visit. The two outliers among MF communities are due to the decision of local BISWA clients to purchase a large number of nets for resale.

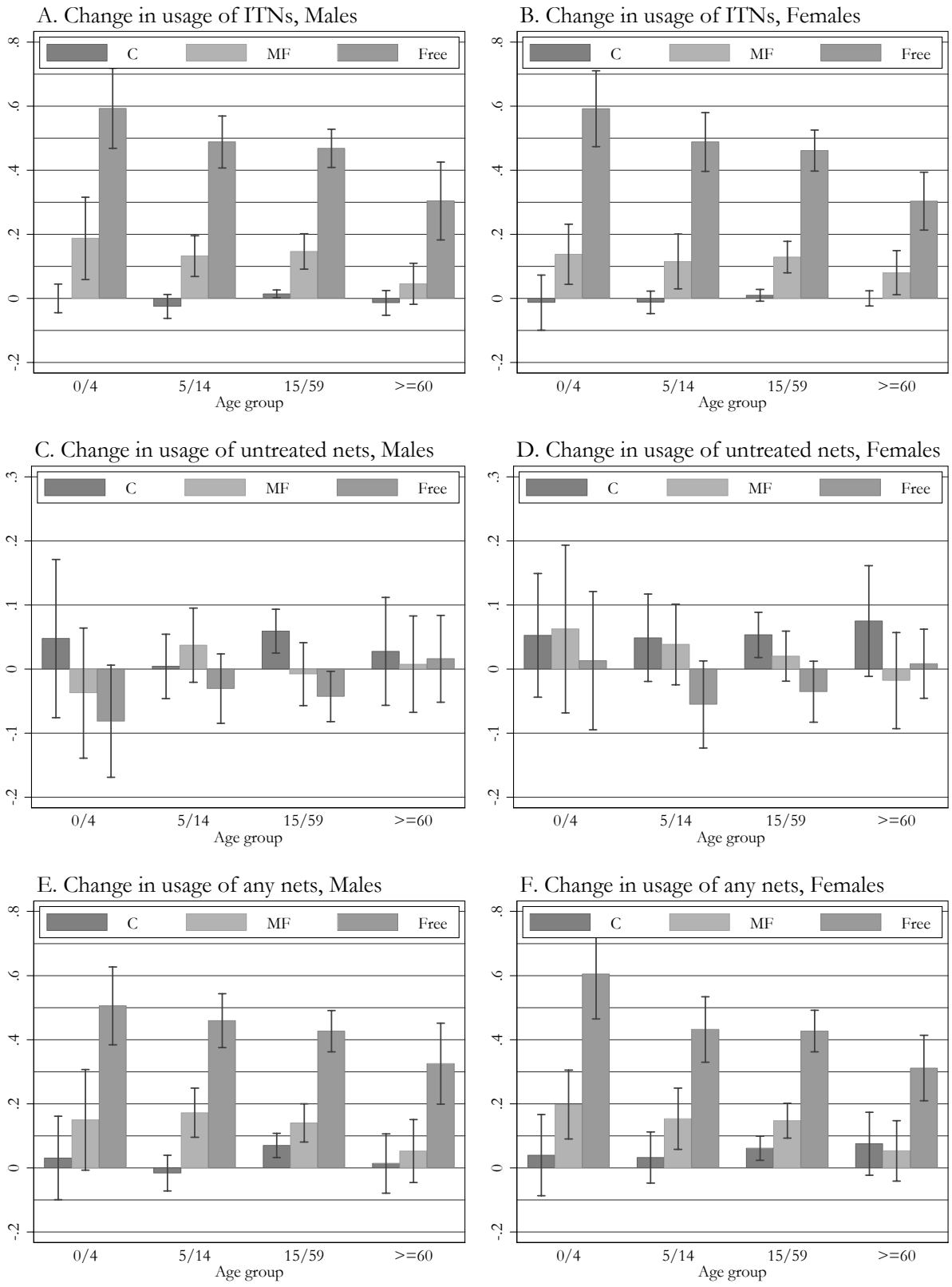

Figure 5: Changes in previous night usage of nets, by Age and gender

Notes: Each column shows the change in the fraction of household members in a specific age-gender group who slept protected by a given type of net the night before the interview, by experimental arm. Each column also displays 95% confidence intervals, robust to intra-village correlation. By construction, the changes are calculated only for individuals who were part of the household both at baseline and at follow-up.

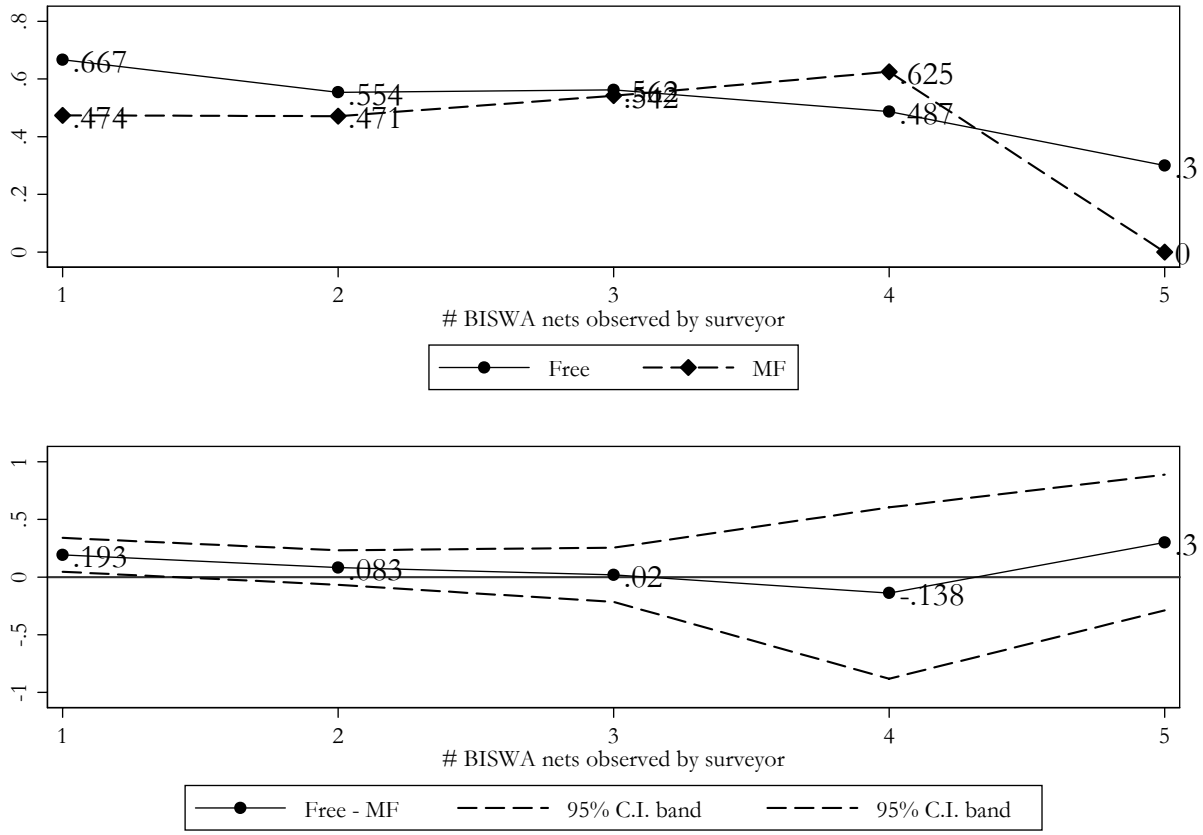

Figure 6: Fraction of observed BISWA nets used the previous night

Notes: Data from post-intervention survey (winter 2008/09). 95% confidence intervals are robust to intra-village correlation (see text for details).

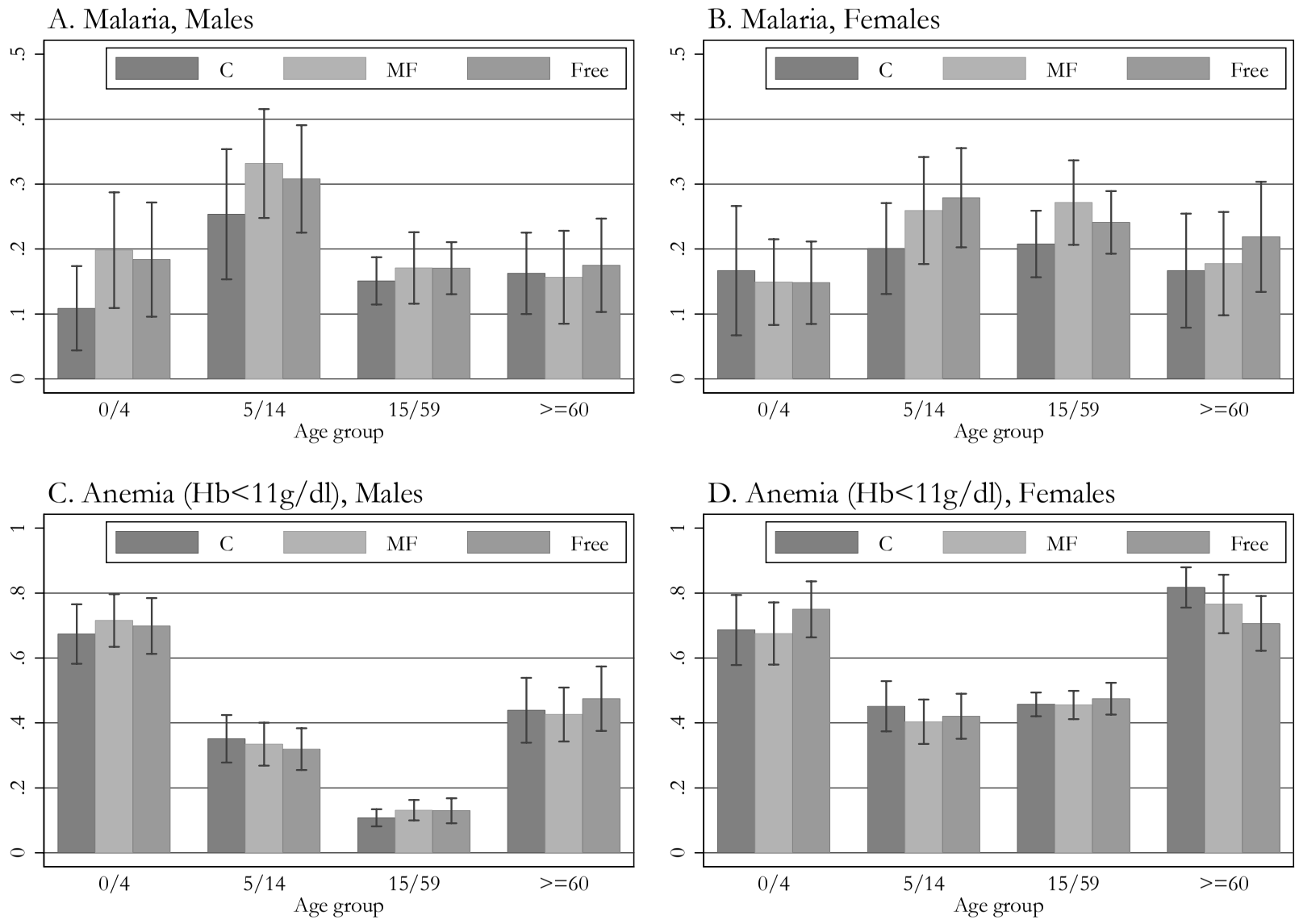

Figure 7: Post-intervention biomarkers, by Age and gender

Notes: Columns show anemia or malaria prevalence in the specific age-gender group, by experimental arm. Each column also displays 95% confidence intervals, robust to intra-village correlation.

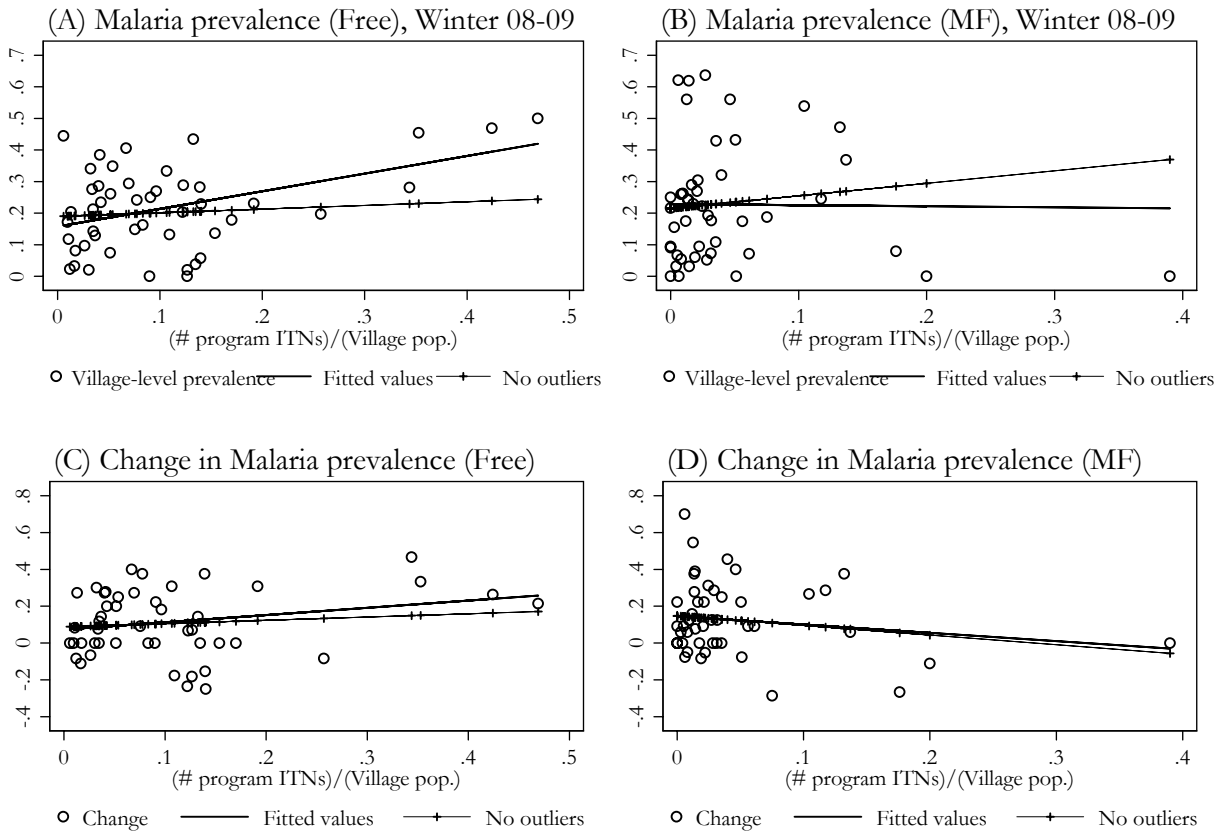

Figure 8: Malaria Prevalence vs. Intensity of ITNs Distribution

Note: Data from winter 2007 and winter 2008/09. Each circle in the graphs represents a village. Each graph also shows fitted values of two village-level OLS regressions of prevalence (or its change) on ITN coverage. The dotted lines are fitted values when we exclude villages with coverage larger than 0.35 (corresponding to the vertical line). The point estimates and heteroskedasticity-robust standard errors (in parenthesis) of the regressions, using all villages or excluding outliers respectively, are as follows: (A) 0.56 (0.13) and 0.12 (0.19); (B) -0.03 (0.46) and 0.40 (0.70); (C) 0.39 (0.18) and 0.18 (0.56); (D) -0.45 (0.41) and -0.52 (0.76).

Table 1: Summary Statistics and Randomization Tests

	(1) Control		(2) Free		(3) MF		(4) p-value	(5) s.dev.
SC/ST/OBC	0.9	(0.013)	0.933	(0.013)	0.912	(0.021)	0.421	0.256
Household size	5.5	(0.103)	5.6	(0.117)	5.3	(0.086)	0.138	2.22
Male (%)	0.499	(0.007)	0.512	(0.007)	0.511	(0.006)	0.296	0.704
Age	27.8	(0.385)	27.4	(0.357)	27.9	(0.324)	0.495	0.235
No. children U5	0.499	(0.033)	0.506	(0.030)	0.487	(0.026)	0.892	0.452
Male household head	0.952	(0.009)	0.941	(0.011)	0.932	(0.010)	0.368	0.287
H. Head has some schooling	0.72	(0.018)	0.706	(0.027)	0.714	(0.021)	0.908	0.476
H. Head completed secondary education or above	0.084	(0.016)	0.075	(0.013)	0.114	(0.015)	0.123	0.154
Expenditure per Head (daily)	22.3	(0.928)	21.2	(0.827)	24.2	(1.101)	0.085	7.9
Expenditure per Head (daily, excl. ceremonies etc.)	19.1	(0.845)	18.6	(0.730)	20.6	(0.779)	0.157	16.2
Debt/total yearly expenditure	0.485	(0.081)	0.435	(0.061)	0.416	(0.049)	0.769	1.06
Household has at least one net	0.654	(0.030)	0.628	(0.029)	0.68	(0.023)	0.373	12.9
Nets (per capita)	0.287	(0.020)	0.264	(0.018)	0.311	(0.018)	0.167	0.3
ITNs (per capita)	0.021	(0.006)	0.046	(0.013)	0.055	(0.014)	0.027	0.146
Used net last night (%)	0.131	(0.022)	0.116	(0.019)	0.162	(0.017)	0.195	0.295
Used ITN last night (%)	0.019	(0.006)	0.022	(0.007)	0.03	(0.010)	0.617	0.134
Use regularly nets during “mosquito season” (%)	0.564	(0.032)	0.512	(0.030)	0.572	(0.028)	0.304	0.453
Malaria prevalence (RDT)	0.108	(0.016)	0.116	(0.018)	0.123	(0.018)	0.841	0.275
Hemoglobin	11.0	(0.087)	10.7	(0.096)	11.0	(0.087)	0.132	1.64
Anemia prevalence (Hb< 11, RDT)	0.527	(0.024)	0.569	(0.025)	0.504	(0.020)	0.121	0.418

Source: Data from 1844 households included in the pre-intervention household survey (April-May 2007). Notes: Per-capita statistics are weighted by household size. For each variable, columns 1-3 show the experimental arm-specific means and the corresponding standard errors, adjusted for intra-village correlation. Column 4 reports p-values for a test of the null hypothesis that the means are identical across the three experimental arms. Column 5 contains the standard deviation of the variable calculated over the whole sample. The figures for debt indicate the mean ration of total household debt over the all-inclusive estimate of yearly total household expenditure.

Table 2: Attrition between Pre and Post Intervention Household Surveys

Dependent variable: Dummy = 1 if household was not re-interviewed at follow-up	(1)	(2)	(3)	(4)
Constant	0.041	0.05	0.2	0.173
	[0.005]***	[0.013]***	[0.108]*	[0.109]
Free		-0.023	-0.022	-0.021
		[0.014]	[0.014]	[0.013]
Micro-loans		-0.003	-0.001	0.004
		[0.015]	[0.015]	[0.015]
log(monthly exp./hhsize)			0.011	0.014
			[0.012]	[0.011]
# household members			-0.002	-0.001
			[0.002]	[0.002]
Access to electricity			0.011	0.011
			[0.010]	[0.010]
BISWA Debt/(Total yearly expenditure)< 0.05			-0.01	-0.021
			[0.016]	[0.017]
BISWA Debt/(Total yearly expenditure)> 0.25			-0.006	-0.012
			[0.022]	[0.022]
Baseline bednets per head			-0.018	-0.035
			[0.023]	[0.021]
% Slept under net last night			-0.009	0.002
			[0.016]	[0.017]
% Sleeps regularly under net			0.001	0.009
			[0.017]	[0.017]
Household head is male			0.008	0.025
			[0.019]	[0.017]
Household head's age (log)			-0.05	-0.053
			[0.019]***	[0.020]***
Household head had any schooling			-0.024	-0.029
			[0.013]*	[0.012]**
% malaria +ve in household				-0.005
				[0.013]
% anemic (Hb< 11) in household				0.005
				[0.011]
Observations	1844	1844	1814	1645
R-squared	0	0	0.01	0.02
H_0 : all coefficients = 0 (p-values)		0.11	0.21	0.14

Notes: OLS estimates. Standard errors (in brackets) are robust to intra-village correlation. Asterisks indicate significance at the 10 (*), 5 (**) and 1% (***) level. All regressions include observations from 141 clusters (villages). The smaller sample size in columns 3 and 4 is due to missing values in one or more regressors.

Table 3: Results: Net Ownership

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ITN Take up (Fall 2007)				Net ownership (Winter 08/09)			
Dependent variable	ITNs delivered	Any ITN Delivered	ITNs delivered >0 only	ITNs delivered (per capita)	Nets owned	Nets owned (per capita)	Nets owned DD	Nets owned DD (per capita)
Free	2.65	0.96	2.77	0.52	1.46	0.27	1.56	0.28
	(0.07)	(0.02)	(0.05)	(0.01)	(0.163)	(0.027)	(0.109)	(0.023)
MF	1.19	0.52	2.28	0.24	0.66	0.15	0.57	0.11
	(0.21)	(0.05)	(0.32)	(0.04)	(0.161)	(0.029)	(0.106)	(0.023)
Intercept (Control)					1.89	0.36	0.30	0.07
					(0.119)	(0.019)	(0.072)	(0.015)
Difference: Free − MF	1.46	0.43	0.49	0.27	0.80	0.13	0.99	0.17
p-value (H_0 : MF=Free)	0.0000	0.0000	0.1275	0.0000	0.0000	0.0000	0.0000	0.0000
Observations	1199	1199	894	1199	1767	1767	1759	1759
R-squared	0.55	0.81	0.65	0.55	0.11	0.12	0.12	0.10
no. clusters	94	94	89	94	141	141	141	141

Notes: Standard errors (in parenthesis) are robust to intra-village correlation. In columns 1 to 4 the dependent variables refer to the number of nets delivered during the intervention (fall 2007). In column 2, the dependent variable is binary and equal to one if the household received at least one net during the intervention. The regressions in columns 5 to 7 refer to all bednets owned by households as measured during the follow-up survey (winter 2008-09).

Table 4: Results: Net Usage in Panel Households

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Previous night						Usual in peak season			
Dependent variable	Any net	Any net	ITN	ITN	Untreated net	Untreated net	Any net	Any net	ITN	Untreated net
Free	0.358	0.378	0.447	0.46	-0.086	-0.084	0.268	0.332	0.708	-0.437
	[0.038]***	[0.036]***	[0.030]***	[0.031]***	[0.025]***	[0.026]***	[0.034]***	[0.037]***	[0.031]***	[0.039]***
MF	0.125	0.09	0.14	0.126	-0.013	-0.037	0.173	0.179	0.296	-0.12
	[0.038]***	[0.034]***	[0.024]***	[0.026]***	[0.030]	[0.026]	[0.037]***	[0.036]***	[0.037]***	[0.046]**
Intercept (Control)	0.176	0.05	0.022	0.003	0.149	0.049	0.659	0.089	0.064	0.59
	[0.025]***	[0.019]***	[0.006]***	[0.007]	[0.023]***	[0.016]***	[0.032]***	[0.022]***	[0.014]***	[0.032]***
DD	no	yes	no	yes	no	yes	no	yes	no	no
Difference: Free − MF	0.23	0.29	0.31	0.33	-0.07	-0.05	0.09	0.15	0.41	-0.32
p-value (H_0 : MF=Free)	0.0000	0.0000	0.0000	0.0000	0.0015	0.1046	0.0001	0.0003	0.0000	0.0000
Observations	9037	7707	8986	7647	8986	7647	9454	8442	9317	9317
R-squared	0.099	0.091	0.203	0.199	0.015	0.007	0.08	0.061	0.353	0.145
no. clusters	141	141	141	141	141	141	141	141	141	141

Notes: Data from spring 2007 (baseline) and winter 2008-09 (follow-up). Panel households only. Standard errors (in brackets) are robust to intra-village correlation. Asterisks indicate significance at the 10 (*), 5 (**) and 1% (***) level. All figures are OLS estimates. Columns 1, 3, 5, 7 and 9 (DD=no) report estimates of model (2), where we include information from all household members listed at follow-up in panel households. Columns 2, 4, 6, 8 and 10 (DD=yes) report differences-in-differences estimates of model (3), where observations include only members of panel households present both at baseline and at follow-up, which explains the smaller sample sizes in these regressions. Missing values are responsible for the other differences in sample sizes: mostly, in some cases the respondent did not know if the net being used the previous night was treated or not, and in other cases net usage the previous night was not known while regular usage during the peak season was.

Table 5: Retreatment Rates

	First re-treatment		Second re-treatment	
	(1)	(2)	(3)	(4)
MF, "Commitment contract" (C2)	-0.08	-0.007	-0.086	-0.094
	[0.030]***	[0.034]	[0.054]	[0.063]
MF, C1	-0.557	-0.582	-0.616	-0.66
	[0.059]***	[0.058]***	[0.059]***	[0.068]***
Intercept	0.92	0.936	0.83	0.907
	[0.013]***	[0.012]***	[0.016]***	[0.015]***
Exclude households who no longer have nets	No	Yes	No	Yes
Observations	875	696	875	681
R-squared	0.346	0.382	0.293	0.314
Clusters	89	88	89	88

Notes: Data from first (spring 2008) and second (winter 2008) re-treatment of bednets in Free and MF communities only. OLS regressions with standard errors (in brackets) robust to intra-village correlation. The dependent variable is the household-specific ratio between treated nets and total nets delivered during the intervention. Asterisks denote significance at the 10 (*) 5 (**) and 1% (***) level. Estimates in columns 2 and 4 exclude households where at least one net was reported as having been sold, stolen, lost or otherwise not available for retreatment.

Table 6: Results: Impact on Health

	(1)	(2)	(3)	(4)	(5)	(6)
	+ve Malaria		**Hemoglobin**		**Anemic (Hb< 11g/dl)**	
	Follow-up	DD	Follow-up	DD	Follow-up	DD
Free distribution= 1	0.037	0.054	-0.033	0.222	0.01	-0.024
	[0.030]	[0.040]	[0.105]	[0.107]**	[0.022]	[0.033]
Micro-loans= 1	0.044	0.063	0.023	0.046	0.005	0.035
	[0.035]	[0.039]	[0.094]	[0.123]	[0.021]	[0.035]
Constant	0.183	0.063	11.433	0.277	0.384	-0.111
	[0.022]***	[0.028]**	[0.064]***	[0.075]***	[0.012]***	[0.024]***
Only panel individuals	No	Yes	No	Yes	No	Yes
Observations	7154	1896	7149	1869	7149	1869
No. clusters (villages)	141	141	141	141	141	141
R-squared	0.0022	0.0037	0.0001	0.0036	0.0001	0.0021
Free=MF (p-value)	0.833	0.8289	0.6058	0.1568	0.8474	0.0937*
Free=MF=0 (p-value)	0.3538	0.228	0.8749	0.1025	0.9043	0.2437

Notes: Data from baseline (Spring 2007) and post-intervention household surveys (Winter 2008-09). All results are OLS estimates with individual observations. Standard errors (in brackets) are robust to intra-village correlation. Asterisks indicate significance at the 10 (*), 5 (**) and 1% (***) level. Estimates in columns 2, 4 and 6 (DD) only include tests from individuals tested both at baseline and at follow-up.

Table 7: Knowledge of Causes of Malaria and Risk Mitigating Behavior

	(1) Control	(2) Free	(3) MF	(4) Test of equality (p-values)
(A) Causes of malaria				
Drinking contaminated water	0.105	0.059	0.073	0.055
Mosquito bites	0.845	0.892	0.854	0.058
Contaminated environment	0.116	0.131	0.148	0.447
Don't know	0.037	0.025	0.051	0.065
(B) Malaria-avoiding behavior				
Nets	0.819	0.866	0.830	0.139
ITNs	0.023	0.023	0.017	0.718
Proper clothing (long sleeves etc)	0.004	0.008	0.010	0.268
Avoid drinking contaminated water	0.076	0.054	0.058	0.471
Insecticides	0.009	0.008	0.017	0.352
Repellents/mosquito coils	0.030	0.020	0.020	0.554
Smoke	0.016	0.023	0.022	0.622
Clearing stagnant water	0.028	0.021	0.022	0.702
Cleaning drainage system/sewage	0.054	0.075	0.087	0.093
Avoiding contaminated environments	0.158	0.170	0.211	0.151
Proper diet	0.051	0.039	0.037	0.618
Medicine	0.042	0.033	0.066	0.058
Other ways	0.035	0.021	0.027	0.469
Don't know	0.035	0.030	0.024	0.608
(C) Residual spraying of walls				
Inner walls sprayed in 2008-09	0.403	0.368	0.296	0.242
Outer walls sprayed in 2008-09	0.531	0.481	0.442	0.580
(D) Nets from other sources in the 12 months before the follow-up survey (per head)				
from Government/health centers	0.051	0.054	0.136	0.321
from NGOs other than BISWA	0.004	0.000	0.019	0.328
Purchased from the market	0.678	0.139	0.511	0.000

Notes: Data from July 2009. Only panel households are included ($n = 1,768$). The p-values in columns 4 are calculated for a test of the joint null hypothesis that means are identical across experimental arms.

Table 8: Impacts on Malaria Prevalence: Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)
	Base results		Controls for spraying		Blood Tester FE	
	Follow-up only	DD	Follow-up only	DD	Follow-up only	DD
Free=1	0.037	0.054	0.04	0.062	0.021	0.038
	[0.030]	[0.040]	[0.035]	[0.039]	[0.026]	[0.036]
MF=1	0.044	0.063	0.035	0.055	0.023	0.046
	[0.035]	[0.039]	[0.030]	[0.040]	[0.029]	[0.036]
Intercept	0.183	0.063	0.185	0.064	0.379	0.227
	[0.022]***	[0.028]**	[0.025]***	[0.031]**	[0.043]***	[0.047]***
Observations	7154	1897	7154	1897	7154	1897
R-squared	0.0022	0.0037	0.0051	0.0041	0.0467	0.0415
Clusters	141	141	141	141	141	141
Free=MF	0.833	0.8289	0.8893	0.8584	0.9502	0.8200
Free=MF=0	0.3538	0.228	0.3899	0.2407	0.6479	0.3971

Notes: Data from baseline (Spring 2007) and post-intervention household survey (Winter 2008-09). Standard errors (in brackets) are robust to intra-village correlation. Asterisks indicate significance at the 10 (*), 5 (**) and 1% (***) level. The results in columns 1 and 2 corresponds to the estimates in columns 1 and 2 of Table 6. In columns 3 and 4, Regressors also include dummies for inner walls having been sprayed in 2008/09, a similar dummy for spraying of outer walls and two dummies = 1 when information about spraying is missing for inner or outer walls respectively.

Table 9: Direct Observations of Nets Used the Night Before the Survey

	(1)	(2)	(3)	(4)	(5)	(6)
	Slept under a net	Surveyor was allowed to see the net	Slept under a net seen by surveyor	Slept under a net in good conditions, seen by surveyor	Slept under a BISWA net seen by surveyor	Slept under a net seen hanging properly by surveyor
Free	0.375	0.075	0.360	0.293	0.472	0.037
	[0.039]***	[0.043]*	[0.038]***	[0.024]***	[0.030]***	[0.011]***
MF	0.135	0.037	0.127	0.093	0.133	0.009
	[0.037]***	[0.046]	[0.036]***	[0.017]***	[0.022]***	[0.008]
Intercept	0.170	0.851	0.144	0.044	0.002	0.018
	[0.025]***	[0.041]***	[0.024]***	[0.010]***	[0.002]	[0.005]***
Observations	8018	2780	8018	8018	8018	8018
Clusters	141	128	141	141	141	141
R-squared	0.1077	0.0089	0.1049	0.1040	0.2406	0.0078
Free=MF	0.0000	0.1189	0.0000	0.0000	0.0000	0.0161
Free=MF=0	0.0000	0.0911	0.0000	0.0000	0.0000	0.0044

Notes: Data from post-intervention household survey (Winter 2008-09). Standard errors (in brackets) are robust to intra-village correlation. Asterisks indicate significance at the 10 (*), 5 (**) and 1% (***) level. All regressions are individual-specific and are estimated using only information about household members who slept in or around the house the night before the survey.

Appendix Tables

Table A.10: Sample Villages vs. Overall Village Population in Study Districts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Means, by village category				Villages	Tests (p-values)	
Area of Village (in hectares)	275.2	413.1	476.4	417.4	8991	0.000***	0.608
Number of Households	121.5	261.4	359.0	284.3	8991	0.000***	0.526
SC population (%)	0.134	0.164	0.164	0.173	8630	0.012**	0.921
ST population (%)	0.478	0.328	0.372	0.321	8630	0.000***	0.597
Females	0.501	0.497	0.496	0.499	8630	0.128	0.763
Primary school in village	0.746	0.936	0.979	0.936	8991	0.000***	0.432
Middle school in village	0.236	0.383	0.596	0.447	8991	0.000***	0.096*
Secondary school in village	0.129	0.319	0.404	0.298	8991	0.000***	0.523
hospital	0.002	0.000	0.021	0.000	8991	0.001***	0.312
Number of Primary Health Centre	0.025	0.106	0.064	0.064	8991	0.132	0.712
Number of Primary Health Sub Centre	0.105	0.170	0.234	0.213	8991	0.029**	0.727
Well Water	0.815	0.830	0.872	0.809	8991	0.692	0.678
Tank Water	0.557	0.702	0.723	0.745	8991	0.000***	0.899
River Water	0.120	0.106	0.170	0.149	8991	0.747	0.643
Canal	0.050	0.128	0.149	0.128	8991	0.034**	0.943
Number of Post Office	0.158	0.234	0.383	0.255	8991	0.003***	0.246
Number of Telephone connections	0.285	0.532	0.617	0.553	8991	0.000***	0.682
Bus services	0.228	0.255	0.298	0.298	8991	0.499	0.866
Number of Commercial Bank	0.027	0.064	0.064	0.085	8991	0.242	0.906
Number of Agricultural Credit Societies	0.027	0.085	0.106	0.106	8991	0.043**	0.919
Approach - Paved Road	0.332	0.383	0.426	0.362	8991	0.506	0.813
Distance from the nearest Town (in Kilometer(s))	34.9	34.3	25.2	26.1	8991	0.000***	0.445
Electricity for Domestic use	0.465	0.702	0.575	0.681	8991	0.000***	0.389
Electricity of Agricultural use	0.066	0.106	0.064	0.149	8991	0.346	0.386
Wet Rice (irrigated) cultivated Area (%)	0.075	0.151	0.188	0.183	8875	0.000***	0.727
Dry Rice (un-irr.) cult. Area (%)	0.422	0.504	0.483	0.510	8875	0.005**	0.864

Notes: The point estimates in column 1 indicate means in villages not included in the baseline sample, while estimates in columns 2 to 4 indicate means in villages that belong to the group indicated in the column header. The figures in column 6 are p-values for the null hypothesis that the mean of the variable indicated in the row header is the same across all four village groups. The p-values in column 7 are for the test of equality among the three experimental arms. Statistical significance is indicated as *** (1% level), ** (5%) or * (10%). All tests are heteroskedasticity-robust.

Table A.11: Changes in Household Membership

	Control	**Free**	**MF**	**Total**
Household and individual match	2,809	3,051	2,815	8,675
%	82.6	81.4	81.8	81.9
New member at follow-up	175	212	221	608
%	5.1	5.7	6.4	5.7
New member (visitor) at follow-up	99	121	86	306
%	2.9	3.2	2.5	2.9
No longer a member at follow-up	319	363	318	1,000
%	9.4	9.7	9.2	9.4
Total	3,402	3,747	3,440	10,589
%	100	100	100	100

Notes: All figures are calculated for the 1,768 households re-contacted in the post-intervention survey. At standard significance levels, we cannot reject the null hypothesis of independence between treatment and a categorical variable representing the different membership status indicated along the rows of the table (p-value= 0.7157). The test is a Pearson chi-squared statistic robust to clustering (Rao and Scott 1984).

Table A.12: Changes in Household Demographic Composition

	Regression Coefficients			Value at Baseline
	Constant (1)	Free (2)	MF (3)	(4)
Males, U5	0.007*	−0.004	−0.005	0.044
	(0.0035)	(0.0043)	(0.0049)	
Females, U5	0.008***	−0.007	−0.010**	0.042
	(0.0029)	(0.0040)	(0.0046)	
Males, 5 to 15	0.003	0.001	0.004	0.096
	(0.0031)	(0.0043)	(0.0047)	
Females, 5 to 15	0.000	0.006	−0.002	0.089
	(0.0032)	(0.0050)	(0.0043)	
Males, 15 to 45	−0.004	0.008	−0.001	0.254
	(0.0048)	(0.0071)	(0.0065)	
Females, 15 to 45	0.005	0.004	0.006	0.256
	(0.0042)	(0.0061)	(0.0058)	
Males, over 45	−0.011**	0.001	0.012**	0.114
	(0.0044)	(0.0058)	(0.0058)	
Females, over 45	−0.007**	−0.008*	−0.005	0.106
	(0.0031)	(0.0048)	(0.0053)	

Cross-equation joint tests	Statistic		p-value
Free = 0	F(8,129)=	1.4187	0.2031
MF = 0	F(8,129)=	2.0016	0.0596*
Free = MF = 0	F(16,121)=	1.5921	0.0904*

Notes: All figures are calculated for the 1,768 households re-contacted in the post-intervention survey. Each row reports coefficients of a separate OLS regression estimated at the household level, where the dependent variable is the change—between baseline and follow-up—in the fraction of the household who belongs to the specified age-gender group. The figures in column 1 are mean changes in control areas, while the coefficients in the next two columns are the additional differences in the changes in Free (column 2) and MF (column 3) communities. Standard errors (in brackets) and tests are robust to intra-village correlation. Asterisks indicate significance at the 10 (*), 5 (**) and 1% (***) level. The joint tests are robust to the presence of cross-equation correlation of residuals.

Table A.13: Correlates of ITN purchase

Dependent variable: at least one ITN purchased	OLS
Log(monthly total expenditure per head)	-0.116
	(0.053)**
Debt towards BISWA (per head, quartic root)	-0.005
	(0.009)
Cost of malaria episodes last 6 months (per capita, quartic root)	0.019
	(0.011)*
% member who slept under net last night	0.209
	(0.093)**
% member who slept under ITN last night	-0.053
	(0.279)
# nets owned by household	0.007
	(0.026)
# nets treated last 6 mts	-0.033
	(0.036)
% members using nets during peak season	-0.035
	(0.079)
Any malaria-related deaths last 5 yrs	0.101
	(0.141)
Expected cost of a malaria episode (quartic root)	0.014
	(0.019)
% blood tests showing Hb$<$ 11	-0.004
	(0.049)
% malaria +ve	0.202
	(0.080)**
% HH members with self-reported malaria episodes last 6mts	0.272
	(0.116)**
Subjective $P(\text{malaria} \mid \text{untreated net}) - P(\text{malaria} \mid \text{ITN})$	-0.066
	(0.106)
Subjective $P(\text{malaria} \mid \text{no net}) - P(\text{malaria} \mid \text{ITN})$	-0.140
	(0.142)
Observations	513
R-squared	0.11

Standard errors in parenthesis are robust to intra-village correlation. Statistical significance is indicated with * (10% level), ** (5%) and *** (1%). Data on ITN purchase collected during sale operations in fall 2007. All other data are part of the baseline survey (spring 2007). Only panel households included. Also included are the following regressors, none of which is significant at standard levels: intercept, age, gender and schooling of household head, household size, number of members younger than 5 years old, or 5 to 14, or older than 60, survey-elicited measures of risk aversion or intertemporal preferences.

Table A.14: Post-intervention malaria Biomarkers: Testing success rate in baseline households

	(1)	(2)	(3)	(4)	(5)	(6)
	Absent	Absent	Absent	Refusal	Refusal	Refusal
Free		-0.001	-0.001		-0.009	-0.01
		[0.018]	[0.018]		[0.015]	[0.015]
MF		0.006	0.005		0.018	0.017
		[0.018]	[0.019]		[0.016]	[0.016]
Male, 0-5			-0.212			0.017
			[0.020]***			[0.013]
Female, 0-5			-0.205			0.045
			[0.023]***			[0.017]***
Male, 5-15			-0.121			0.017
			[0.018]***			[0.010]*
Female, 5-15			-0.136			0.008
			[0.019]***			[0.010]
Female, 15-45			-0.187			0.011
			[0.015]***			[0.006]*
Male, >45			-0.133			0.003
			[0.017]***			[0.006]
Female, >45			-0.212			0.036
			[0.018]***			[0.009]***
Constant	0.194	0.193	0.32	0.057	0.054	0.043
	[0.007]***	[0.013]***	[0.018]***	[0.006]***	[0.011]***	[0.012]***
Observations	9589	9589	9555	9589	9589	9555
R-squared	0.0000	0.0001	0.0404	0.0000	0.0023	0.0052
Clusters	141	141	141	141	141	141
Free=MF=0		0.9209	0.9343		0.2303	0.2355
M=F,0-5			0.7449			0.1558
M=F,5-15			0.4402			0.4505
M=F,Over 45			0.0000			0.0010

Notes: Data from post-intervention household survey (Winter 2008-09). Standard errors (in brackets) are robust to intra-village correlation. Asterisks indicate significance at the 10 (*), 5 (**) and 1% (***) level. All figures are OLS estimates of a linear probability model where the dependent variable is indicated in the column header. Both absence and refusal refer to malaria RDTs, but the figures for Hb are almost identical. All regressions include only observations from the 1768 households interviewed at baseline and re-contacted during the follow-up survey.

Table A.15: Results of Rapid Diagnostic Tests Validation (July 2009)

	RDT(1)	RDT(2)	RDT(3)
RDT(2)	0.7873		
RDT(3)	0.7844	0.8760	
Microscopy	0.5274	0.6131	0.5968

		Microscopy	
		-ve	+ve
Tester 1 RDT	-ve	129	1
	+ve	45	30

		Microscopy	
		-ve	+ve
Tester 2 RDT	-ve	148	3
	+ve	26	28

		Microscopy	
		-ve	+ve
Tester 3 RDT	-ve	146	3
	+ve	28	28

Notes: Data from July 2009. The results refer to tests of 205 blood samples collected from symptomatic cases in 3 villages in Rourkela district (Orissa). Codes 1 and 2 refer to two of the testers who participated to follow-up household survey. Code 3 refer to the most senior survey monitor in the team.