Micro-loans, Insecticide-Treated Bednets and Malaria: Evidence from a Randomized Controlled Trial in Orissa (India)

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Abstract

We describe findings from the first large-scale cluster randomized controlled trial in a developing country that evaluates the uptake of a health-protecting technology, insecticide-treated bednets (ITNs), through micro-consumer loans, as compared to free distribution and control conditions. Despite a relatively high price, 52% of sample households purchased ITNs, although coverage remained significantly lower than what achieved with free distribution. Most strikingly, neither micro-loans nor free distribution led to improvements in malaria and anemia prevalence. We examine several plausible explanations, and argue that insufficient ITN coverage was the most likely cause.

JEL: I1,I3
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1 Introduction

There has been much recent debate on the virtues of the sustainability of development initiatives (Kremer and Miguel 2007). Advocates of sustainability often focus on cost-recovery as a key criterion for evaluating poverty reduction, health and education initiatives (Akin et al. 1987, Nolan and Turbat 1995, Ansah et al. 2009, Alam and Ahmed 2010, Sarriot et al. 2010, Smith 2010). On the other hand, subsidies may be optimal if there are substantial externalities as is the case with some health interventions.[1] In addition, a number of recent empirical studies have demonstrated that reducing subsidies even by relatively small amounts leads to dramatic declines in demand for health protective technologies among the poor. In Kenya, Kremer and Miguel (2007) find that a 20% co-pay for deworming drugs reduced uptake from 75 to 19%. In the case of water disinfectant, Ashraf et al. (2010) estimate a price elasticity of demand of -0.6 in urban Zambia, while Kremer et al. (2009) document only a 10% uptake when the product was offered at half-price in Kenya. In rural Kenya, Cohen and Dupas (2010) find that a still remarkable 90% subsidy reduces uptake of insecticide treated bednets to 10%, relative to 99% achieved with free distribution. In rural Zambia, Agha et al. (2007) find that ITN subsidization did not increase ownership rates among the poorest households.

In this paper, we describe findings from the first large-scale cluster randomized controlled trial (RCT) in a developing country context that evaluates the uptake of a health-protecting technology through micro-consumer loans, as compared to free distribution and control conditions. In addition, we also estimate health impacts by measuring key health indicators through the collection of biological samples ("biomarkers"). Specifically, we evaluate the relative effectiveness of micro-loans at increasing ownership and use of insecticide treated bednets (ITNs), and ultimately at reducing the burden of malaria in highly malarious areas of rural Orissa (India).

Transmitted by *Anopheles* mosquitoes, malaria represents an enormous global health burden, with a worldwide incidence of 300-660 million cases annually, 80 million in India alone.[2] One third of the human population is estimated to live in areas at risk for the most severe form of malaria, caused by *Plasmodium falciparum* (Snow et al. 2005). The negative association between the disease and growth and the accumulation of human capital have been long recognized (Gallup and Sachs 2001, Sachs and Malaney 2002, Malaney et al. 2004), although studies that convincingly document and quantify causal links are relatively recent within economics (Hong 2007a, Hong 2007b, Barreca 2010, Bleakley 2010, Cutler et al. 2010, Lucas 2010, Kitchens 2010). Numerous studies have shown that high coverage and use rates of ITNs are efficacious at reducing malaria-related morbidity and mortality, as documented in the extensive survey in Lengeler (2004). One key trial demonstrated that ITNs distributed to achieve near-complete coverage in an area with high levels of perennial malaria transmission in Western Kenya resulted in a 19% reduction in *falciparum* malaria prevalence, a 39% reduction in moderately severe anemia in children under three,

[1] See e.g. Hammer (1997), Gersovitz and Hammer (2004, 2005) for an examination of economic approaches towards health and infectious diseases in particular.

[2] Snow et al. 2005, Korenromp 2005. Malaria infection may develop into debilitating febrile episodes and lead to severe anemia, pregnancy complications, permanent neurologic and developmental impairment for children, kidney failure, seizures, coma and death. Mortality rates are particularly high among young children and pregnant women (Breman 2001).

and a 90% reduction in the malaria vector population (Gimnig et al. 2003 and ter Kuile et al. 2003).

High population coverage of ITNs is considered a key means to reduce malaria, through externalities which benefit even individuals not sleeping under an ITN due to the reduction in mosquito density (Hawley et al. 2003, Killeen et al. 2007). However, ITN adoption in most malarious areas remains very low and public health agencies frequently have insufficient resources to provide complete ITN coverage for all individuals at risk. In such a context, a more sustainable approach focusing on cost-recovery may be desirable, but it may lead to the exclusion of vulnerable individuals who do not have access to sufficient funds.

Our field experiment was conducted 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 communities with BISWA presence in the five districts of Bargarh, Balangir, 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 lender clients in a second group ("Free" villages) received at no cost a number of ITNs determined as a function of household composition. Clients from the third group of villages (Microfinance or "MF" villages) were offered contracts for the purchase of ITNs and re-treatments, using consumer loans with a one-year repayment period. The ITN offer price was *not* subsidized and included a mark-up to cover delivery and overhead costs to BISWA. The price was not negligible, corresponding approximately to three to five times the local daily agricultural wage. The pre-existing affiliation with BISWA was expected to ensure high repayment rates on the loans. Buyers in MF communities could decide between purchasing ITNs alone or a bundle which included, in addition, two future re-treatments with insecticide. After six and twelve months, field workers returned to MF and Free communities, and offered re-treatment at no cost both in Free villages and to buyers who chose the bundle in MF communities. In the latter villages, buyers who chose to purchase the ITNs only could re-treat the net for cash.

Our project has several specific aims. First, we evaluated to what extent the offer of small loans for purchasing ITNs would lead to increases in ITN ownership, even among poor households. To the best of our knowledge, this is the first large-scale cluster RCT to evaluate the efficacy of a public health program where a health-protecting technology was provided at full cost but allowing for repayment over time, as compared to both control conditions or free distribution. We also evaluate the cost-effectiveness of micro-loans when compared to free distribution, taking into account the incomplete repayment rates observed in the field.

Second, we evaluated the impacts of the alternative ITN delivery mechanisms on malaria prevalence and hemoglobin levels (an important indicator of malaria exposure) measured through blood tests. The use of biomarkers is important, both because health indicators (and not ITN ownership or usage rates) are the key outcomes, and because self-reported health indices commonly suffer from substantial non-random measurement error (Strauss and Thomas 1998). In addition, ours is the first large-scale RCT that analyze the impact of ITNs on malaria indices in India.[3]

[3]Of the 22 RCTs surveyed in Lengeler (2004), none has been carried out in India, and only one was conducted in a south Asian country (Pakistan, see Rowland et al. 1996), but using within-community randomization. A number of studies conducted in Orissa and other Indian states are listed but not surveyed in Lengeler (2004) because the lack of an appropriate

Third, we analyzed whether re-treatment rates for bednets differed systematically in MF villages between households that purchased ITNs with different contracts. Although treatment of nets with insecticide is considered to be safe and efficacious, and is relatively inexpensive, regular re-treatment is rare. 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 any study that analyzes the relationship between commitment devices and health-seeking behavior in a developing country.

Lastly, we re-visit 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 who do not need the product) and to increase usage conditional on uptake. 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) showed that, conditional on willingness-to-pay, Kenyan women who paid higher prices did not use ITNs more relative to others who received them for free. Ashraf et al. (2010) also found no evidence of sunk cost effects in the usage of a water purification product in Zambia, although they estimated that higher prices screened out individuals who were less likely to use the product in the short term.

We find that micro-loans successfully increased ITN ownership and (self-reported) use. In MF villages, 52% of sample households purchased at least one ITN. This was despite the relatively high offer price of the ITNs, which also included a 6% overhead for delivery and other costs. In practice, the fraction of loans repaid by households at the time of the follow-up survey (about 2 months after most loans were due) was 64%. Assuming no further repayments, this amounted to an average subsidy of 36% on the full price of the ITNs. Moreover, despite the relative success of the sales, the mean uptake (0.24 ITNs per person) was substantively and statistically significantly lower than in areas where nets were distributed for free (0.52).

Although the study design does not allow us to estimate a price elasticity, the high uptake contrasts sharply with the low demand for health products documented among poor households in Agha et al. (2007), Ashraf et al. (2010), Cohen and Dupas (2010), Kremer and Miguel (2007) and Kremer et al. (2009). Many alternative explanations have been proposed for these low take-up rates, including low perceived benefits, present-biased preferences and credit and liquidity constraints. While we cannot experimentally distinguish between the competing hypotheses, we provide evidence that cash-on-hand constraints likely played a key role in our context. Unlike Cohen and Dupas (2010), we also find evidence of significant screening effects of cost-sharing, with strong associations between demand and measures of perceived benefits from ITNs.

Net usage rates were consistent with the pattern of uptake among study arms. 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

control group.

"usual" during the peak mosquito season in the three experimental arms were respectively 77, 36 and 7%. Conditioning upon ownership, we find that usage rates were substantially and statistically significantly *higher* when ITNs were delivered at no cost. Consistent with Ashraf et al. (2010) and Cohen and Dupas (2010), we thus find no evidence in favor of the hypothesis that free provision leads to lower usage of health products relative to cost sharing (we actually find support for the opposite).

In MF villages, we also found that re-treatment rates were significantly higher when ITNs were purchased bundled with two re-treatments, even after controlling for household characteristics predictive of contract choice. Although the non-experimental nature of the result does not allow us to interpret it as conclusively causal, the finding is consistent with the implicit inclusion of a "commitment" to re-treat in this contract via the bundling of future re-treatments.[4] This finding has potentially important implications for the design of public health policies, because it suggests that when programs call for cost-sharing and require compliance with certain behaviors over time (such as re-treatment of bednets), the inclusion upfront of any monetary costs of such behaviors may increase compliance.

The most surprising findings of our study come from the evaluation of the health impacts. We found 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. The point estimates actually indicate slightly higher prevalence rates in intervention areas relative to control locations. This, together with the relative precision of the estimates, allows us to rule out substantial declines in prevalence comparable to those found in earlier studies. Hemoglobin levels barely differed across treatment areas, and we could only document a small improvement (11% of a standard deviation) in Free villages, significant at the 10% level. In addition, the lack of improvements was largely shared by all demographic groups.

Our data allowed us to rule out a number of potentially plausible reasons for the lack of health benefits, including contamination due to other public health programs in study areas, measurement error in health outcomes, and behavioral responses such as changes in malaria treatment or prophylactic behavior that could have potentially offset the benefits of the intervention. Ultimately, we conjecture that the most plausible explanation rests on a comparison between our study design (which involved 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/or close monitoring of health and ITN usage). In this sense, our results should *not* be interpreted as contradicting these earlier seminal studies, which convincingly document the efficacy of ITNs in controlled conditions. Rather, our results complement this literature, and suggest that ITN distribution programs that do not lead to sufficient coverage and regular usage may do little to reduce the malaria burden. This interpretation has important implications,

[4]Note that this is not the only interpretation because differences in other preference parameter (e.g. risk or cost parameters), endowments, information or risk perceptions and beliefs could also explain the data. In a related paper, Mahajan and Tarozzi (2011) specify a dynamic discrete choice model with potentially time-inconsistent preferences to isolate the relative importance of these explanations. They find that after accounting for differences in endowments and beliefs, heterogeneity in time-preferences plays a much larger role in explaining take up than heterogeneity in risks or cost preferences.

because many public health programs of ITN distribution only target vulnerable groups, such as young children and pregnant women, who may represent only a small fraction of the population (WHO/UNICEF 2005). Such programs may not guarantee sufficiently high coverage (with their associated externalities) and may therefore fail to provide health benefits to the intended beneficiaries. In such a context, cost recovery programs (even when relatively cost effective and successful at inducing high uptake of health products, as in our case) may further limit the ability to reach the critical levels of coverage required for health benefits to arise.

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 an analysis of attrition between baseline and follow-up survey. Section 3 describes the estimated impacts of the interventions on ITN uptake, (self-reported) usage, re-treatment rates and finally on health outcomes. Section 4 considers several potentially plausible hypotheses to explain 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

This study took place in rural Orissa, India, in a number of communities spread across a wide area in the five districts of Bargarh, Balangir, Keonjhar, Kandhamal, and Sambalpur (Figure 1). Orissa, one of poorest states in India, is also the most highly malaria endemic state in the country (Kumar et al. 2007). The Indian National Vector Borne Disease Control Programme recorded 365,593 confirmed cases of malaria in Orissa in 2007, 88% from *P. falciparum*. This accounted for 25% of malaria cases and 40% of *P. falciparum* malaria in India, despite Orissa hosting less than 4% of the Country's population. Records from 2003-2006 showed similar patterns. Data from the 1998-1999 National Family and Health Survey (NFHS) show that the fraction of individuals who had (respondent-diagnosed) malaria over the three months preceding the interview was 8.5% in Sambalpur and Bargarh, 8.8% in Balangir, 12.3% in Keonjhar and 17.2% in Kandhamal.

Our study was conducted in collaboration with BISWA (Bharat Integrated Social Welfare Agency), a micro-lender with a large presence in rural Orissa. At the beginning of the study, BISWA provided a list of 878 villages where it operated, together with rosters of clients as of November 2006. These 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, a maximum of one village from each *panchayat* was allowed in the study, to limit the extent of cross-village contamination.

From these 878 villages, 150 were randomly selected for inclusion in the study; we selected 33 villages from Balangir, 48 from Bargarh, 30 from Keonjhar, 9 from Kandhamal and 30 from Sambalpur. The allocation of the sample was approximately proportional to the number of BISWA communities in each

district. Villages were drawn by means of a pseudo-random number generator, and the selection algorithm ensured the inclusion of a multiple of three villages from each block. After the completion of the baseline survey, one-third of villages within each block were then randomly assigned to each experimental arm. Blocks where the Government of Orissa was planning to initiate free distribution of nets were excluded from the sampling frame.[5] Despite BISWA's widespread operating network, communities where the micro-lender operate were not a representative sample of all villages in the five study districts. In fact, BISWA villages were, on average, larger and with better amenities than the overall population.[6] After the baseline survey, but before the intervention, nine of the 150 villages were found to have no actual BISWA activity and were then excluded from the study. Data from these villages are excluded from the analysis.

Next, we describe the pre-intervention data and the nature of the intervention in detail, and we briefly lay 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.

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. The sampling frame at baseline included all 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 households were included.

The baseline survey assessed a broad range of demographic, socio-economic and health variables, including information about expenditure in (or, when relevant, home production of) a comprehensive list of 18 different consumption categories. For each household member, we also recorded age, gender, schooling, occupation, and a complete history of notable health-related problems in the six months before the survey. 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; or, were due to malaria. 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 caretaker. Although the health history section of the questionnaire was very detailed, the respondent-reported nature of these data should be kept in mind, because self-reported health information is often plagued by non-random measurement error (Strauss and Thomas 1998). In addition, in areas with continuous malaria transmission, adults are often parasitemic but asymptomatic because of acquired partial immunity (Vinetz and Gilman 2002). Fevers of different origins are also often mistakenly attributed to malaria, and symptoms for very young children may be hard to identify.

[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 state Government 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.

[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.

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 testing was performed for all pregnant women, children under the age of five (U5), mothers of U5s, and one randomly selected adult (age 15-60). Fingerprick blood samples were obtained, which required less than 0.5 ml of blood each. Malaria prevalence was determined using the Binax Now malaria RDT. This test is well validated in comparison to blood smears for the diagnosis of malaria. The RDT detects both current and recent infections, up to 2-4 weeks prior to the test. The test does not indicate the level of parasitemia, and can only detect positive / negative for malaria infection, and whether that infection is due to *P. falciparum*, to one of the other *Plasmodium* species, or both (Moody 2002, Farcas et al. 2003, van den Broek et al. 2006, Khairnar et al. 2009).[7] Anemia, defined here as hemoglobin (Hb) levels below 11 grams per deciliter of blood, is a common health condition in developing countries, with multifactorial causes, including nutrition and intestinal parasites (Thomas et al. 2006). Malaria can severely worsen anemia, because the parasite destroys red blood 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 in U5 is often one of the most sensitive indicators of changes in malaria prevalence (Hawley et al. 2003, ter Kuile et al. 2003, Leenstra et al. 2003). Hemoglobin levels were tested with the HemoCue 201 Hb analyzer, a portable, accurate system for measuring Hb. The test, like the one used to detect malaria prevalence, requires less than 0.5 ml of blood and delivered results in approximately 15 minutes. Consent for testing both malaria infection and Hb levels was sought for the same set of individuals.[8] Overall, malaria infection was tested in 2,561 individuals from 1,704 households, and Hb levels were measured for 2,532 from 1,687 households. In all cases, test results were communicated to the tested individual or his/her guardian.

The survey questionnaire also included questions aiming at gauging respondents' risk aversion and time preferences. Separate sections gauged knowledge and practices related to malaria and bednets, including 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 sleeping arrangement of household members 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 study groups of 47 villages each. We label the three arms (described in detail later) as "MF" (microfinance), when nets

[7] 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). The test is particularly sensitive for *P. falciparum* infection. Sensitivity is lower for *P. vivax*, *P. malariae* and *P. ovale*.

[8] 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. (2011) 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.

were offered for sale on credit, "Free", when the intervention called for free distribution of ITNs, 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. The null of equality of means across arms is not rejected at standard significance levels in 19 of 21 variables, which suggests that the randomization provided overall good balance across experimental arms.

A large majority of households belong to Scheduled Castes and Tribes and Other Backward Castes, and less than 10% of household heads had a secondary school diploma or above. Estimates of expenditure per person per day ranged 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 across the three arms at the 10% level (p-value= 0.085). Using purchasing power parity (PPP) conversion rates (World Bank 2008), these estimates are just below 1.5 USD per person per day. Approximately 20% of households are below the official poverty line for rural Orissa (see table caption for details). Note that, despite all sample households being affiliated to BISWA, more than half of respondents state that it would be difficult or impossible for the household to borrow the sum of Rs 500, which is approximately the price of two program ITNs (see below).

At the time of the intervention, at least one bednet was already present in 65% of households. Almost all bednets had been purchased from the market: among households who owned at least one net, 95.4% purchased from local markets, while less than 5% received nets in other ways, including as gifts from any source. Among the bednets used the night before the survey, only four had been obtained for free, while the others had been purchased at an average price of Rs 92 per net (the median was Rs 70). Despite the high ownership rates, bednet coverage was far from universal, with one third of households not owning 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. Despite the low ownership rates in all three arms, the null of equality is rejected at the 5 percent level (p-value = 0.027). Less than 15% of individuals slept under any type of bednet the night before the baseline survey, and less than 3% slept under an ITN, which also did not differ significantly between study arms. 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.[9] 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 the 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 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

[9]Our study did not measure mosquito activity, but the seasonality of malaria transmission is well known and confirmed by studies conducted in neighboring areas (Sahu et al. 2003, Sharma et al. 2006).

individuals tested positive for malaria, in almost all cases its most severe form, caused by *P. falciparum*. The prevalence of malaria is similar across 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), see Figure 3. In areas of high malaria endemicity, it is common to find that malaria prevalence declines with age, due to the partial immunity that is acquired with repeated exposure to the parasite (Doolan et al. 2009). However, such age gradient is especially pronounced for elevated parasitemia ($>$ 5000 parasites per microliter of blood (μl, Beadle et al. 1995, McElroy et al. 1997). The RDTs used in our study have been shown to be very sensitive to *Pf* even at low levels of parasitemia, with 89% or above of infected blood samples correctly identified as positive to *Pf* with parasitemia level as low as 100-500/μl, and ~100% sensitivity for level of 1000/μl or above.[10] It is then possible that the absence of a monotone age gradient in our sample was due to relatively low parasitemia levels in our study areas, which would also be consistent with the very low rates of self-reported malaria. In fact, at baseline, only 23 households reported that at least one member had malaria at the time of the interview, despite prevalence being ten times as large according to the RDT results. Sharma et al. (2006) found similar non-monotone age gradients in incidence and prevalence in Sundargarh, a district of Orissa that shares borders with two of our study districts.

Looking now at hemoglobin levels, we find that almost half of the tested individuals were anemic, although there is significant heterogeneity by age and gender. Approximately 80% of tested U5, of either gender, were anemic. Anemia rates declined significantly among adults aged 15 to 45, but prevalence remained extremely high (60%) among women, while it was less than 12% among men. Prevalence increased again among older adults, where it characterized about three-quarters of women and one quarter of men. Similar patterns for anemia for different ages and genders are common in developing countries (see for instance Thomas et al. 2006), and are also present in data from Orissa collected as part of the Indian National Family and Health Survey in 2004-05, which showed an anemia prevalence of 65% among U5, 34% among women 15-49 and only 8% among men in the same age group.

Overall, Figure 3 documents an overall poor health status of the study population that is consistent with large potential health gains from a reduction in the malaria burden.

2.2 The Intervention

The 141 study 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 malaria, the means by which it is transmitted and the importance and rationale for ITN use, a demonstration of how to hang nets properly, and advice on proper use and re-treatment.[11] 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

[10] In addition, the performance of the RDT did not appear to be related to the age of the individual tested. Details about the Binax RDT can be found in the documentation included with the product by the manufacturer. See also Moody (2002).

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

composition, with a maximum of four. The nets were treated with insecticide on the spot by trained personnel, following rules recommended by the World Health Organization (World Health Organization 2002).[12] Individuals were also informed 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. Pyrethroids have been widely used for bednet impregnation with encouraging evidence about the lack of side-effects on human health (World Health Organization 2005). In Orissa, synthetic pyrethroids have been in use since 1999, and tests performed in 2002-03 in several districts (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 Sundargarh, which borders the study district Sambalpur (Yadav et al. 2001, Sharma et al. 2006).

In the 47 "MF" communities, ITNs were offered through micro-loan contracts and, like in Free communities, only BISWA clients were targeted. ITNs could also be purchased for cash, but this option was chosen in only a handful of cases. The micro-consumer loans were offered by BISWA separately and in addition to any other loan already outstanding. There was no movement of funds at the time the loan was initiated: if a household decided to purchase one or more ITNs, the nets were delivered and repayment was scheduled to be completed within one year (see below). At the time of delivery, nets were treated identically to that described above for free distribution villages, but the IC also included a detailed explanation of the loan contracts. ITN distribution and recording of loan contracts were to be completed 2-3 days after the IC.[13] 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. A second visit was conducted approximately one month later, where ITNs were offered again with the same contracts. No ITNs were offered after this second visit. The program ITNs were of very good quality and significantly sturdier than most of the pre-existing nets in the study areas.[14] During both village visits, detailed records were taken regarding attendance and ITN uptake by households included in the baseline survey, together with summary statistics about ITN distribution at the community level (comparable data were also recorded in villages with free distribution).

ITNs were offered for sale with two alternative loan contracts, both at BISWA's standard interest rate, 20% per year. With the first offer (contract "C1"), single sized ITNs were sold on credit for Rs 173, double sized ITNs for Rs 223, and repaid with twelve monthly installments of Rs 16 (single) or Rs 21

[12] 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. The concentration of the insecticide was determined based on the manufacturer's instructions: 10 ml of insecticide to 500 ml of water for single nets and 15 ml-750 ml for double nets.

[13] 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.

[14] 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 single and 2,500 double nets.

(double). Households were informed that survey personnel would re-visit the villages after six and twelve months and offer re-treatment for Rs 15 (single) or Rs 18 (double). With the second contract ("C2"), the household purchased not only the ITN but also a sequence of two re-treatments. In this case, the price was Rs 203 (single) or Rs 259 (double), to be paid as twelve monthly installments of Rs 19 (single) or Rs 23 (double). With the second option, no cash payment was required for re-treatment as the loan amount already included the price of the insecticide. To put these prices in perspective, at the time of the intervention, daily wages for agricultural labor were around Rs 50, and 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), who share the responsibility among members for repayment. 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 containing 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 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, and pay current and past dues after the harvest; early repayments are also 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 for the re-treatment of the bednets, which was completed by study personnel in a central location within villages. In Free villages, the service was provided free of cost. Re-treatment was also without additional cost in MF villages for those households which had purchased ITNs with the C2 contract, whose price included both the ITN and two re-treatments. Those who purchased ITNs using the C1 contract type (which did not include pre-purchased re-treatments) were offered re-treatment for cash. At the time of the first re-treatment, a short questionnaire was completed, 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 re-treatment choices and, in MF villages, information about loan repayment up to that point.

A detailed post-intervention survey was conducted shortly after the second re-treatment, between December 2008 and April 2009. The content of the survey instrument was similar to the baseline questionnaire and again measured ITN ownership and usage, and health status. Malaria prevalence and hemoglobin levels were measured by similar methodology to the baseline survey. A longitudinal data set was created by re-contacting all baseline households whenever possible. We also increased the number of biomarkers collected by attempting to test all household members for malaria and hemoglobin, rather than for specific

demographic groups as was done at baseline.[15]

Attrition at follow-up was limited and mostly due to temporary migration or inability to find eligible respondents despite repeated visits.[16] Of the 1,844 initial households, 1,768 (96%) were re-interviewed. The null of equal attrition rates among arms is not rejected at standard levels, and neither bednet ownership not the results of the biomarkers at baseline are statistically or substantively significant predictors of attrition (see Appendix A.2 for details).[17]

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). 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), 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 uptake. All statistical inference is conducted with tests and standard errors robust to the presence of intra-village correlation of residuals.

3.1 Bednets uptake and Ownership

We first evaluate the impact of the intervention on ITN uptake, 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 uptake for household i at the time of the intervention d.[18] We estimate all regressions with Ordinary Least Squares (OLS), clustering standard errors at the community level. The

[15] 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.

[16] The survey protocol called for at least three attempts, but a handful of households were re-contacted after 4 or 5 visits. Refusals accounted for only 13 of the 76 lost households.

[17] We also investigated changes in the demographic structure of the households located both at baseline and follow-up. We find little evidence that such changes were associated to the interventions in ways that could potentially matter for the interpretation of the results (see Appendix A.2 for details).

[18] 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 2. In communities with free distribution, almost all sample households (96%) received at least one ITN, with an average of 2.7 nets per household, about one for every two people. Of the 610 sample households in Free villages, only 25 did not receive any nets, in 22 cases because our field teams could not locate any member at the time of the delivery. In MF villages, ITN acquisition was significantly lower, with 309 of 589 (52%) of households purchasing at least one ITN (1.2 nets per household, or one ITN every four people). Almost all buyers chose to purchase on credit, with only ten households choosing to pay in cash. The null of equality in ITN uptake between Free and MF communities is rejected at any standard significance level. Still, it is remarkable that 52% of sample households purchased one or more ITNs, despite their non-trivial cost. If we look only at households who received at least one ITN, mean uptake among buyers in MF villages was lower but close to that achieved with free distribution (2.3 vs. 2.8 ITNs per household respectively), although the difference is significant at the 10% level. The high uptake of ITNs purchased on credit contrast sharply with the very low purchase rates for health products documented among poor households in Agha et al. (2007), Ashraf et al. (2010), Cohen and Dupas (2010), Kremer and Miguel (2007) and Kremer et al. (2009).[19]

While there was little variation in household uptake rates in Free villages, in MF villages there was significant heterogeneity (see Figure 4). The distribution of the mean number of ITNs delivered for free to sample households is very concentrated around the mean. Most of the variation is due to differences in household composition or (in rare cases) to the absence of household members during the visit. The sale of ITNs on credit led instead not only to a lower mean but also to more variation, with no purchases among sample households in five of the 47 villages. The two outliers among the MF communities are villages where several households decided to purchase a large number of ITNs for resale. If we omit these two communities from the analysis, uptake in MF areas declines from 0.24 to 0.2 ITNs per person (results not shown).

3.1.1 Correlates of ITN Purchases on Credit

In Table 3, we look at correlates of ITN purchases in MF villages. On the one hand, these results are descriptive and do not necessarily imply causal associations between the predictors and the decision to purchase. On the other hand, the estimates provide useful information on two key related issues. The first is whether cost-sharing leads to screening, so that households with higher benefits from ITNs are more likely to purchase them. The possible selection effect induced by positive prices is sometimes seen as an argument in favor of cost-sharing. However, Cohen and Dupas (2010) do not find evidence of such selection into purchase of ITNs in a sample of women in rural Kenya, when they measure need for ITNs using low hemoglobin levels. Ashraf et al. (2010) find instead that households who agreed to purchase a water purification product at higher prices in urban Zambia were more likely to use the product, at least in the short term. A second related issue is whether purchase decisions are consistent with the presence of credit and/or liquidity constraints. Such constraints are often listed among the key reasons for low take-up of health-protecting technologies among the poor.

[19]Note also that the highest offer price for long-lasting ITNs in Cohen and Dupas (2010) was $0.60, that is, less than 20% of the least expensive ITN offered in our intervention. At this low price, they estimate a purchase rate of approximately 40%.

To analyze these points, we estimate 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, not reported, are almost identical). All covariates were recorded in the baseline survey which, as a reminder, was carried out 4-5 months before the net sales. Regressors include measures of expenditure, indebtedness with the micro-lender BISWA, demographic structure, ownership and usage of nets, proxies for risk aversion and time preferences, perceived protective power of nets, and measures of past exposure to malaria (see the table caption for additional details). The study design does not allow us to estimate price elasticities, because the same products were offered at the same conditions in all communities. To reduce the influence of outliers among regressors measured in Rupees, we transform values into logarithms or, when zeros are present, using the quartic root, which has a shape similar to the logarithm for positive numbers (Thomas et al. 2006). We omit some of the estimated coefficients from Table 3 for brevity, but none of the non-reported slopes (listed in the table caption) are significant at standard levels.

Many of the predictors are not statistically significant at standard levels, and overall the model explains only 11% of the variance of purchase decisions. Variables that describe the demographic structure of the household (including presence of U5s) are not significant, either individually or jointly (p-value= 0.6276). The estimates, however, indicate strong associations between demand and measures of perceived benefits from ITNs. First, usage of nets the night before the interview is one of the strongest predictors of purchase: conditional on other covariates, households where everyone used a net were 21 percentage points *more* likely to purchase nets relative to others where no one did. This is consistent with bednets being an experience good, with past usage perhaps associated with higher perceived benefits (Dupas 2010). Second, an increase from zero to the median monetary cost of malaria episodes incurred in the 6 months before the baseline interview, increases demand by 9 percentage points ($0.019 \times 590^{1/4}$). Third, malaria-related deaths in the previous five years increase the predicted probability of purchase by 10 percentage points. However, deaths were rare (only nine respondents reported any) and the coefficient is not significant. Fourth, both self-reported malaria episodes and prevalence as measured by our blood tests are among the strongest predictors of purchase. Moving from a household with no self-reported past malaria cases in the previous six months 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 predicts a 20 percentage points increase, and both coefficients are significant at the 1% level.[20] In contrast, we find that anemia levels are not correlated with demand, consistent with Cohen and Dupas (2010). This is consistent with anemia being a poor indicator of perceived marginal benefit from ITNs, perhaps because among poor households low hemoglobin levels are often caused by a number of epidemiological and nutritional factors besides exposure to malaria (de Benoist et al. 2008). Overall, these results strongly suggest that measures of past exposure to malaria are strongly associated with the decision to purchase. This is consistent with the presence of strong screening effects associated with the non-negligible prices charged by our program in MF communities.

[20]Recall that self-reported malaria cases were recorded for all members with a six-month recall period, while blood was drawn only from a subset of them at the time of the baseline survey. The two measures of malaria exposure are therefore not comparable.

The strong association between indicators of perceived benefits from ITN and demand for bednets on credit is also consistent with the presence of credit and/or liquidity constraints, although other explanations of the pre-intervention low ITN ownership rates cannot be completely ruled out. To test the hypothesis that such constraints were a key factor, it would have been ideal to include an experimental arm where identical ITNs were offered for cash only. Despite the absence of such intervention type, we can provide several pieces of evidence in support of this hypothesis. First, the option to purchase for cash, which was available to everyone, was chosen by only 10 of the 309 sample households who purchased ITNs. Second, households with higher monthly total expenditure per head were *less* likely to purchase ITNs, despite controlling for ownership and usage of pre-existing nets: a 10% increase in per capita expenditure predicts a 1.2% decrease in the probability of purchase, with the slope significant at the 5% level. This could be because poorer households may have found the opportunity to purchase ITNs on credit more appealing. Third, we have shown that bednets were already widely present throughout the study area before our intervention. So, the low pre-intervention ITN ownership rate was clearly not due to bednets being a completely new health-protecting technology in the area, although our ITNs were overall of better quality relative to those available in local markets. In principle, it is possible that households purchased ITNs on loan not because they were liquidity constrained but because they had alternative investment opportunities for their cash that yielded a return higher than the BISWA interest rate (20% annually). However, in that case (and in the absence of investment ceilings) one would have expected households to be maximizing their BISWA borrowing. Although we cannot rule out this possibility completely, we find that only about 14% of households had a current BISWA loan at follow up (excluding the ITN loan). Another possibility is that the preference for purchase on credit relative to cash was due not to liquidity constraints but to buyers having present-biased preferences. A purchase on credit could have been seen as a way to start enjoying the benefits of ITNs while postponing the associated costs. However, we find that an indicator of present-biased preferences predict neither the decision to purchase not the choice of cash vs. credit.[21] Overall, these arguments are consistent with cash-constraints having played a key role in explaining the relatively high take-up observed in our study.

3.1.2 Cost Effectiveness Analysis

Here we provide a rough evaluation of the relative cost effectiveness of ITN provision to Free and MF communities. The comparison would be trivially in favor of micro-loans with complete repayment and comparable take-up between arms, but neither condition held in reality. Together with the cost of the ITN themselves (which included delivery at the BISWA headquarters in Sambalpur), distribution expenses included costs for labor and for transporting the ITNs from Sambalpur to the villages. We estimate a transport cost of Rs. 500 per day and wages for Rs. 150 per day per worker, and about 1.5 days to cover a village. Note that, if our intervention were scaled up through a micro-finance network, these delivery costs could be actually lower if the delivery operations were scheduled using the MFI's existing labor

[21] The indicator is a measure of whether the respondent exhibited "preference reversals" in a set of intertemporal choices, similarly to Ashraf et al. 2006. See the caption of Table 3 for additional details. The results are available upon request from the authors.

and transportation resources. Insecticide treatment costs were Rs. 10-13 per net (depending upon size). Dividing the total cost thus obtained by the total number of ITNs distributed in each arm provides lead to cost per ITN delivered of Rs. 305 in MF villages and Rs. 226 with free provision. Turning to revenues, at the time of the follow-up survey, about one and a half years after the sale, sample households in MF communities had repaid on average 64% of ITNs, and we assume that no further payments were made afterwards. The price charged to buyers also included an overhead to cover BISWA's costs in administering the loan.

Overall, there are then two key drivers of the differences in the per ITN cost estimates across the two arms. On the one hand, approximately four times as many ITNs were distributed in Free villages, thus lowering considerably the incidence of fixed costs per ITN distributed. On the other hand, in MF villages a substantial fraction (though not all) of costs were recouped through repayments. These calculations yield a cost per ITN of about Rs. 150 in the MF villages and Rs. 225 (50% higher) in the Free villages. Overall, then, sales on credit were more cost-effective and the figures suggest that for every 1,000 Rupees (approximately \$60 at PPP) an additional $1,000 \times (1/150 - 1/225) = 2$ extra ITNs could have been distributed using micro-credit relative to free disbursal. However, note that since net disbursal density in MF villages was considerably lower than in Free villages, covering the same number of households under MF would require ITNS to be distributed across more villages. For instance, the 4,000 nets that we distributed in total in the 47 Free communities would have required reaching about 200 villages if they had to be sold on credit. An important corollary to this point is that cost effectiveness was achieved at the expense of significantly lower ownership rates relative to free distribution. To the extent that externalities from mass distribution are an important source of ITN protective efficacy, cost-recovery may be suboptimal since it will likely result in lower ITN densities.

An alternative way to evaluate the two disbursal schemes is to consider whether micro-credit provides better targeting of ITNs. We have shown earlier that in MF villages ITNs were more likely to be acquired by households with more recent exposure to malaria, while in Free villages ITN distribution only depended on household composition. Consider, for instance, ad admittedly crude categorization where households are "high benefit" if any of the members had malaria in the baseline, and "low benefit" otherwise. By this metric, 60% of all households were high benefit. In MF villages, these were 17 percentage points more likely to purchase an ITN. These considerations would tilt cost-effectiveness further in favor of microloans, implying that the cost of reaching a high benefit household using microloans was Rs 227 and the corresponding figure with free distribution was Rs 375, that is, two-thirds higher.

3.1.3 Post-intervention Bednet Ownership

Next, we turn to the assessment of bednet ownership at the time of the post-intervention household survey, completed in winter 2008-09, that is, about one and a half years after the intervention. We report ITT estimates where the dependent variable y_{ip} is the number of bednets owned by the household, regardless of acquisition mode or treatment status, either in total (column 5 of Table 2) or per person (column 6). Net ownership was reported by the respondent, but enumerators were instructed to ask permission to check

that the net was present with the household. The presence of the net was confirmed in about 90% of cases so that the estimates are likely to be only marginally affected by reporting errors (see also Section 4.3). 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 impacts 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 uptake results described earlier. The sale of ITNs on credit was successful in increasing ownership rates, although less than with free distribution. Even in control communities, we observed a small but statistically significant increase of 0.07 nets per person, while the increase was 0.18 in MF villages and 0.35 with free distribution. The very small increase in net ownership in control areas provides suggestive evidence that the short information campaign likely did not change behavior substantially. Free distribution led to a coverage of 0.63 nets per person, which is close to the figure of 2/3 of nets per person which has been taken to represent full coverage in some contexts (see for instance ter Kuile et al. 2003).

We also find evidence that the increase in net ownership was lower than the number of nets delivered during the intervention, particularly for Free villages. On average, households received 2.7 nets in Free villages, but post-intervention net ownership was only 1.9 higher than at baseline (column 7) for a gap of .8 nets. In MF communities, an average of 1.2 nets per household were purchased, but at follow-up the difference in ownership was only 0.9 (column 7) for a gap of .3 nets on average. In these same communities, the average gap is reduced to .1 if we exclude the two outlier villages where a number of BISWA members purchased more than 15 ITNs each, apparently for resale purposes. When we exclude these two villages, we find that in both Free and MF communities there was a .2 reduction in BISWA-provided ITNs relative to the time of the intervention (results not shown). These ITNs had been sold or otherwise lost or disposed of. In MF communities (again, excluding the two outliers), we observe instead a .1 increase in nets purchased from sources other than BISWA. Conversely, in Free villages the additional .6 gap is explained by a *reduction* in the number of non-BISWA nets. This is consistent with the hypothesis that a number of older, worn out nets had been disposed of, and replaced by the new, high-quality ITNs distributed by our program. This is also confirmed when we look at the fraction of non-BISWA nets identified by surveyors as being in good conditions at follow-up. This was 18% in Free villages, but only 14% in control villages, although the difference is not significant at standard levels (p-value = 0.2). In addition, the switch towards the newer ITNs supplied by BISWA is also confirmed by the bednet usage data, which we now turn to.

3.2 Bednet Usage

Next, we move to the analysis of bednet usage among panel households. We re-estimated models (2) and (3) using respondent's reports about individual bednet usage as the dependent variable. Both at baseline and follow-up, we recorded whether each household member had slept under a bednet the night before the interview, and whether the net had been treated in the previous six months. We also asked about bednet usage during the peak mosquito season, but in this case the distinction between treated and untreated nets was made only at follow-up, so for this outcome we estimate only model (2). The results are shown in Table 4. Estimates of model (2) are obtained using all household members at follow-up. By construction, the DD estimates (model 3) use only information about bednet usage for individuals who were household members at both time periods, and this explains the smaller sample sizes for these regressions.

The results, in Table 4, are largely consistent with the ownership patterns in Table 2: net usage in MF villages is significantly higher than in control areas, but remains significantly lower than that achieved with free distribution (column 1). The difference in usage of "any net" or ITNs between MF and Free villages is always significant at the 1% level. In control areas, at follow-up 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 ITNs relative to baseline in control villages. In MF villages, we find instead a 13 percentage points increase in ITN usage relative to baseline. The increase 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.

An interesting finding 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, 66% of individuals are reported as using nets regularly (column 7), but only 6% use ITNs (column 9). 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. Most likely, some of the new ITNs were given away or lost, while a number of them replaced lower-quality, previously owned ones, although we did not directly measure this.

A related question is whether the newly acquired bednets were disproportionately allocated to specific demographic categories. In Figure 2, we have shown that usage rates at baseline were higher among very young children but were otherwise relatively homogeneous. In Figure 5, we show changes in the fraction of household members who slept protected by an ITN the night before the interview, by gender, age group

and experimental arm. A first conclusion that emerges is that the changes are overall very similar between genders (compare graphs A and B).[22] Second, increases in usage rates are larger for younger individuals. This is especially true in Free areas, where usage for both genders goes up by about 60 percentage points among U5, but only by half as much among members over 60 years old. Third, although the increase in usage rates is significantly larger with Free provision, both the age gradient and the similarity between genders are observed regardless of whether the nets were acquired for free or on credit. Hence, we find no evidence that the allocation of available ITNs depended significantly on the mode of acquisition (see Hoffmann 2009 for an opposite result).

3.2.1 ITN Usage Related to Cost

The design of our study also allows us to look, to some extent, at the relationship between the price paid and the use of ITNs. A number of development practitioners hold the view that usage, conditional on ownership, may be lower when there is no cost sharing. 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. Two recent RCTs have questioned these arguments; Ashraf et al. (2010) addressed the provision of chlorine in Zambia, and Cohen and Dupas (2010) study 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 willingness to pay actually pay a different price.

Our study, lacking such two-stage randomization, is not ideally designed to study the link between ITN cost and usage. However, we show below that in our sample we found *higher* usage rates, conditional on ownership, when ITNs were received at no cost. In our sample, the net effect of self-selection, quality signalling and sunk-cost effect was thus not consistent with the hypothesis that free products are used less than purchased ones. One limitation of our results is that, like in Cohen and Dupas (2010), although ownership was confirmed by direct observations of the nets in the field, bednet usage was self-reported. Free provision of ITNs may have led to overstated usage rates, perhaps because of gratitude, or because beneficiaries were afraid of losing the nets if sparsely used. To reduce the risk of reporting bias, we count a net as having been used the night before only if the net was seen by field staff during the follow-up survey, and identified by them as one of the nets distributed through our program. In a large majority of cases, enumerators were able to verify the presence of the nets (see Section 4.3). The likely extent of reporting bias is further reduced by the fact that previous-night usage was very similar when estimated independently from two separate sections of the survey instrument, see Section 4.3.

For each household, we calculate the ratio between the number of utilized BISWA nets measured as

[22] In no case the null of equal changes between genders can be rejected at standard levels (results available upon request).

described above, and the number of BISWA nets delivered to the household.[23] The top panel of Figure 6 shows estimates of usage rates of ITNs delivered through our program in Free and MF villages, as a function of the number of nets delivered to the household in fall 2007. The bottom panel shows estimates of the difference in usage rates between MF and Free villages, together with 95% confidence intervals. The results show no evidence that paying a positive price increases usage, consistent with the findings in Ashraf et al. (2010) and Cohen and Dupas (2010). However, our results differ from these earlier contributions because the fraction of nets in use was actually 16-31 percentage points *higher* in free communities, and the null of equality is always rejected at the 5% level. Barring systematic over-reporting of usage of ITNs received for free, this unexpected finding could perhaps arise from social interaction effects such as imitation or social norms facilitated by the higher overall ownership rates in Free villages. Probing this hypothesis is beyond the scope of this paper, but it is an important area of future research, since it suggests another channel through which externalities may occur (in addition to the infection externalities discussed later).

3.3 The Decision to Re-treat

Periodic re-treatment 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 insecticidal nets (LLINs) which do not require re-treatment, in many locations the supply of LLINs is still limited. Regular ITNs were the standard type of treated bednet in our study areas at the time of the intervention, but re-treatment of ITNs has been uncommon (see Section 2.1.1).

In Free and MF villages, our team conducted two re-treatment campaigns approximately six and twelve months after ITN 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 re-treatments (contract "C2"). Contract C2 can therefore be seen as one which financially "commits" the buyer to comply with future re-treatments. Clients could also choose a mix of contracts, but among the 309 households (of 589) who purchased ITNs, 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-treatment campaigns, 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 re-treatment. Fees were Rs 15 for a single net and Rs 18 for a double. Table 5 shows the re-treatment rates in Free villages relative to those observed among buyers in MF communities, shown separately as a function of the contract chosen. The sample comprises all panel households that received ITNs during the intervention, excluding the handful in MF

[23] The results are similar if we use, as denominator, the number of BISWA nets present in the household's dwelling during the follow-up visit, of if we estimate usage rates for all BISWA nets, regardless of whether the net was actually observed.

[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.

communities who purchased ITNs with both contract types. After six months, 92% of free nets were re-treated (column 1), but the proportion of treated nets decreased to 83% at the second re-treatment campaign (column 3). Both re-treatment rates were thus very high, although not universal.[25]

As expected, re-treatment rates were also high among households who purchased the commitment product C2 in MF communities, although the rates are 8-9 percentage points lower. Remarkable differences emerge instead relative to re-treatment rates among buyers who chose contract C1. During the first re-visit, only 36% of nets purchased with C1 were re-treated, with the fraction declining to 21% during the second re-visit. The difference in re-treatment relative to the mean observed in Free villages, as well as relative to C2, are 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 endogenous. This is also confirmed by results from a linear probability model 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 covariates we used to explain purchase decisions in Table 3 (the results are available upon request). In fact, several demographic characteristics and measures of past malaria exposure are statistically significant predictors of contract choice.[26] However, we find that the estimated differences in re-treatment rates are overall similar once we include in the regressions all these other correlates (see columns 2 and 4 of Table 5).

Overall, re-treatment rates were very high when offered for free. The fraction of nets with insecticidal capability should have therefore remained very high over time, increasing the protective power of nets. In MF communities, on the other hand, not only were significantly fewer ITNs distributed, but the fraction re-treated was also lower, largely due to the low re-treatment rates among households who chose the non-commitment product C1. The results, albeit nonexperimental, suggest that there is some scope for improving compliance with prescribed behaviors by designing protocols that "commit" households in a financial sense to a particular course of action. This is consistent with similar findings regarding savings behaviors in developing and developed country contexts (Bryan et al. 2010).[27]

3.4 Impact on Health Outcomes

Next, we finally move to the main outcomes of interest, that is, the measures of hemoglobin levels (Hb) and malaria infection detected through finger-prick blood rapid diagnostic tests (RDTs). Our prior hypothesis

[25] A sizeable fraction of the shortfall was accounted for by nets being no longer with the household. Re-treatment rates increase to 94 and 91% in the first and second re-visit if we exclude households where at least one net was reported as having been sold, stolen, lost or otherwise not available for re-treatment. Whether the ITN is still with the households is, however, likely to be endogenous and we do not report these results in the table.

[26] This result is opposite to what was 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.

[27] Mahajan and Tarozzi (2011) exploit the features of the contracts, along with information on time preferences and beliefs about future health events, to identify time-inconsistent preferences in the context of a dynamic discrete choice model with hyperbolic discounting.

was that the considerable increase in ITN ownership rates would have led to significant improvements in health outcomes, especially in areas with free distribution. Unfortunately, we will show that such improvements did not take place.

As a reminder, at baseline the population targeted for testing comprised pregnant women, all U5s as well as their mothers, and a randomly selected adult (age 15-60). In the post-intervention survey, the availability of additional funding allowed us to include all household members in the target population. Overall, blood testing at follow-up was performed on 75% of panel household members, while 19% were not tested because they were not present during the visits, and 6% refused. In Appendix A.3 we show that both refusal and absence were balanced across experimental arms, and that testing success was significantly higher (∼90%) for U5s and women 15-45. Adult men were the least likely to be tested, mostly because they were most likely to be at work during the survey.

The ITT estimates of the program impact on malaria prevalence and anemia 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*.[28] Second, Hb levels, measured in grams per deciliter of blood. Third, the prevalence of anemia, defined as Hb$<$ 11g/dl. We estimated these impacts using two alternative samples. The first included all blood tests completed at follow-up, regardless of whether the individual had been tested at baseline, giving a sample of 7,154 individuals tested for malaria and 7,149 for Hb levels.[29] Next, we show DD estimates using, by construction, only information from individuals tested in both surveys, giving a sample of 1,896 observations for malaria and 1,869 for Hb levels.[30] All regressions were then estimated using individuals as the unit of observations, but the standard errors are as usual clustered at the village level.

Among all individuals tested at follow-up in control areas, 18.3% tested positive for malaria. Prevalence was 22.7% and 22% in Free and MF communities respectively. Malaria prevalence is therefore about 20% *higher* in intervention communities, although the null of no difference between each intervention arm and control areas cannot be rejected at standard levels.[31] Because of the positive point estimates and the relatively tight standard errors, we can also reject the null hypothesis of large reduction in malaria prevalence in intervention relative to control areas. The lower bound of the 95% confidence interval for β_{Free} is -0.022, which corresponds to a 12% reduction in malaria prevalence relative to control areas. Similarly, the corresponding lower bound for β_{MF} (-0.026) would imply a 14% lower prevalence than in control villages. Many earlier RCTs evaluating the impacts of ITN adoption found reductions in prevalence substantially larger than these lower bounds (see Lengeler 2004, Appendix 8 and 9).

The higher prevalence in Free and MF areas could have been explained in part by pre-intervention

[28]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.

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

[30]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.

[31]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).

differences. The figures in Table 1 show that before the intervention malaria prevalence in Free and MF villages was respectively 7% and 14% higher relative to control areas, although the differences were small in magnitude and not significant at standard levels. However, the DD estimates, which only include individuals tested both before and after the intervention, are similar to the results in levels. Relative to baseline, malaria prevalence in control areas increased from 11 to 17.3%. The overall increase in prevalence was expected, because the baseline survey was completed during the hot and dry months of spring, when malaria prevalence is lower, and the follow-up survey during winter, when malaria prevalence is generally higher in Orissa (Sharma et al. 2006). Consistent with the results in levels, the increase in prevalence was 5 percentage points higher in Free communities and 6 percentage points higher in the MF arm, although again the differences are not significant at standard levels. When we calculate mean changes in malaria prevalence within villages, we find that prevalence declined in only 11 of 47 control, 9 of 47 Free and 8 of 47 MF villages, while we observe increases in prevalence in 20 control, 27 Free and 30 MF communities, and no change in the remaining locations. The lack of a relative decline in malaria prevalence in intervention areas was most surprising for areas where ITNs were distributed free of cost, where we have documented very large increases in ITN ownership and (self-reported) usage, as well as very high rates of net re-treatment.

Looking now at hemoglobin levels, when we use all follow-up data, Hb levels were on average 11.4 g/dl in control and Free villages, and 11.5 in MF communities. The estimated impacts are therefore close to zero and not significant at standard levels (column 3). The DD estimates are the only instance where we find some evidence of positive impacts of the intervention, although only in Free villages: mean Hb increased by 0.28 g/dl in control areas, 0.32 in MF and 0.50 in Free villages. The DD between Free and control areas is significant at the 5% level, although its magnitude is small and just above 10% of a (baseline) standard deviation.[32] When we look at anemia prevalence, we find that it was 38.4% in control areas, 39.4% in Free and 38.9% in MF villages (column 5). Anemia prevalence was then close to identical across experimental arms, and the differences are never significant at standard levels. Consistent with the increase in mean Hb, the DD estimates show that anemia prevalence decreased by 11.1 percentage points in control areas (column 6). This was a large decline ($\sim$20% relative to baseline levels), and is significant at the 1% level. In Free communities, we find a 2.4 percentage points additional decline in anemia relative to control areas, but the difference is not, unlike for Hb, statistically significant. The decline in anemia prevalence was instead smaller in MF villages relative to control areas, although the difference, equal to 3.5%, is not significant. However, the null of equal change in MF and Free communities is rejected at the 10% level.

A key 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. Among adult males (age 15 or above), malaria prevalence was $\sim$15% and almost identical across arms (panel A). Among U5s, prevalence was 11% in control villages but about twice as large in intervention communities: 18.4% in Free and 19.8% in MF villages. However, the

[32]The increase in Hb, despite the overall increase in malaria prevalence, is likely due to better nutrition during the follow-up survey, which was conducted in a period when household income is seasonally higher for many households. 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.

estimates are imprecise, and the difference relative to control is not significant at standard levels, although the p-values are relatively small (below 0.2).[33] Prevalence among males is highest among 5-14 boys, where in each arm it is ∼15 percentage points higher than for younger children, so that the differences among groups are almost identical in the two age groups. These patterns change when we look at females (panel B), although again differences between arms are never significant at standard levels. Among females, we observe almost identical prevalence across arms among the youngest girls (∼15%) and higher prevalence in intervention villages in older age groups. In each experimental arm, the highest prevalence is observed among females of age 5 to 59. Overall, these results document remarkable differences in malaria prevalence across sub-groups, but these differences are largely concentrated between genders or across age groups rather than across experimental arms.[34]

Consistent with the baseline results, the results for anemia (panels C and D) show large systematic gaps across gender-age groups. In particular, these results confirm the U-shape of anemia prevalence with respect to age for both genders, as well as the significantly higher anemia rates among females 5 and older relative to males of the same age. Like for malaria, however, the differences in anemia prevalence between arms are small and never significant at standard levels.

4 Interpretation and Discussion

Given the published evidence regarding efficacy 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 associated with increases in ITN ownership and use in our Free and MF villages. Indeed, one of the primary objectives of our study was to evaluate to what extent a program of ITN sales on credit could replicate the benefits of free distribution. Overall, our results show that micro-loans were partly successful at increasing ITN ownership and usage. Although we found statistically significant increases in ITN ownership and usage in MF villages relative to control areas, the increases remained well below what was achieved with free distribution. Despite this, malaria indices remained similar between MF and Free communities and more surprisingly, such outcomes did not improve relative to control areas. Why did this happen? In this section, 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 for why improvements in malaria indices were not seen is that the availability of a larger number of ITNs led to unexpected behavioral responses leading to perverse outcomes. On the one hand,

[33] Details of the test statistics are available upon request from the authors.

[34] Note also that, consistent with the baseline results, we do not observe prevalence rates monotonically declining with age. The relatively low prevalence among U5s is actually driven by very low rates among children less than two years old (results not in the figure). Of a total of 263 children in this latter age group, only 12 (4.6%) tested positive, while prevalence jumps to 23.3% among the 412 two to four years old tested. Overall, in our sample malaria prevalence peaks among 5 to 10 years old, and then gradually declines with age. These patterns are similar among experimental arms.

sleeping under an ITN provides a mechanical barrier against mosquito bites: an important reason for the successful reduction in malaria burden in several ITN studies is the late-night biting habits of most anophelines (Pates and Curtis 2005).[35] 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, mosquito coils, or the control of drainage pools. It is possible that the broader availability of ITNs in Free and MF villages reduced the use of such alternative prophylactic measures. We tested this hypothesis 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." In each arm, 85% or more of respondents list mosquito bites as a cause of malaria. Overall, households in intervention communities appear to be about as knowledgeable regarding 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, and although in three of four cases it is one of the experimental arms that suggests the best knowledge. There was no systematic variation in malaria-avoiding behavior among groups (panel B). Bednets are by far the most commonly listed precaution, mentioned by 82-87% of respondents (with the highest proportions in intervention villages). The next two most common precautions are "avoid contaminated environment" (16-21%) and "avoid drinking contaminated water" (5-8%). For all indices, the test of equal means is not rejected at the 5% level, although the joint null of equality for all behaviors is rejected (p-value = 0.0421). However, the differences in alternative risk-avoiding behavior are not consistent with such behavior being more common in control villages, and indeed in several cases they indicate the opposite (for example, use of smoke or long sleeves, or cleaning of drainage pools).

Next, we analyze differences in residual spraying of indoors or outdoor walls (panel C). Spraying, like ITNs, is widely considered an effective tool in the fight against malaria (Mabaso et al. 2004, World Health Organization 2006). In 2008-09, 36% (38%) of respondents reported that spraying of inner (outer) walls had been done since the fall of 2007, when the ITN distribution took place. Although the null hypothesis of equal proportion among treatment groups 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 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 driven by household decisions, or if instead they resulted from choices made by public health officials who may have scheduled wall spraying taking into account our intervention. To evaluate whether differences in spraying rates help explain the lack of health benefits in

[35] Although we did not collect information regarding the species, number and feeding patterns of malaria vectors in our study villages, in areas within one of our study districts (Keonjhar), Sahu et al. (2009) found that biting activity of the major local malaria vectors was concentrated in the two middle quarters of the night, regardless of the season.

intervention villages, we re-estimate the ITT models in Table 6 including dummies for recent wall spraying among the regressors. In columns 3 and 4 of 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 lack of effect on malaria or anemia prevalence cannot be explained by the presence of other ITN distribution programs, possibly sponsored by the Government or by other NGOs. First, the results on net ownership in Table 2, which showed large increases in ITN ownership rates in treatment versus control areas, 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. As expected, given the supply of BISWA ITNs, we also find that fewer nets were purchased from the market in MF and especially Free villages.

Even though preventative behavior did not change, it is possible that there were systematic changes in treatment behavior across experimental arms that can explain our results and are consistent with ITN effectiveness. In particular, it could be the case that households in Control villages were treating malaria effectively using drugs, so that on average outcomes were no different from those in treatment villages. We think that this is unlikely for a number of reasons. First, we find that conditional upon having a fever, the rate at which treatment was sought was the same across all three arms. Second, in order for differential treatment to explain the lack of health improvements, individuals would first need to recognize the need for a treatment. However, we find that across all arms the vast majority of malaria cases were asymptomatic. In each experimental arm, less than 2% of RDT-positive individuals had been identified by respondents. Third, the least expensive and widely available form of malaria treatment in the study area, chloroquine, is no longer effective against the disease, due to widespread resistance in Orissa (Satpathy et al. 1997, Ranjit et al. 2009). More generally, the Indian government's National Drug Policy for Malaria no longer recommends presumptive treatment with chloroquine and calls for no treatment of *Pf* cases with chloroquine. In sum, we conclude that the lack of health improvements cannot be attributed to differential treatment regimes.

4.2 Measurement Error in Malaria Indices

Another possible reason for the lack of observed impact on malaria indices was errors in reading the RDTs. On the one hand, the RDTs we used to detect malaria infection have been shown to have very high sensitivity (the probability of detecting a positive correctly) and specificity (the probability of detecting a negative correctly). On the other hand, the interpretation of RDTs presents a degree of subjectivity that could lead inexperienced readers to make errors: 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 *Plasmodium* antigens (histidine-rich protein 2, or HRP2) 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 of the Binax RDT (Khairnar et al. 2009), a degree of subjectivity is hard to rule out completely, because the lines can sometimes be difficult to detect when parasitemia is low. 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, but during the field work RDT results were not confirmed with microscopy, so we cannot gauge directly the nature and extent of measurement error. However, measurement error seems an unlikely explanation for the lack of health benefits from our program, for a number of reasons.

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).[36] As prevalence tended to be *higher* in treatment areas, misclassification would more likely have led instead to *underestimation* of the differences.

Second, we carried out a small validation study after the conclusion of the follow-up survey (in July 2009) in collaboration with the Malaria Research Centre (MRC) Field Station in Rourkela (Orissa), which confirmed the accuracy of the RDTs. A total of 205 blood samples were independently collected from the MRC team from individuals with malaria symptoms from three villages. The RDT cards were interpreted by three different blinded readers, that is, two of the testers who were part of the field team during our study, and the most senior survey monitor in our research team. These results were then compared with thick and thin blood smears read with microscopy by the MRC team for the same samples, with the smear result accepted as the correct infection status. The results showed very high sensitivity ($> 90\%$ for each of the three readers, see Table A.15 for details). The fraction of correctly identified negatives (specificity) ranged from 74 to 85%. The lower specificity (higher prevalence) measured by the RDTs relative to microscopy was not surprising, given that these tests often detect the presence of the *P. falciparum* antigens up to 2-4 weeks after parasitemia has cleared (Humar et al. 1997). The RDT results were then very similar but not identical between readers (pairwise 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 was declining in treatment villages over the course of the study, the likelihood of fainter, harder-to-detect test lines may have increased in these areas, possibly *overestimating* the reduction in prevalence. Finally, measurement error was unlikely to be a problem for the Hb testing, which also showed little evidence of differential changes across experimental groups. Even in this case erroneous testing cannot be ruled out entirely, but measuring Hb simply requires reading a number from the display of a HemoCue machine. In addition, the strong cross-sectional correlation between malaria infection status and hemoglobin levels supports the reliability of the malaria RDTs. 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 a salutary effect on malaria indices observed in our data reflect the reality in our sample, and is not the result of imperfect measurements.

[36] 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.

4.3 Reliability of Reporting of ITN Ownership and Usage

An alternative possibility for the lack of impact on malaria indices 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 the possibility 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 discussed earlier were obtained using information on bednet usage recorded separately for each individual included in the household roster. However, bednet 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 spaces. 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 whether the net was in good condition, hanging properly, and recognizable as one of those distributed by our program. We can therefore re-estimate the ITT for ITN usage the night before the survey using this alternative source of information.

The results in column 1 of Table 9 show that these alternative 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, with most missing data due to the temporary absence of the member the previous night. The similarity of the results reflect very strong concordance between the two data sources regarding bednet usage. 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. In addition, such concordance is not simply due to the respondent reporting every household member as either using or not using nets the night before. If that had been the case, it would have been easy for the respondent to mis-report usage and still produce high consistency between the two data sources. Although most of the variation in bednet 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 noisy indicator of consistent bednet usage, and there 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 check the actual presence of the bednets mentioned (regardless of reported usage) by the respondent. In a large majority of cases the surveyor was allowed to see 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, usage rates of nets observed by the surveyors are only slightly lower relative to the reports unconditional on observation (column 3). Additional evidence

that our intervention improved the availability and quality of ITN protection is provided in columns 4 and 5. 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 points higher in Free communities. This was due to usage of program ITNs, which were of good quality and relatively new. In column 5 we see that virtually no BISWA nets were present in control areas, which also confirms the absence of cross-arms contamination. In MF villages, 14% of individuals used program ITNs (observed by the enumerator) the night before the survey. This corresponds to about half of all nets seen by the surveyors and reported as having been used the night before in MF villages (the total was 0.144+0.127=27.1%, see 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 observed nets used the previous night (the total being 0.144+0.360=50.4%).

In addition, the large number of ITNs retained by households is confirmed by the high re-treatment rates, especially in Free villages (see Section 3.3). Finally, only between 2 and 6% of the nets reported as having been used the night before were seen hanging properly within the dwelling. This may have been largely because interviews were done during the day, when nets are usually stored away to avoid being damaged, and to increase living space in what are often small dwellings, and where sleeping and living environments frequently coincide. In the end, our analysis strongly suggest that our program did increase considerably both the availability of ITNs (especially with free distribution) and ITN usage.

4.4 ITN Coverage

The most likely explanation for the lack of impact of our intervention on malaria indices is that even in villages with free distribution, the fraction of sleeping spaces protected by ITNs remained low. Recall that only BISWA clients receive free ITNs or the offer of ITNs for sale on credit. Although BISWA has a large presence in the study area, we estimate that on average only 20% of people live in households with at least one BISWA affiliate and thus were eligible for the study.[37] Low coverage rates are common in public health programs that only distribute free ITNs to pregnant women and young children. Such high-risk demographic groups are consensus targets, for instance, according to guidelines of the World Health Organization and the US President's Malaria Initiative (USAID-CDC 2005, World Health Organization 2007). The targeting of vulnerable groups is justified on the ground that regular usage of an ITN secures some protection to the individual user. On the other hand, it is now accepted that the externalities offered by mass adoption of ITNs are a key factor for ITN efficacy, although the relative role of personal versus mass protection of ITNs is not yet well understood (Binka et al. 1998, Hawley et al. 2003, Killeen et al. 2007). Reductions in malaria indices have been documented among non-users of ITNs living within a few

[37] We estimated 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 $\hat{s}_v(m_v/\hat{b}_v)/p_v$.

hundred meters of communities covered by mass distribution of ITNs. In our intervention, study villages were scattered spatially over a very broad geographical area (see Section 2), so cross-village externalities are not plausible. Rather, our conjecture is that the increase in ITN ownership and usage rate *within* study villages was not sufficiently large to dent the cycle of malaria transmission. To probe this hypothesis, we first look at the relationship between village-level coverage and changes in 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 estimated village-specific changes in malaria prevalence in all intervention communities. We then plot the results against a measure of village-wide ITN coverage, calculated as the ratio of the total number of ITNs distributed to BISWA households (regardless of their inclusion in the survey sample) and village population counts from the 2001 Indian Census. Although not up-to-date, the population counts are a good proxy for current population, and if anything, in most cases 2001 population would underestimate current population, so that our estimates may overstate true coverage. The results are displayed separately for MF and Free communities in the two panels at the top of Figure 8. Each graph also shows the fitted values of two OLS regressions, one where we include data from all villages (the continuous line) and the other where we exclude the very few villages where the ITN coverage ratio was larger that 0.35 (the dashed lines).

Panel A shows that, when we include all Free villages, there is a *positive* association between malaria prevalence at follow-up and program coverage. The estimated slope (0.59) is actually significant at the 1% level. However, the results are driven by the three outlier villages with coverage > 0.35, and when we exclude them the slope becomes negative but very close to zero (-0.02) and not significant at standard levels (p-value = 0.966). In MF villages (panel B), where substantially fewer ITNs were distributed, the slope of the regressions are negative but we cannot reject the null that slopes are zero at standard levels, although when we include all villages the slope is almost significant at the 10% level (p-value = 0.103).

Because the ITN coverage achieved in MF communities was endogenously determined by household purchase decisions, its association with changes in malaria prevalence should not be interpreted as necessarily causal. In communities with free distribution, the number of ITNs delivered was instead decided by our research team based on household size and composition. This produced variation in ITN coverage resulting only from the distribution of BISWA affiliation and household composition within the community. Even so, BISWA affiliation could be associated with village characteristics related to malaria prevalence, although 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). On the one hand, the fact that the dependent variable in panels A and B is the *change* in prevalence, eliminates any possible spurious correlation due to time-invariant (observed or unobserved) village-level characteristics. On the other hand, there may be other unobserved differences in trends correlated with both ITN coverage and malaria prevalence. To address this concern, in panel C of Figure 8 we look at the relationship between changes in prevalence and the fraction of the population affiliated to BISWA in control villages ("BISWA penetration"). No ITNs were distributed in these communities, but by construction BISWA penetration is very strongly correlated with the measure of ITN coverage that would have been observed if ITNs had been distributed as in Free communities.

Indeed, the correlation between the two variables in Free villages is 0.95. The graph in panel C shows no clear association between changes in malaria prevalence and BISWA penetration. This suggests, albeit indirectly, that the lack of an association between changes in prevalence and ITN coverage in Free villages (panel A) is unlikely to be caused by differential trends in prevalence across communities with varying degrees of BISWA penetration.

As an additional check, we use Control and Free villages to estimate an OLS regression of the village-level change in prevalence on BISWA penetration, the Free dummy, and the interaction between the two variables. If ITN coverage were causing declines in malaria prevalence in our sample, we would expect the coefficient on the interaction to be negative. Consistent with the results in panel A, we find instead that the coefficient is positive and significant when we include all 94 villages (= 1.8, p-value= 0.009), and close to zero and not significant (= 0.25, p-value= 0.770) when we exclude the three villages with coverage larger than 0.35. Overall, we conclude that in our sample the coverage of the intervention did not appear to be systematically related to 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. The unique features of our study make comparisons difficult, but these very differences in design likely hold the key to explain the different findings. In particular, in all but one of the 14 cluster RCTs surveyed in Lengeler (2004), the number of ITNs distributed was sufficient to ensure that almost all sleeping spaces were protected by nets in treatment communities. For instance, in the largest of these studies, ITNs were delivered in number sufficient to cover all sleeping spaces in 113 of 221 communities in Kenya (ter Kuile et al. 2003). In the RCT conducted 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 D'Alessandro et al. (1995). In this study, re-treatment with permethrin was attempted with all bednets in half of 104 villages in The Gambia, but only ∼60% of children under the age of four slept regularly under ITNs. Interestingly, the program was unsuccessful in reducing child mortality in the one stratum (out of five) where coverage and usage rates remained relatively low. Our program never achieved more than 50% coverage and in most cases covered a significantly lower share of the population (Figure 8). The low coverage may have played an important role in the lack of effects on malaria if benefits arise only beyond certain thresholds in coverage. The existence of non-linearities in the coverage-benefits relationship is indeed strongly suggested by 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 were seen in compounds near intervention areas where 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. It must be kept in mind, however, that our data are not suited to test this hypothesis formally. 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.

A better comparison for our study might be the eight additional studies reviewed in Lengeler (2004) where the impact of ITNs was evaluated using a within-community randomization. In such studies, the intra-community coverage rates were low, but large reductions in malaria indices were observed nonetheless. However, these studies involved intense monitoring of net usage and/or health outcomes, including a combinations of nightly surprise visits and frequent (sometimes daily) health checks. Such study design could have induced behavioral responses such as increased compliance with regular ITN usage.[38] Conversely, our study involved a brief information campaign in fall 2007 and two rounds of blood tests (at baseline and follow-up) separated by about 18 months, while no permanent structure was in place to ensure continuing compliance with ITN use and to monitor the health status of the study population. Although we have argued that previous-night self-reported ITN usage data were reliable, it is not unlikely that reports on "regular" usage during the peak mosquito season were actually somewhat overstated.

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 by highlighting the need to examine the relative importance of the two distinct channels (individual usage and externalities) through which malaria reduction occurred in these studies.

5 Limitations and Conclusions

The primary objective of this study was to evaluate whether sale on credit could increase ITN ownership and usage, and in turn decrease the burden of malaria in a poor rural area of Orissa (India) where existing markets and public health interventions have not been successful at ensuring adequate ITN coverage. Across the 47 MF villages, our program succeeded in selling ∼1,100 ITNs on credit to BISWA clients over a few months. The micro-loan program increased ITN ownership substantially relative to control areas, with 52% of sample households purchasing at least one net. This was despite the relatively high price of the ITNs, about 3-5 times the daily agricultural wage in the study area. The increased ownership rates were also associated with large increases in (self-reported) use. At follow up, 16% of individuals were reported as having slept protected by an ITN the night before the survey, compared to only 2% in control areas. Regular usage rates during the peak mosquito season was only 6% in control areas, but it increased to 36% with micro-loans, an increase of over 500%. These purchase rates are substantially higher than those in earlier studies who documented very low cash purchases of health products among poor households, despite heavy subsidization (Ashraf et al. 2010, Cohen and Dupas 2010, Kremer and Miguel 2007 and Kremer et al. 2009). Hence, our work suggests that micro-loans should be considered as a potentially effective tool to increase uptake of health products in poor areas, when free provision is not possible or desirable (although the lack of impacts on malaria indices in our study areas, which we will review below, adds an important warning).

A number of caveats should, however, be taken into account when interpreting the relative success

[38] 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.

of our micro-loans intervention at increasing ITN ownership rates. First, not everyone repaid the micro-loans in full. At the time of the follow-up survey, 1 to 1.5 years after the sales, we estimate that 49% of sample households had repaid the loan in full, although one in every five (20%) had not repaid anything. On average, the fraction of the loans repaid among sample households was 64%: assuming no further repayment, the MF program corresponded to an average subsidy of 36% on the full price of the ITNs. The cost effectiveness of micro-loans is further reduced by the presence of fixed distribution costs at the village level. We estimate that for every $60, an additional 2 extra ITNs could have been distributed using micro-credit, relative to free provision. However, because ITN coverage was much lower under micro-credit, and to the extent that externalities from mass distribution are an important source of ITN protective efficacy, cost-recovery may be suboptimal.

Second, although our study area comprised a large number of villages spread across a very wide geographical area, the study population is not a representative sample of the five districts where we operated. Our study villages were selected because BISWA already had a presence there, and only BISWA clients were eligible for the intervention. Therefore, our study does not identify the impacts of introducing sales of ITNs on credit within a population with no access to BISWA's credit network. Extending sales to non-BISWA clients within our study communities could have increased the overall coverage of ITNs within the village, but our data are silent about this. In addition, data collected at the village level during the delivery of ITNs, in the fall of 2007, indicate that uptake was larger among households included in our baseline survey relative to non-sample households. So our results, while valid for our sample, may over-estimate the overall impact of micro-loans on uptake and usage at the village level in our study areas.

Third, our results do not imply that liquidity constraints were the only reason for the low ITN ownership rates observed before our intervention. In fact, our research design does not experimentally identify what demand would have been if the same ITNs had been *only* offered for cash. However, almost all households who purchased ITNs did so choosing the loan contract, and only a handful purchased cash. Hence, the offer of micro loans plausibly played a crucial role in explaining the relatively high uptake. In addition, there was a strong negative association between per-capita expenditure and purchase, which is also consistent with the presence of credit or liquidity constraints. In principle, it is possible that households purchased ITNs on loan not because they were liquidity constrained but because they had alternative investment opportunities for their cash that yielded a return higher than the BISWA interest rate (20% annually). However, in that case (and in the absence of investment ceilings) one would have expected households to be maximizing their BISWA borrowing. Although we cannot rule out this possibility completely, we find that only about 14% of households had a current BISWA loan at follow up (excluding the ITN loan).

Finally, the increase in ITN ownership and usage observed in micro-loans communities remained significantly below that achieved with free distribution. In these latter locations, 47% of individuals were reported as having slept under an ITN the previous night. This is 31 percentage points more than in MF villages. The fraction reported as sleeping regularly under an ITN during the peak mosquito season was 77%, that is, 41 percentage points more than in MF villages. The possibility of substantial cost recovery offered by micro-loans needs therefore be traded-off with product uptake which is likely to remain well below universal. Although (unlike Cohen and Dupas 2010) we find that micro-loans also led to significant

screening based on malaria risk, such non-universal purchase rates likely limit the possibility of reaching mass coverage, which in some situations may be essential for the success of health campaigns.

Next, we also found evidence against the hypothesis that, conditional on ownership, ITNs that had been purchased on credit were more likely to be reported as having been used the night before the follow-up survey relative to ITNs received for free. Our findings are thus consistent with analogous conclusions found for ITNs in Kenya (Cohen and Dupas 2010) and for water disinfectant in Zambia (Ashraf et al. 2010), although we find that free ITNs were actually being used *more* than ITNs purchased on credit. In addition, we document that, in communities where ITNs were sold on credit, regular re-treatment with insecticide was significantly higher among households who decided to "commit" to re-treat by purchasing ITNs bundled with a sequence of two re-treatments. Although contract choice makes pre-commitment clearly endogenous, we find that the gap in re-treatment rates remains similar if we control for a large number of household characteristics which also include proxies for preference parameters. These results suggest that in situations where public health programs call for cost-sharing and also require compliance with certain behaviors over time, the inclusion upfront of any monetary costs of such behaviors may increase compliance. Despite its non-experimental nature, we think this result has potentially important policy implications and deserves further examination.

Finally, perhaps the most surprising result of our study is that we found close to no improvements in either malaria prevalence or hemoglobin levels, not even among households which received nets for free. At follow-up, malaria prevalence was 18% in control villages, but it was 4 percentage points *higher* in MF and Free communities, although the null of equality cannot be rejected at standard levels. The estimates are also sufficiently precise that we can reject the hypothesis of improvements in prevalence as large as those documented in several earlier studies on the health impact of ITNs. Hemoglobin levels and anemia prevalence were almost identical among experimental arms: we only find a statistically significant increase in hemoglobin levels with free distribution as compared to control areas (p-value = 0.038), when we estimate a DD model for the subset of individuals tested both at baseline and at follow-up, but the change is small in magnitude.

All malaria indices were measured with blood samples analyzed with RDTs. Our prevalence measures are thus likely accurate, although additional measurements could have helped in identifying impacts. While our RDTs assessed the prevalence of *Plasmodium* parasitemia, we did not assess clinical disease, that is, episodes of malaria illness accompanied by recognizable symptoms. In a partially immune population (such as in our study areas in Orissa), clinical malaria is an important marker. In addition, we assessed malaria prevalence (the fraction of individuals who tested positive at a given time), but not incidence (the number of episodes during the study period), and did not conduct active surveillance for malaria during the course of the study. Detecting incident cases would have likely improved our case-finding for malaria, and provided a more accurate assessment of malaria burden. This may have resulted in a more sensitive detection of the impact of ITNs on decreasing malaria burden. Finally, our study was not designed to measure entomologic indicators such as anopheline density, biting rates and behaviors. We thus were not able to assess directly one of the key impacts of ITNs, and thus lacked a more direct measure of their community-wide protective effect. Although our indirect assessment of this (through malaria and anemia prevalence) suggests there

was little effect on the vector population, direct measurement could have provided better confirmation for one possible reason that our intervention did not reduce the malaria burden.

One additional limitation is that our study was conducted with standard ITNs, and not with the long-lasting insecticidal nets (LLINs) that are being increasingly used in many mass distribution campaigns, particularly in Africa. Although lower availability and higher cost for LLINs are still considerations in the decision of whether to use ITNs or LLINs in such programs, these factors are becoming less important, as LLINs become more available and less expensive with each passing year. The WHO now recommends that all mass bednet campaigns utilize LLINs (World Health Organization 2007). Use of LLINs in our program would have eliminated the issue of re-treatment, which our study examined in detail; the higher cost of LLINs could have altered the ownership and use decisions in ways difficult to predict. Crucially, LLINs may have provided a more reliable insecticide concentration on the ITNs in the field, given that they are factory pre-treated, more wash resistant, and do not need to be re-treated every six months. In our study, field staff treated bednets following guidelines from the insecticide manufacturer and the World Health Organization (World Health Organization 2002). Recent studies have shown that the insecticide used in our study, deltamethrin, was highly effective in Orissa (see Section 2.2). This suggests that resistance to the insecticide was unlikely to be a key factor in explaining the lack of health benefits. However, concentrations of the chemical decline over time, especially when the net is washed frequently. Towards the conclusion of the follow-up survey, seven ITNs that were re-treated approximately six months earlier were tested by gas chromatography. Of the seven ITNs, only two had deltamethrin concentrations that approached the level recommended for field-treated ITNs. While it is not unexpected to find low insecticide concentrations six months after re-treatment (particularly if the ITN has been washed multiple times), these tests suggests that the use of LLINs may have led to better health impacts.

While keeping these caveats in mind, we argue that the lack of improvements in malaria indices 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 rates of ITNs. Rather, we conjecture that the findings are likely explained by a combination of two factors. A first likely cause was the low ITN coverage achieved by our program, even in communities with free provision of ITNs, a result of our program targeting only households affiliated to the micro-lender BISWA. Second, our study design did not include direct monitoring of ITN usage, or 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 malaria indices. We emphasize that low coverage, coupled with low or no monitoring, are likely to mimic more closely actual public health interventions than studies carried out under ideal trial conditions. Unfortunately, although we argue that our conjecture is likely correct, our study was not designed to generate variation in monitoring and coverage.

Importantly, the unique features of our study design also imply that our results should *not* be interpreted as contradicting earlier studies on the efficacy of ITNs. 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, our results suggest that public health interventions which only achieve the distribution of a relatively limited number of ITNs may fail to achieve the desired effects. Much more may be needed, and efforts should include ensuring high village-wide coverage, providing incentives for regular use, and possibly adding complementary interventions such as indoor residual spraying, case management and environmental measures. 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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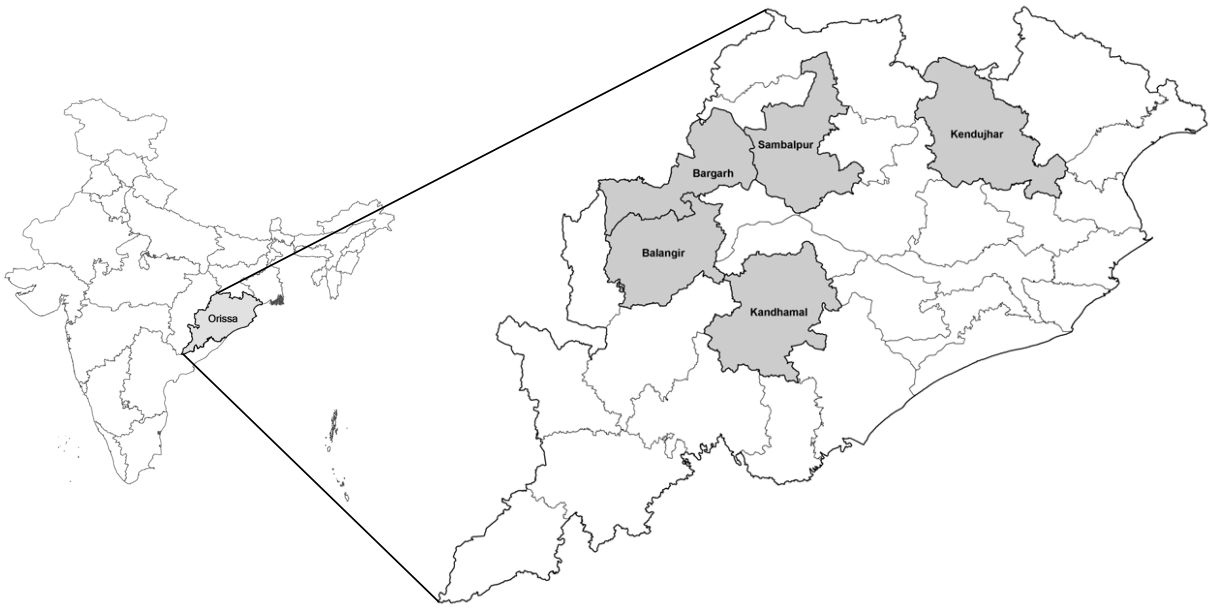

Figure 1: Study Areas

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

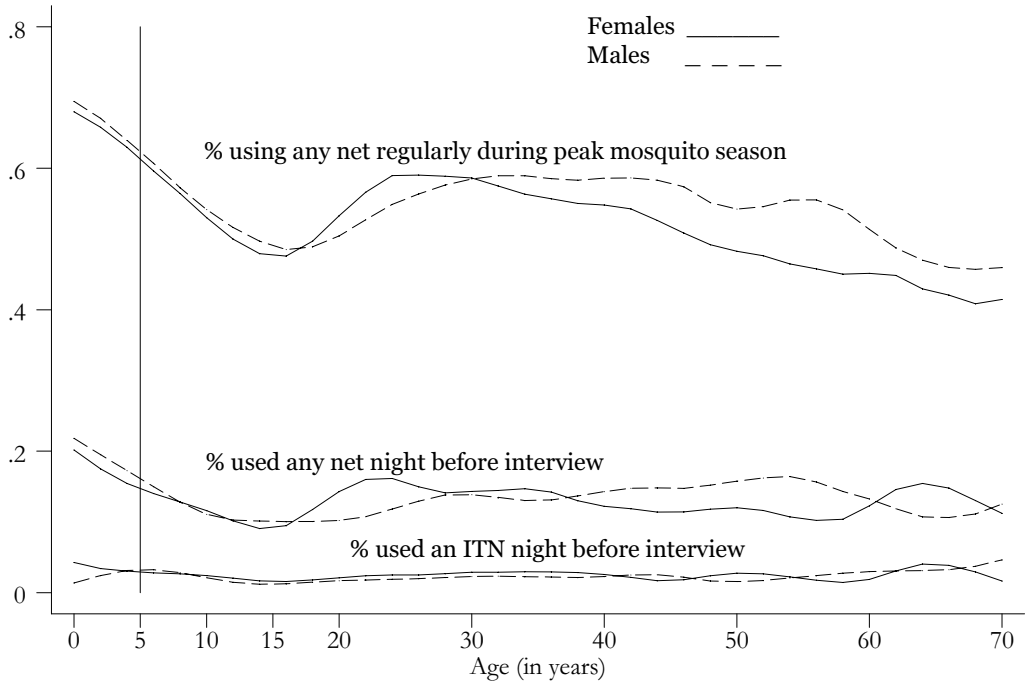

Figure 2: Net Usage at Baseline, by Age and Gender

Notes: Each line is a locally linear non-parametric regression of usage on age (in years). Regressions are estimated using a bi-weight kernel with bandwidth equal to 7. We categorize a household member as "using any net regularly during peak season" if the survey respondent responded "yes" to the question "Does [name] usually sleep under a bednet when there are lots of mosquitoes around?". 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.

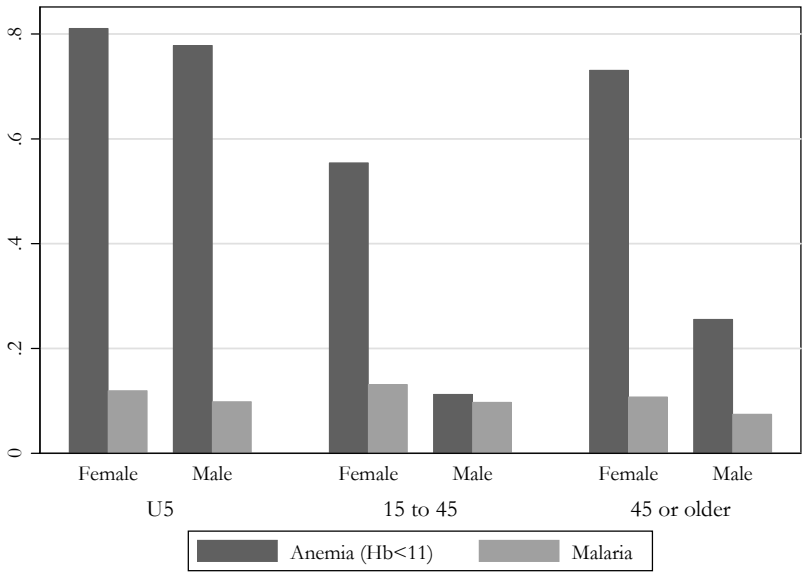

Figure 3: Malaria and Anemia Prevalence, by Demographic Group

Notes: The columns represent the results of blood testing for anemia ($n = 2,532$) and malaria ($n = 2,561$) prevalence.

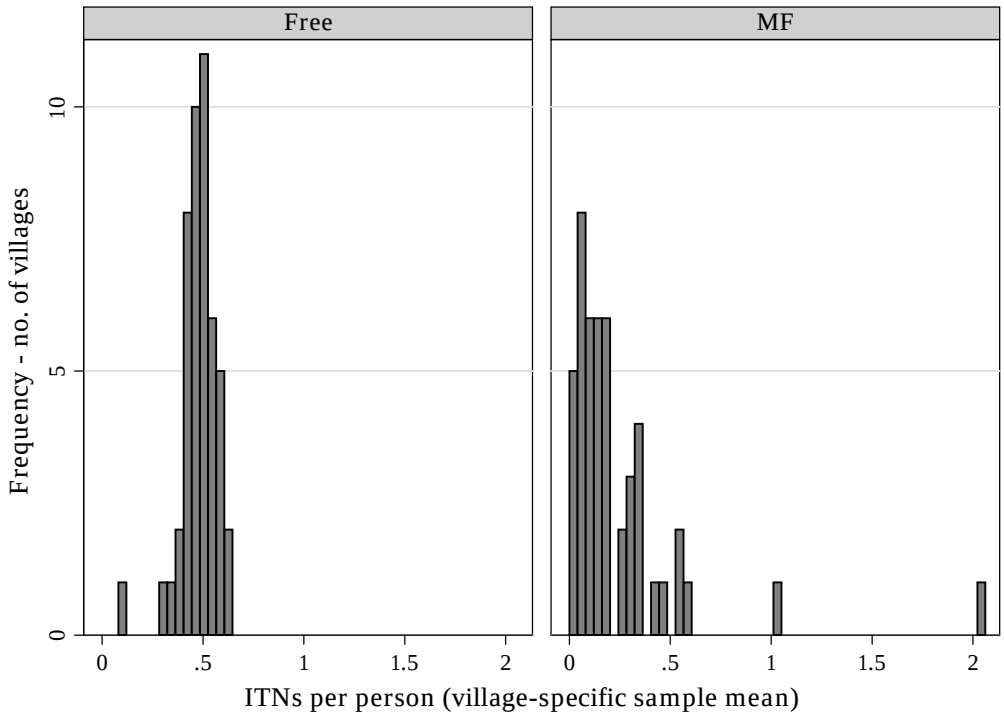

Figure 4: Mean Number of ITNs delivered per Head.

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 outlier in the left tail of the uptake distribution in Free villages is due to a large number of sample households not present during the visit. The two outliers in the right tail of the distribution in MF communities are due to a number of sample households who purchased a large number of ITNs for resale.

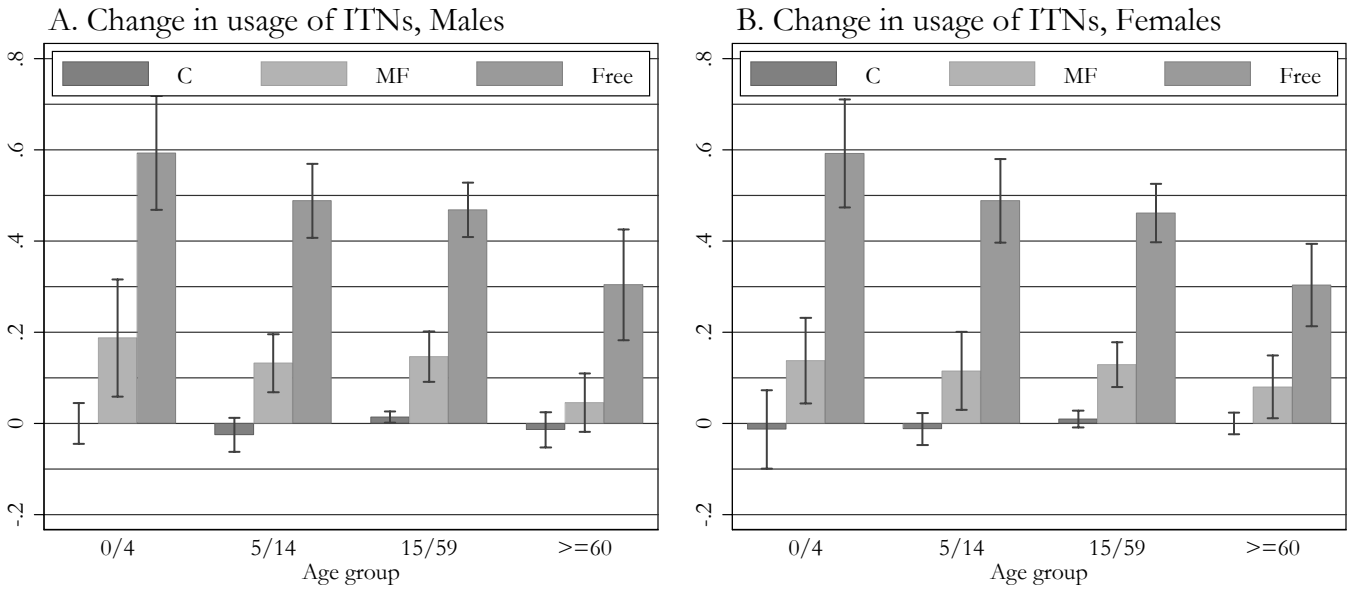

Figure 5: Changes in previous night usage of ITNs, by Age and gender

Notes: Each column shows the change from baseline to follow-up survey in the fraction of household members in a specific age-gender group who slept under an ITN 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.

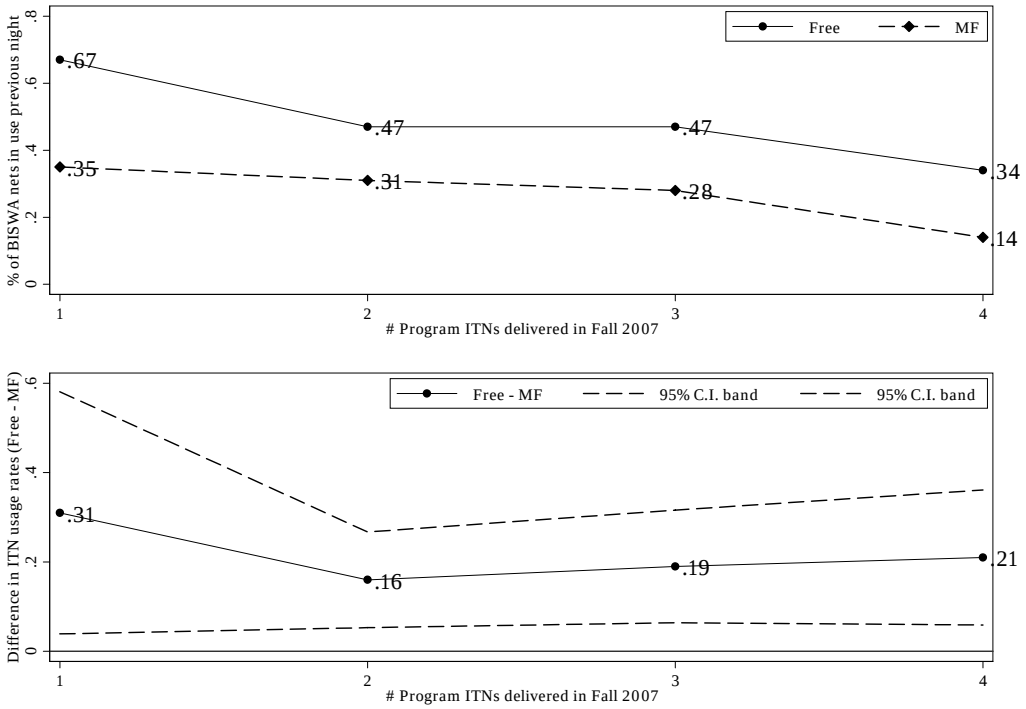

Figure 6: Fraction of observed BISWA nets used the previous night

Notes: Data on usage rates from post-intervention survey (winter 2008-09). For each household, usage rates are calculated as ratios, with the numerator equal to the number of BISWA nets reported as used the night before, seen by field staff during the follow-up survey, and identified by them as nets distributed through our program and the denominator is equal to the number of BISWA nets delivered to the household. 95% confidence intervals are robust to intra-village correlation (see text for details).

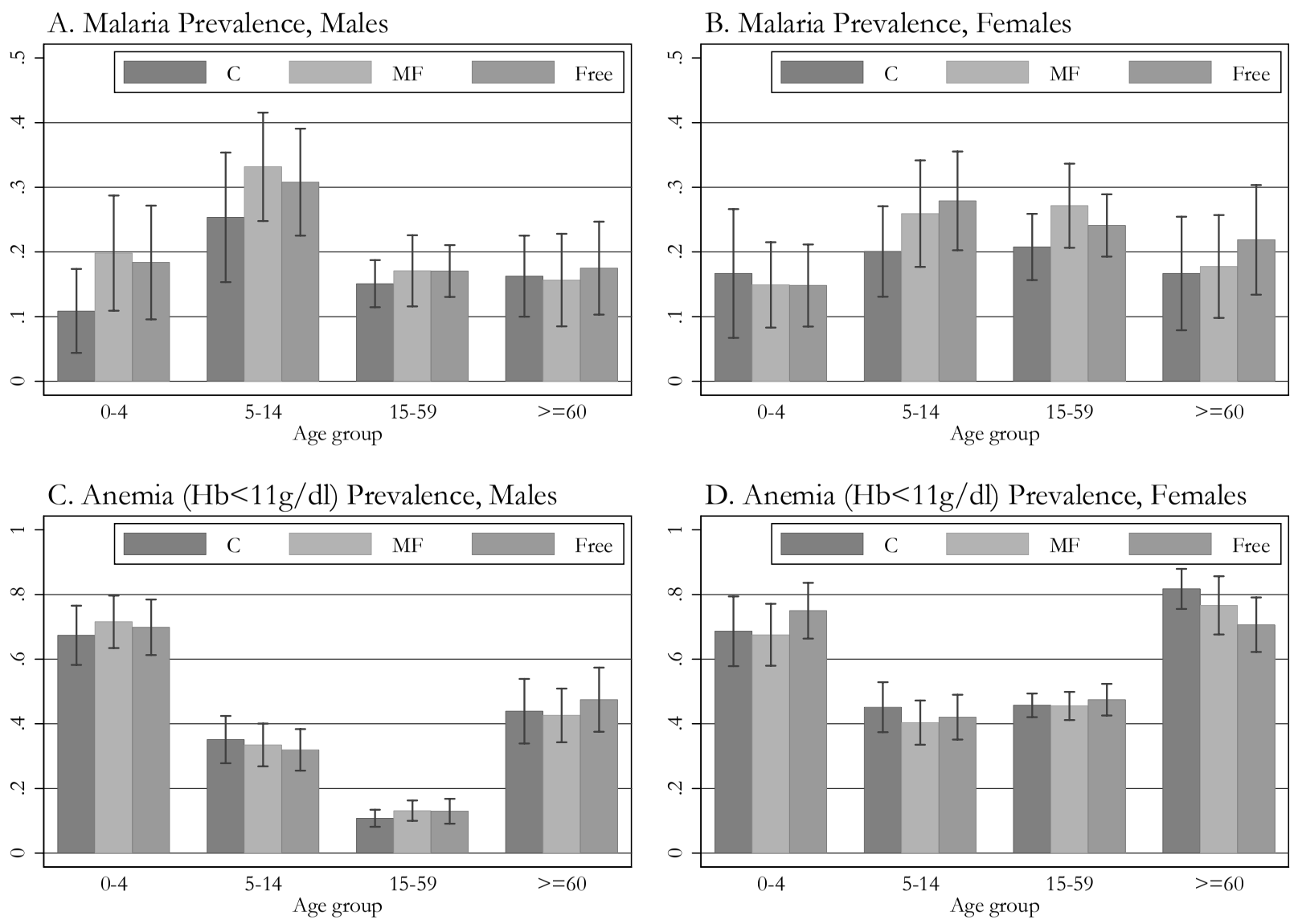

Figure 7: Post-intervention Malaria and Anemia Prevalence, by Age and Gender

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.

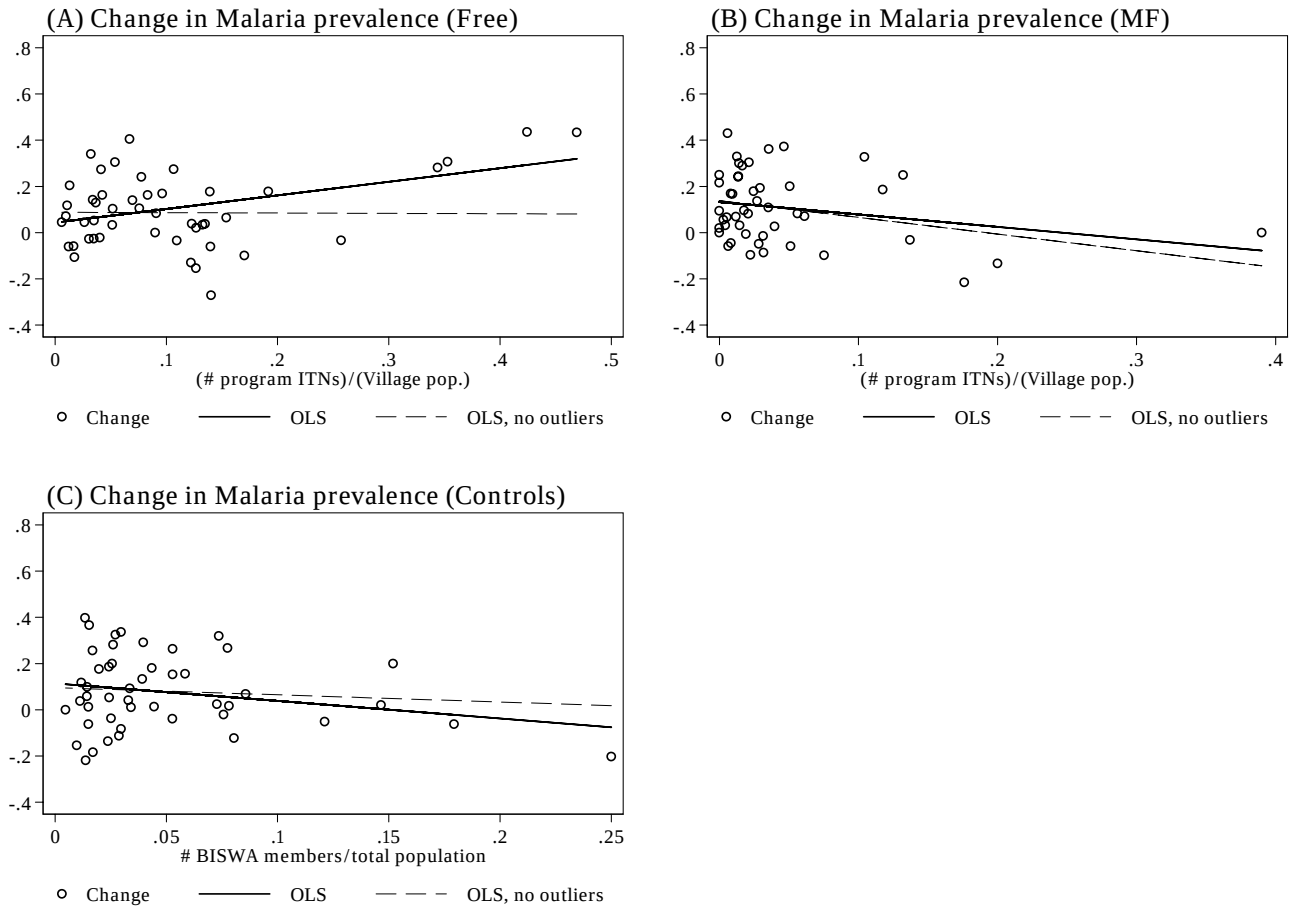

Figure 8: Malaria Prevalence vs. Intensity of ITNs Distribution

Note: Data from spring 2007 and winter 2008-09. Each circle in the graphs represents a village. The continuous lines in each graph show fitted values of a village-level OLS regression through the points. The dashed lines show fitted values when we exclude villages with coverage larger than 0.35 (graphs A and B) or with more than 20% BISWA membership (graph C). The point estimates of the slopes and the corresponding heteroskedasticity-robust standard errors (in parenthesis), using all villages or excluding outliers respectively, are respectively as follows: (A) .59 (.17) and −.02 (.37); (B) −.54 (.32) and −.72 (.54); (C) −.76 (.43) and −.31 (.53).

Table 1: Baseline Summary Statistics and Randomization Tests

	(1) Control		(2) Free		(3) MF		(4) p-value	(5) s.dev.
Scheduled Caste/Tribe/Other Backward Castes	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 (Rs per day)	22.3	(0.928)	21.2	(0.827)	24.2	(1.101)	0.085	7.9
Poor	0.195	(0.025)	0.24	(0.031)	0.196	(0.024)	0.463	0.408
Difficult/impossible for household to borrow Rs 500	0.525	(0.029)	0.529	(0.031)	0.518	(0.026)	0.953	0.500
Ratio 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	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 g/dl)	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. "Poor" is a dummy equal to one if per capita monthly household expenditure is below a poverty line equal to Rs $381 = 326 \times (373/319.5)$, where 326 is the official poverty line for rural Orissa in 2004-05, and 373 and 319.5 are the Consumer Price Index for Agricultural Laborers in May-June 2007 and July 2004-June 2005 respectively.

Table 2: Bednet Acquisition and Ownership

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ITN uptake (Fall 2007)				Any Bednet Ownership (Follow-up Survey, 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=1	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	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. All estimated coefficients in columns 1 to 8 are significant at the 1% level. 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 3: Correlates of ITN purchase

Dependent variable: at least one ITN purchased	OLS-LPM	
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)[1]	0.019	(0.011)*
% members who slept under net last night	0.209	(0.093)**
% members who slept under ITN last night	-0.053	(0.279)
# nets owned by household	0.007	(0.026)
# nets treated last 6 months	-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)[2]	0.014	(0.019)
% tested members positive to malaria	0.202	(0.080)**
% members with self-reported malaria episodes last 6 months	0.272	(0.116)**
Subjective $P(\text{malaria} \mid \text{untreated net}) - P(\text{malaria} \mid \text{ITN})$[3]	-0.066	(0.106)
Subjective $P(\text{malaria} \mid \text{no net}) - P(\text{malaria} \mid \text{ITN})$[3]	-0.140	(0.142)
Observations	513	
R-squared	0.11	

Notes: OLS estimates of a linear probability model with a binary dependent variable = 1 if the household purchased at least one ITN. 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. Sample size is smaller than the 589 panel households in MF villages because 76 observations (13%) have at least one regressor missing. Also included in the model 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, measures of risk aversion and intertemporal preferences. Risk aversion is measured by an indicator equal to one when the respondent chose a no-risk lottery from a list of different lotteries (played with real monetary payoff), differing in the expected value and variance of the reward. We evaluated time preferences with 12 questions where the respondent had to choose between an earlier reward and a later but larger one. The regression includes a dummy equal to one when the respondent always chose the earlier reward, and a variable recording the number of "preference reversals" implicit in the choices, which arise when an individual chose a reward at date t over a larger one at date $t + s$ but preferred the later reward when the two dates were shifted by an equal time period.
[1] Includes all actual expenses for in-patient and out-patient care, drugs, transportation and lost household earnings. [2] Expected total cost of a malaria episode for a working adult male, including all items listed above. [3] The probabilities were elicited by asking respondents to express the likelihood of an event by choosing an integer between zero (impossible event) and ten (certainty).

Table 4: Bednet 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: Re-treatment Rates

	(1)	(2)	(3)	(4)
	First re-treatment (spring 2008)		Second re-treatment (fall 2008)	
	All	All, adding controls	All	All, adding controls
MF, "Commitment contract" (C2)	-0.08	-0.06	-0.09	-0.19
	(0.030)***	(0.028)**	(0.054)*	(0.084)**
MF, C1	-0.56	-0.50	-0.62	-0.58
	(0.059)***	(0.055)***	(0.059)***	(0.062)***
Intercept (Free)	0.92	-	0.83	-
	(0.013)***		(0.016)***	
Includes baseline characteristics	No	Yes	No	Yes
H_0 : C1=C2				
Difference	0.48	0.50	0.53	0.39
p-value	0.000	0.000	0.000	0.000
Observations	875	781	875	787
R-squared	0.346	0.373	0.293	0.358
Clusters	89	88	89	87

Notes: Data from first (spring 2008) and second (winter 2008) re-treatment of bednets in Free and MF communities only. The regressions are estimated using only information from households who received at least one ITN during the intervention. OLS regressions with standard errors (in parenthesis) robust to intra-village correlation. The dependent variable is the household-specific ratio between treated bednets and total ITNs delivered during the intervention. Asterisks denote significance at the 10 (*), 5 (**) and 1% (***) level. The regressions in columns 2 and 4 include the same baseline household characteristics used to predict ITN purchase in Table 3 (full results are available upon request from the authors). Missing values in some of these characteristics explain the decline in sample size relative to the results in columns 1 and 3. We do not report the intercept in these regression because the inclusion of other covariates makes it non-comparable with the other models.

Table 6: Impact of Intervention on Health Indices

	(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				
Bednets	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) Number of nets from other sources in the 12 months before the follow-up survey				
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 winter 2008-09. Only panel households are included ($n = 1,768$). The figures in panels A and B show proportions of respondents who list, unprompted, the cause/behavior indicated in the row header. The p-values in columns 4 are calculated for a test of the joint null hypothesis that means are identical across experimental arms. All tests are robust to the presence of intra-village correlation of residuals

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.

A Appendix - Not for Publication

A.1 Comparison of Sample Villages and Overall Study Districts

The villages included in our sample were selected from a list of 878 villages where BISWA operated in 2007. In Table A.10, we evaluate the characteristics of communities in our sample relative to other communities in the five study districts, by using data from the 2001 Census of India on a broad range of village-level characteristics. Overall, the five study districts include a population of 8,991 villages. Although the data used in this paper have been collected in 2007-09, the time gap relative to the 2001 census is short enough that a comparison between sample and non-sample villages should be informative.

The results show that the null hypothesis of equality of means between sample and non-sample villages is strongly rejected for most of village characteristics (column 6). 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.

We also test the null hypothesis that village characteristics are on average equal in the three experimental arms (column 7). This is useful, because the randomization tests in Table 1 only evaluated balance in household-level characteristics among villages included at baseline. We find 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, at the 10% level, for the presence of a middle school in the village.

A.2 Attrition and Changes in Household Composition

We look first look at attrition at the household level, which was equal to 5% in MF and control villages and 3% in Free communities (see Table A.11, column 2). The null of equal attrition rates among arms is not rejected at standard levels, regardless of whether we use individual or joint tests. There was little correlation between attrition and household characteristics at baseline, including RDT results and bednet ownership and usage (columns 3 and 4). The only regression coefficients that are individually statistically significant indicate that households with 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).

We also investigated 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, which is potentially important for two reasons. First, changes in availability of ITNs may also arise from changes in the number and age of households members (for instance, young children often share a sleeping space with their parents). Second, malaria and anemia prevalence at baseline differed across age and gender groups (see Figure 3), 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 looked at both entry into or exit from panel households and to changes in the relative weight of different demographic groups. This analysis is possible because 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.12 shows 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.13 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.12 and A.13 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 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 in the regressions) could not be tested because of absence during the visits, probably because they 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 U5s 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 genders cannot be rejected at standard significance levels.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Means, by village category				no. of	Tests (p-values)	
	Not in sample	Control, $n = 47$	Free, $n = 47$	MF, $n = 47$	Villages	H_0: All equal	H_0: Exper. arms equal
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
Scheduled Caste population (%)	0.134	0.164	0.164	0.173	8630	0.012**	0.921
Scheduled Tribe 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	0.746	0.936	0.979	0.936	8991	0.000***	0.432
Middle school	0.236	0.383	0.596	0.447	8991	0.000***	0.096*
Secondary school	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 Centres	0.025	0.106	0.064	0.064	8991	0.132	0.712
Number of Primary Health Sub Centres	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 Banks	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 Kilometers)	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 (not irrigated) cultivated Area (%)	0.422	0.504	0.483	0.510	8875	0.005**	0.864

Notes: Data from the 2001 Government of India Census. 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: 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 expenditure/household size)			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]
% Members who slept under net last night			-0.009	0.002
			[0.016]	[0.017]
% Members who 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 A.12: Changes in Household Membership

	Control	**Free**	**MF**	**Total**
Member at both baseline and follow-up	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.13: 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 differences in the changes, relative to control areas, 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 at the bottom of the table are robust to the presence of cross-equation correlation of residuals.

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 individuals with malaria symptoms in 3 villages in Rourkela district (Orissa). The figures in the sub-table on top are sample correlations between the results as read by the tester indicated in the column header and the one indicated in the row. The figures in the three sub-tables below indicate the details of the sample joint distributions of the test results as read by each tester vs. microscopy. Testers 1 and 2 were part of the field team that conducted blood tests during the follow-up household survey. Tester 3 was the most senior survey monitor in the team.