(Limited) Diffusion of Health-protecting Behaviors:

Evidence from Non-beneficiaries of a Public Health Program in Orissa (India)

Chutima Tontarawongsa
Aprajit Mahajan
Alessandro Tarozzi*

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Abstract

We describe evidence of limited diffusion of bednet acquisition and usage from beneficiaries of an ITN distribution program in rural Orissa, India, to households that did not receive bednets during the intervention. Identification of such network effects relies on the change in ITN adoption among the beneficiaries of a program of bednet distribution that was carried out in a randomly selected subset of 141 study villages. On average, spillovers were limited. However, we find that bednet usage (but not acquisition) was substantively and significantly associated with some (but not all) measures of social links between non-beneficiaries and beneficiaries.
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*We are grateful to participants of the Duke Population and Development Workshop for constructive comments and suggestions. All errors are our own. Chutima Tontarawongsa: Department of Economics, Duke University, ct94@duke.edu; Alessandro Tarozzi: Department of Economics, Duke University, Social Sciences Building, PO Box 90097, Durham, NC 27708, taroz@econ.duke.edu; Aprajit Mahajan: Department of Economics, Stanford University, 579 Serra Mall, Stanford, CA 94305, amahajan@stanford.edu.

1 Introduction

Transmittable diseases such as malaria, yellow fever or intestinal worms, remain a heavy burden for public health in developing countries. In many cases, technological advances have created efficacious preventative measures. For instance, de-worming drugs are very effective at eliminating intestinal infections (Miguel and Kremer 2004) and insecticide treated nets can reduce considerably the burden of malaria (Lengeler 2004). The cost of such preventive technologies are very low for rich countries standards, but can be prohibitively expensive in low-income countries where neither individuals nor public health programs may have sufficient funding. A growing literature therefore studies the reasons of and possible solutions to the low uptake of health-protecting technologies in poor countries, see Holla and Kremer (2009) and Dupas (2012) for recent reviews.

Given the low rates of adoption typically observed among the poor, the lack of experience with such potentially useful technology is often a factor that—together with budget constraints—further reduces demand. Several researchers have thus examined if social networks can facilitate the diffusion of health-protecting products. More generally, public health interventions that introduce such products on a large scale can generate important externalities through changes in disease environment, see Hawley et al. (2003) for the case of insecticide treated nets and malaria, or Miguel and Kremer (2004) for deworming drugs.

However, identifying network effects is hard. The main econometric problem lies in the endogeneity of social networks, as well-documented by a rich literature (see Manski 1993, Brock and Durlauf 2001, Bramoullé et al. 2009 among others). To address this empirical problem, economists have used different strategies depending on the nature of the data. Some of the previous studies have used non-experimental data to tease out the network effects on the adoption of new agricultural technologies by making various identifying assumptions (Besley and Case 1994, Foster and Rosenzweig 1995, Munshi and Myaux 2006). Another key element in the estimation of network effects involves defining *which* social group constitutes the network. Some works use geographical or cultural proximity (Bertrand et al. 2000, Angelucci and Giorgi 2009, Dupas 2010). Others have argued that self-reported networks of friends and family are a better representation of social links (e.g. Bandiera and Rasul 2006, Conley and Udry 2010).

In this paper, we study the links between social networks and adoption of health-protecting technologies using data from a randomized controlled trial conducted in highly malarious areas of rural Orissa, India, between 2007 and 2009. The project was carried out in collaboration with the micro-lender Bharat Integrated Social Welfare Agency (BISWA) in 141 villages where BISWA lending activity was operational. The main purpose of the field experiment was to evaluate the impact, relative to control conditions, on the take-up, usage and health effects of insecticide-treated bednets (ITNs) distributed either through micro-consumer loans or free of cost. Numerous studies have shown that high coverage and use rates of ITNs can significantly reduce malaria-related morbidity and mortality, see Lengeler (2004) for an extensive review of the evidence. Considerable evidence in particular shows that ITNs can be significantly more effective than bednets not treated with insecticide, because only the former nets can lead to 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).

The results of the ITN distribution program on BISWA households' outcomes are described in detail in Tarozzi et al. (2011). The authors show that despite a substantial increase in ITN ownership and (self-reported) usage, especially in areas with free distribution, malaria indices did not improve during the study period. A key feature of the study was that the beneficiaries of the distribution programs were only 'BISWA households', that is, households where at least one individual was already affiliated to BISWA. On average, about 20% of the population in the 141 study villages had such affiliation, so that the majority of the local population was not directly affected by the program. Tarozzi et al. (2011) conjecture that the low coverage of the ITN distribution program was a leading cause for the lack of observed health benefits. If externalities that derive from high usage rates are key factors for the effectiveness of a health product, then public health programs that do not lead to such high coverage rates may lead to a waste of resources. Such concerns may be mitigated if within-community usage diffuses from beneficiaries to non-beneficiaries of distribution programs through network effects.

In this paper we analyze how the increased rates of ITN ownership and usage observed among BISWA households affected the same outcomes among non-beneficiaries, using data collected during the post-intervention survey, carried out in the winter of 2008-09. A key feature of our data is the availability of information on the number and type of social links between non-beneficiaries and a sample of BISWA households directly affected by the program. We then examine three specific questions. First, we estimate simple differences in outcomes between non-beneficiaries in control areas versus others residing in program areas where ITN ownership rates of BISWA households were exogenously increased by the program. Second, we examine if such differences where affected by the number and type of social ties between non-beneficiary and BISWA households. Although such ties are clearly endogenous, finding that the *interaction* between the ties and an (exogenous) program dummy matters for non-beneficiaries' outcomes would signal that spillover effects are present and likely mediated by the social links. Third, we estimate the effect of BISWA peers' behavior on the behavior of non-beneficiaries with instrumental variables, using program dummies as plausibly exogenous instruments for the (endogenous) peers' behavior.

Our paper contributes to a growing literature that uses experimental variation to estimate peer effects in the adoption of health-protecting technologies in developing countries. In a seminal paper, Kremer and Miguel (2007) showed that take-up of deworming drugs among schoolchildren in Kenya was *lower* among children with a larger fraction of peers exposed to a public health program of free treatment. They rationalize the result on the basis of the small private benefit and large positive externalities of the drug. In contrast, Dupas (2010) finds that experimental variation in the fraction of neighbors who received free or highly subsidized ITNs (a product with potentially high private returns) increases the likelihood of purchase. Kremer et al. (2011) find that random variation in the fraction of peers exposed to a point-of-use chlorine treatment for drinking water in Kenya had little impact on take-up. However, using an approach based on Graham (2008), they also find strong peer effects in the adoption of a point-of-collection water purification method based on a chlorine dispenser system. They explain the different results based on the public (point-of-collection) versus private (point-of-use) nature of the action required to adopt the two technologies.

The remainder of the paper proceeds as follows. Section 2 describes the experimental setup and descriptive statistics. We discuss the results on the spillover effects of the free ITN distribution on non-beneficiaries in Sections 3.1 and 3.2, while in Section 3.3 we estimate peer effects of ITN adoption and usage using instrumental variables. Section 4 concludes.

2 Data and Study Design

The results described in this paper are part of a broader evaluation of the cost effectiveness and health impacts of alternative mechanisms to deliver ITNs in poor areas of rural Orissa, India. Official figures pinpoint Orissa as the most highly malaria endemic state in the country (Kumar et al. 2007). The key element of the broader project was a large-scale cluster randomized controlled trial (RCT) designed to evaluate the uptake and impacts of insecticide-treated bednets (ITNs) through micro-consumer loans, as compared to free distribution and control conditions. The study was conducted in rural Orissa in 2007-09, in collaboration with Bharat Integrated Social Welfare Agency (BISWA), a micro-lender with an important presence in the study areas.

A baseline survey was completed in May-June 2007 with a sample of 1,844 households from 141 villages with BISWA presence. In all sampled households, at least one member was affiliated to BISWA, having joined a BISWA 'self-help group'. These are self-formed groups that can apply for micro-loans for which each member becomes jointly liable. We will refer to this sample of 1,844 households affiliated to BISWA as 'baseline' or 'BISWA' households.

After the baseline, villages were randomly assigned to one of three different experimental arms.

In the fall of 2007, the project team carried out in all villages an information session about malaria and proper use of bed nets. In addition, in a first group of 47 villages ("Free" experimental arm), the team distributed free ITNs to all households with members affiliated to BISWA, along with the promise of two free insecticide retreatments at six month intervals. A second group of 47 villages ("MF") received offers to buy ITNs on credit, through micro-loans with a repayment term of one year at a 20% interest rate. The ITN offer price was not negligible, corresponding approximately to three to five times the local daily agricultural wage. Lastly, the control group received no other intervention beyond the information session.

About 96% of the households approached at baseline were then re-interviewed between December 2008 and May 2009, forming a panel of 1,768 households. In earlier work, Tarozzi et al. (2011) show that 52% of sample households purchased ITNs on credit in MF villages, although coverage in these locations remained significantly lower than what achieved with free distribution, where 96% of households received at least one ITN. Unexpectedly, neither micro-loans nor free distribution led to improvements in malaria and anemia prevalence.

A key element of the RCT was the focus on households with BISWA affiliation. Only these households were included in the delivery program, and all surveyed households were selected from lists of BISWA affiliates. In this paper, we focus instead on a supplementary sample added at the time of the post-intervention survey, in 2008-09. This additional sample was added for the purpose of studying impacts on non-beneficiaries.[1] The sampling frame was represented by publicly available census lists drafted as part of the Below the Poverty Line (BPL) census carried out in 2002 by the Ministry of Rural Development, Government of Orissa, with the purpose of identifying 'poor' households eligible to benefit from a number of welfare programs. Although our survey was carried out a few years after the census, preliminary observations in the field showed that the rosters remained overall reliable. In each study village, 10 new households were thus randomly chosen from the census lists, regardless of their BISWA membership. Interviews were then completed with a total of 1,425 new households, of which 1,153 were not affiliated with BISWA. In this paper, we analyze the behavior of these 1,153 non-BISWA households as a function of their indirect exposure to the ITN distribution program. In contrast, we do not use information about households with BISWA affiliation, because these would have likely been affected directly by the interventions.

In principle, the categorization of households as 'BISWA' or 'non-BISWA' could be problematic if affiliation with the micro-lender was endogenously affected by our ITN distribution programs. For instance, suppose that after the ITN distribution programs non-members with higher expected

[1] Non-beneficiaries were only included in the post-intervention survey because the necessary funding was not available beforehand.

benefits from ITNs were more likely to join a BISWA self-help group, in the expectation of future ITN distribution programs. Then we would have likely observed a higher fraction of BISWA members among the new census-drawn households in Free or MF villages relative to control areas. However, the fraction of BISWA members in the population is similar in the three experimental arms (22.5, 20.5 and 20% in Control, Free and MF villages respectively) and the null of equality cannot be rejected at standard levels (p-value= .806). In the rest of the paper, we will thus assume that BISWA membership was exogenous.

2.1 Descriptive Statistics

The post-intervention survey was conducted using a uniform questionnaire for all interviewees, regardless of whether they were part of the panel or their BISWA affiliation. Enumerators recorded demographic, socio-economic, health and other indicators as well as detailed information on sleeping patterns, bednet ownership and use. In a key section of the questionnaire, we collected information about social ties between the respondent's household and each of the BISWA households interviewed in the same village before the intervention ('baseline households'). First, the respondent was asked if anyone in his/her household knew any member from each of the baseline households. Second, if the answer was yes, we recorded the frequency of the social contacts (daily, weekly, monthly, less than monthly). Third, we asked how often the social contacts involved conversations about health-related issues. Fourth, we asked "[w]hen you think about ways of protecting yourself and your family from malaria, do you take into consideration what persons in [panel household] do and what their opinions are?".

We use this information to construct three alternative measures of social links between non-BISWA households in the supplemental sample and baseline households from the same village. The first and most basic measure is the fraction of baseline households known to the respondent ('BISWA Network'). Next, we calculate the fraction of baseline households with whom the respondent's households interacted at least once a week ('Close BISWA Network') and finally the fraction whose opinions about ways of protecting oneself from malaria were taken into account ('Influential BISWA Network'). Previous empirical works on social networks have used a similar concepts, by directly asking about common conversation topics to identify sources of information (Kremer et al. 2011, Conley and Udry 2010).

We chose to use fractions instead of numbers because the number of baseline households are not always the same across villages. Suppose that a respondent reports links to, say, 10 baseline households. In a village where 15 baseline household were interviewed this would indicate links with an estimated 2/3 of BISWA households, while in a village where only 10 were interviewed we would estimate that a link exists with all BISWA household in the village, thereby indicating a stronger potential indirect exposure to the program. Recall that all households in the baseline survey were randomly chosen from a list of BISWA members within the village, whereas the new households were randomized from census list of that village. Therefore, the network measures as described above are unbiased estimators of the true fractions we would have observed if we had information about links to *all* BISWA households in the village.[2]

In Figure 1 we show the histogram of BISWA Network. The average network size of the entire sample was 0.64 so that, on average, non-BISWA households knew about two-thirds of BISWA households from the same village. The majority of households (68%) knew more than half of the BISWA links within their villages, with 260 households (23%) knowing everyone (BISWA Network = 1). Very few households had no or very few links with the baseline sample. In Figure 2 we show the histogram of Close BISWA Network. A comparison with Figure 1 shows that frequent interactions were not the rule, and on average close links only existed with less than half (44%) of baseline households. In total, 691 respondents (60%) reported frequent interactions with only half or less of the baseline BISWA households. Finally, Figure 3 shows the histogram of Influential BISWA Network. This looks overall similar to the histogram from close links, although there is more mass both on the bottom and the top of the distribution.

To further investigate the pattern of social networks in the sample, we explore the association between the network measures and a number of household characteristics. We estimate simple OLS regressions using the measures of social ties with BISWA households as dependent variable. All estimates also include village fixed effects, because the network variable may partly reflect the fact that some villages are smaller in size and therefore have a closer-knit community. The results, in Table 1, show that the network measures are overall very weakly correlated with almost all of the socio-economic and demographic indicators included as regressors. The clear exception is the scheduled caste/scheduled tribe (SCST) dummy, whose coefficient is systematically significant at the 5% or lower level, and relatively large in magnitude. Households that belong to SCST are on average linked to 5-6 percentage points more BISWA households than non-SCST ones. This is perhaps not surprising, given that a large fraction of BISWA affiliates belong to SCST social groups.

[2] Note, however, that a corollary of this way of estimating links to BISWA households is that each BISWA Network variable is measured with error, and the error will be correlated with the fraction of households affiliated to BISWA in each village. For instance, in a village with only 10 BISWA households, all of them would have been interviewed, and BISWA Network would be estimated with no error. But in a village with 50 BISWA households, only 15 of them would have been included in the baseline sample, thereby increasing the measurement error of the BISWA Network variable. We have ignored these considerations so far, although they likely deserve further scrutiny.

Tarozzi et al. (2011) showed that the characteristics of BISWA households were overall balanced across arms. Here we look at cross-arm balance in selected summary statistics for the supplementary sample of non-BISWA households see. Because we do not have baseline data for these households, all statistics are derived from the post-intervention survey. We focus on household characteristics unlikely to have changed with the interventions. The results in Table 2 show that sample households are on average large and poor. Only 40% of sample households have access to electricity, and the average monthly expenditure per head is 692 Rupees, about 50 USD using the most recent parity purchasing power exchange rate (World Bank (2008)). When we carry out tests of equality of means across experimental arms, the null is never rejected at standard levels. This is also true for each of the three Network variables, although links to BISWA households appear to be slightly more infrequent in Free and above all MF communities. In panel B, we also look at village-level characteristics, using data from the 2001 Census of India. Villages in Free groups are larger that in the other two arms in terms of area and populations, but the differences are not statistically significant.

As documented in Tarozzi et al. (2011), bednets were already available in the study areas prior to the intervention, but treatment with insecticide was rare. About two-thirds of baseline BISWA households owned at least one bed net, but less than 10% owned at least one treated net. Almost all of these bed nets were purchased from the market, at a median price of 70 Rupees, about 1.5 times the typical daily wage for agricultural labor in the area. Free distribution of bednets from Government or NGO-driven public health program was very rare, outside of our intervention. This was consistent with our sampling frame which, to avoid contamination, excluded areas where such distribution programs had been or were expected to be conducted in the foreseeable future. Tarozzi et al. (2011) show that no such contamination appeared to have taken place during the duration of the evaluation.

2.2 Outcomes

Throughout the paper we focus on different indicators of bednet adoption, using data on ownership, purchases and usage. We look separately at ITNs and 'any net', where the latter includes all bednets regardless of treatment status. Information on bednets purchases and ownership was collected as follows. First, the respondent was asked to list all 'sleeping spaces' (indoors or outdoors) used by members during the previous night. We then recorded who slept in each space, and whether the space was protected by a bednet. If the answer to the latter question was yes, we recorded when the bednet was acquired, from which source and at which price, and whether and when the bednet had been treated with insecticide. We count a bednet as an ITN if it had been treated with insecticide up to six months before the interview. Standard bednets need to be periodically re-treated with chemicals in order to retain their insecticidal power. The frequency of the re-treatment depends on the type and concentration of the chemical used, but we choose six month because such was the appropriate time interval given the specifics of the insecticide used with the project nets (see Tarozzi et al. 2011 for details).[3]

The focus on last-night usage reduces the possibility of recall error, although it is admittedly a noisy measure of regular usage.[4] Next, we asked if the household owned any other bednet besides those used the previous night, and if so we collected the same information detailed above about each additional bednet.[5] We categorize a bednet (or ITN) as 'recently acquired' (from any source) if it had been with the household for less than 18 months. Such nets were thus likely acquired after our interventions.

3 Results

We organize the results in three parts. First, we examine simple differences in mean outcomes between experimental arms. Second, we estimate the association between outcomes and the alternative measures of links to BISWA households across arms, indicating the likely presence of spillovers from BISWA to non-beneficiary households. Finally, we re-visit the standard problem of peer effects using instrumental variable estimation, making use of the exogenous variation in peers' outcomes that derive from the experimental setting.

3.1 Differences in Means

We first look at the simple cross-arm differences in outcomes. Recall that we are examining the behavior of non-BISWA households, none of whom was targeted by our distribution programs.

[3]Unlike the standard bednets used in our project, Long-lasting insecticidal nets (LLIN) do not require period re-treatment with chemical. Such nets are becoming more common in public health programs, and their use is recommended by the World Health Organization. However, they remain rarely available in local markets in developing countries.

[4]Information on bednet usage was also independently collected in the household roster. For each member, we recorded whether the member had slept under a bednet the night before the interview, and whether the net had been treated in the previous six months. Data on usage from the two alternative sources are very highly correlated and so the results are substantively the same using either source. The similarity of the results is also reassuring because it reduces the likely extent of reporting error.

[5]Survey enumerators also asked permission to see the bednets. This allowed them to verify the presence of the bednets, their state of maintenance, and whether the net was from our distribution program (BISWA nets were clearly labeled and easily identified).

Recall also that observed characteristics unlikely to be influenced by the interventions appeared to be balanced across the different arms, and that we maintain the assumption that BISWA affiliation was not affected by our intervention. Under these conditions, in the absence of any kind of spillovers to non-beneficiaries we should observe similar bednet ownership and usage rates in Free and MF villages relative to Control areas. On the other hand, there are at least four channels through which the interventions could have impacted bednet ownership and usage among non-beneficiary households. First, non-BISWA households may have been exposed directly to the short information session on malaria and bednet that took place in the fall of 2007. Second, behavior may have been affected later on through imitation or learning, mechanisms that we will explore in more details in Section 3.2. Third, some ITNs may have been transferred from BISWA households to non-beneficiaries as gifts or through sales. Fourth, the frequency of bednet usage may have affected by community-wide changes in the local mosquito population and malaria prevalence caused by our programs of ITN distribution, especially in areas where a large number of ITNs were delivered for free to all BISWA affiliates.

The results, in Table 3, show that for all but two outcomes the null of equal means cannot be rejected at standard levels. About one every three households acquired at least one bednet during the previous 18 months, while only one in ten acquired nets that had been recently treated with insecticide. The fraction of individuals who slept under a bednets or an ITN was slightly higher in treatment versus control areas, but the null of equality is never rejected at standard levels. In particular, less than 10% of individuals slept under a treated net (3.9% in control areas, 4.3% in Free and 4.7% in MF villages). The null of equality is rejected at the 10% (but not at the 5%) level only for the outcomes that measure recently acquired ITNs. While 9% of non-BISWA households acquired any ITNs in the last year and a half in control areas and 7% did in Free areas, the fraction was much higher at 14.2% in MF villages.[6] Transfers of BISWA nets from baseline households to non-BISWA households via reselling or donations are unlikely to be the cause of such differences, because most of the recently acquired ITNs were reported to be purchased from the market. In fact, only seven households owned BISWA nets.

In the last two rows of Table 3 we also examine difference in malaria prevalence and hemoglobin levels (another key health indicator often associated to malaria). Both indices were measured through blood tests conducted with rapid diagnostic tests that delivered results within minutes, directly in the field (see Tarozzi et al. 2011 for details). All individuals of age below 10 or between 18 and 40 were targeted for testing.[7] Overall, 2,345 individuals of age from 876 households were

[6]The difference remains large and significant even after dropping two MF villages where BISWA households in the sample purchased an unusually large number of ITNs for resale.

[7]Thirty-two percent of individuals were not tested, with the proportion seven percentage points higher in Free

tested for malaria, while hemoglobin was measured for 2,362 individuals from 881 households. The results show that both health indicators were very similar across treatment groups, and the null of equality is not rejected at standard levels. Malaria prevalence was very high, with about 20% of individuals testing positive. Hemoglobin level was 11.5 grams per deciliter (g/dl) of blood on average. This is low, given that 11 g/dl is sometimes taken as a threshold below which individuals are consider anemic (Thomas et al. 2006).

The remarkable similarity of malaria indices across arms is a strong indicator that the local epidemiological environment was not affected by the interventions. This is consistent with earlier studies that suggest that community protective effects of ITNs only emerge when 60% or more of sleeping spaces are covered. Tarozzi et al. (2011) show that such high coverage rates were never achieved by our interventions, which only targeted BISWA households. Indeed, the authors suggest that this may have been a key factor in explaining the lack of impacts of the ITN distribution programs on health indices observed even among the beneficiary households.

3.2 Heterogeneous Impacts as a Function of Links to Beneficiaries

We have shown that mean outcomes in Control villages appear to be very close to those observed in Free villages. In this section, we explore whether such aggregate results actually mask the existence of spillovers for non-beneficiary households with tighter links to BISWA members. Among BISWA members, Tarozzi et al. (2011) show that increases in ITN ownership and usage in Free and MF versus Control communities were substantively and statistically significant. At the time of the post-intervention survey (when data on the supplemental sample were collected as well), BISWA households owned on average 1.9 bednets in Control areas, 2.5 in MF villages and 3.4 with Free distribution. The random assignment of villages into experimental arms generated thus exogenous variation among non-BISWA households in their exposure to information about bed nets and insecticide treatment, through their links with BISWA households.

For a given outcome Y_{vi} for household i from village v, we estimate models such as the following:

$$Y_{vi} = \alpha \text{Network}_{vi} + \beta \text{Network}_{vi} \times \text{Free}_v + \gamma' X_{vi} + F_v + \epsilon_{vi}, \tag{1}$$

where Network_{vi} is one of the measures of links to a 'BISWA Network' described in Section 2.2, $Free_v$ denotes the treatment status, F_v is a village fixed effect, and X_i is a vector of household characteristics unlikely to have changed as a consequence of the intervention. Such control variables

communities, and two percentage points higher in MF villages. The joint null of equal testing rate is not rejected at standard levels (p-value= 0.1113), although the null of equality between Free and Control areas is rejected (p-value= 0.042).

include household size, number of children under 5 years old, fraction of males in the household, number of household members who completed some schooling, number of rooms in the dwelling, access to electricity, and whether the household belongs to scheduled tribe/caste. We estimate all models using Ordinary Least Squares.[8] We calculate standard errors allowing for intra-village correlation of residuals.

Note that in estimating model (3.2) we only include observations from Control and Free villages. In the latter communities, virtually all sample BISWA households received ITNs during our intervention. The number of nets received was a function of the demographic composition of the households (with a ceiling of four ITNs per household) and was therefore exogenously determined by our field team. In contrast, demand for ITNs in MF villages was endogenously determined by BISWA members. In these latter communities, non-BISWA households with stronger links to program beneficiaries were then not necessarily 'exposed' to more nets, and this would complicate the interpretation of the results.

In the absence of any spillover to non-beneficiary households, we would expect $\hat{\beta}$ to be close to zero. This coefficient can be interpreted as the differential impact of the free ITN distribution on outcomes of non-BISWA households, as a function of their links to the beneficiaries of the program. A key limitation of this approach is that social links with BISWA households are clearly not exogenous. Individuals with stronger or more numerous ties are likely to be systematically different from others with smaller networks. Indeed, in Table 1 we have shown that some household characteristics (in particular SCST status) predict the size of the 'BISWA Network'. In addition, being part of a large network may be correlated with unobserved characteristics such as sociability and open-mindedness which, in turn, are likely to influence the propensity to adopt a new technology. In principle, for instance, large estimates of $\hat{\beta}$ could be due not to peer effects, but to more connected households having been more likely to be present during the bednet treatment with insecticide that often took place in public areas. In such case, any impact on bednet usage or treatment rates would have been due to direct exposure to the implementation of the program rather than to any social dynamic.

While keeping these caveats in mind, we first estimate a specification where social links are measured by 'BISWA Network', defined as the fraction of the baseline BISWA households known to the non-beneficiary household i (see Section 2.1 for details). We look at four different outcomes: a dummy for households that acquired at least one ITN from any source in the 18 months before the interview; a similar dummy defined for nets regardless of treatment status; the fraction of

[8]The linear probability models yield similar results compared to probit and logit models. These are available upon request.

household members that slept under an ITN the night before the interview, and the fraction who slept protected by a bednet regardless of treatment. The results are in Table 4. All estimates of α, which measures the association between the network variable and the outcome in control areas, are small and not significant at standard levels.

The estimate of β for recently acquired ITNs is close to zero and not significant, while the estimate for all acquired bednets is relative large (0.183) but again not significant at standard levels. In contrast, usage of both ITNs and any bednets appears to be more common for households with more social ties with BISWA affiliates. The estimates implies that, in non-beneficiary households in areas with Free distribution of ITNs, knowing all BISWA members in the baseline sample as opposed to none increases the fraction of individuals having slept under ITNs by 7 percentage points, and the fraction who slept protected by a net regardless of treatment by 16 percentage points. Both coefficients are significant, although only at the 10% level. These estimates are substantively large, given that the overall fraction of individuals who used bednets (treated or not) was well below 10% (see table 3).

Next, we explore whether the type of the social interactions plays a role in how networks affected bednet-related behavior. Because closer peers may be more influential in household decisions, the impacts of the intervention channeled through close peers may be stronger. We do not find much support for this hypothesis. When we use the two alternative measures 'Close BISWA Network' and 'Influential BISWA Network' described in Section 2.1, the results show weak evidence of network effects, see Table 5. The only outcome where β is significant (and only at the 10% level) is the dummy for recently acquired bednets. A household acquainted with all baseline BISWA households, and who is influenced by their viewpoints on malaria matters, has a 21 percentage point higher probability of having recently acquired at least one net relative to another household with no such social ties. More generally, the estimated impacts on net acquisition and usage are small and not significant for ITNs, while they are substantively large (but not significant, with the exception indicated above) for all nets regardless of treatment status.

In sum, we find only weak evidence supporting the view that the large increase in ITN ownership and usage observed among BISWA households who received nets free of cost was transmitted to non-beneficiaries through social ties. One simple explanation for this finding is that, given the absence of health benefits even among beneficiaries, and even in the presence of strong social ties between the two groups of households, non-beneficiaries simply did not have sufficient incentives to adopt a technology that, after all, was not protecting health effectively among their peers.

3.3 Instrumental Variable Estimation of Peer Effects

The previous section described models that aimed at identifying the spillover effects of the free ITN distribution on the behavior of non-beneficiaries, spillovers that may have taken place through social networks. An alternative identification strategy also allows to identify directly the impact of BISWA social contacts' behavior on the choices of non-beneficiaries. Formally, we are interested in estimating the slope β_1 in the following simple model:

$$Y_{vi} = \beta_0 + \beta_1 X_{g,vi} + \epsilon_{vi}, \tag{2}$$

where, like before, Y_{vi} is a given outcome for household i in village v, and $X_{g,vi}$ is a measure of malaria-related behavior among i's social links, where social links are defined in one of the alternative ways described in Section 2.1. Given the likely endogenous sorting of individuals into social groups, OLS estimates would likely lead to positive and significant estimates for β_1, but such estimates would not be consistent for the true causal impact of $X_{g,vi}$ on the behavior of non-beneficiaries. However, the exogenous variation in the behavior of social links due to the randomized interventions provides a useful framework to estimate peer effects. Specifically, we can use the randomly assignment treatment status as an instrument for the endogenous behavior of peers.

We then use data from all three experimental arms and use the two treatment dummies MF_v and $Free_v$ as instruments for the endogenous variable $X_{g,vi}$. The two dummies are defined as $MF_v = 1$ for individuals in villages where ITNs were offered for sale on credit, and $Free_v = 1$ in villages where ITNs were delivered free of cost. Given that $X_{g,vi}$ will be an index of bednet ownership or usage, and given that such outcomes were significantly affected by the interventions, the instruments should be very strongly correlated with the endogenous variable. In contrast, instrument exogeneity is more demanding, because it requires that the only link between i's behavior (measured by Y_{vi}) and the treatment dummies passes through the behavior of the social links. A first reason why such assumption could fail is if the large increase in the fraction of village population protected by ITNs led to a reduction in the village-wide malaria prevalence. However, the results in Table 3 show barely any difference in malaria indices by experimental arms. The assumption could also fail if peers effects also work (as they are likely to) through indirect links. In other words, $X_{g,vi}$ measures only behavior among BISWA households included in the village-specific sample at baseline, but non-beneficiaries may have also been affected by the behavior of BISWA households not included in the sample, or by the choices made by others with social ties to BISWA households.[9] Although

[9] Network effects due to social ties with beneficiaries from other villages are instead very unlikely, because the study villages were spread over five different districts, so that most villages were geographically far apart.

these caveats must be kept in mind, the availability of two instruments at least allows us to test their exogeneity by carrying out standard tests of overidentification.

The estimates are shown in Table 6. As expected, all OLS estimates are positive, although they are only large and significant when $X_{g,vi}$ is the average number of all bednets per person owned by the BISWA social links. However, when we estimate equation (3.3) using two-stage least squares $\hat{\beta}_1$ is always become smaller (in some cases even changing sign) and it is never significant at standard levels. As expected, there is no weak instrument problem, and the first stage F test is larger than 30 in both models. The overidentification tests also provide overall support to the hypothesis of instrument exogeneity, although the null is rejected at the 10% level when the outcome is a dummy for recently acquired ITNs. Overall, the results suggest that peer effects were not important elements for the diffusion of bednet usage in the sample.

4 Discussion and Conclusions

In this paper we have described evidence of some limited diffusion of bednet acquisition and usage from beneficiaries of an ITN distribution program in rural Orissa, India, to households that did not receive bednets during the intervention. Identification of such network effects hinged on the change in ITN adoption among the beneficiaries of a program of bednet distribution that was carried out in a randomly selected subset of the 141 study villages.

On the one hand, we have shown that, on average, there were very limited spillovers. On the other hand, we find that bednet usage was substantively and significantly associated with some (but not all) measures of social links between non-beneficiaries and beneficiaries. This provides some evidence of network effects in the adoption of a health product that has potentially high protective power against malaria risk. However, given the endogeneity of social links, we cannot exclude that such associations were at least in part due to indirect effects of the programs mediated by channels different from, but correlated with, the number of social links between beneficiaries and non-beneficiaries.

In interpreting the results, it is useful to recall that the study area was very broad, covering 141 villages in five different districts. At the same time, the ITN distribution program was only conducted in communities where the micro-lender BISWA was operating at the time of the baseline survey. The external validity of our results should therefore be evaluated with caution. With this caveat, our results should be a useful contribution to a growing literature that evaluates the diffusion of health-protecting products through social networks in developing countries. Gauging the extent of such diffusion is particularly important in settings where public health programs only cover a fraction of the population at risk, and when coverage rates are a key element for the effectiveness of the program. This can be crucial, given the important role of externalities in fighting several transmittable diseases such as malaria or other insect-borne diseases, or intestinal worms.

In our context, the limited diffusion in the adoption of ITNs in a highly malarious area of rural Orissa may have been due to the overall absence of health benefits among the primary beneficiaries of the ITN distribution program. This may have been due to the low fraction of beneficiaries in the population, coupled with perhaps irregular usage of ITNs. These factors may have limited the effectiveness of ITNs, which has been otherwise convincingly documented in controlled conditions in the field. In contrast, a different RCT carried out in Kenya, Dupas (2010) found that the demand for bednets increased when a randomly determined higher fraction of social links had adopted the product. Although the different study area likely justifies results different from ours, Dupas (2010) describes how a large fraction of bednet users perceived a reduction in malaria risk, as well as little discomfort in using the nets supplied through the project. The absence of clear health benefits in our context is a likely key reason for the limited diffusion of ITN usage in our study area. If so, free or heavily subsidized ITNs extended to the whole population may have been the only way to reduce the malaria burden through this potentially important health product.

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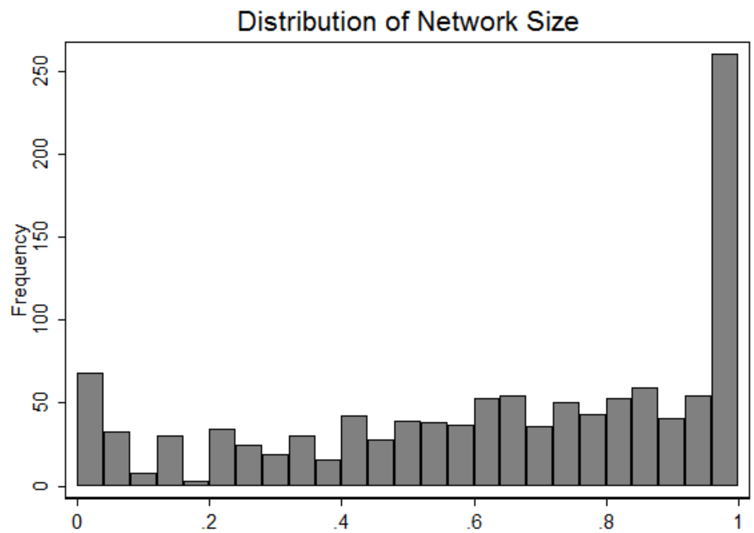

Figure 1: Distribution of BISWA Network
Data from winter 2008-09 from a supplemental sample of 1,153 households not affiliated to BISWA. BISWA Network is calculated as the fraction of baseline households from the same village known to the respondent's household. The overall sample mean is 0.64.

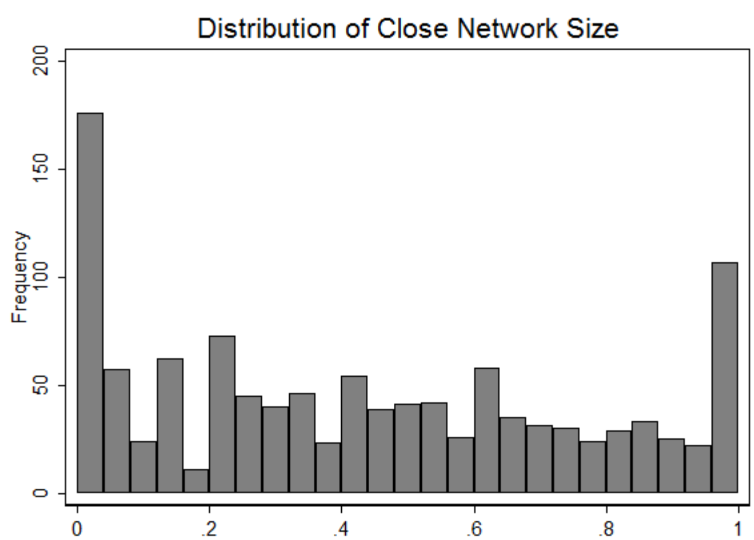

Figure 2: Distribution of Close BISWA Network
Data from winter 2008-09 from a supplemental sample of 1,153 households not affiliated to BISWA. Close BISWA Network is defined as the fraction of baseline households from the same village with whom the respondent's households interacts at least once a week. The overall sample mean is 0.44.

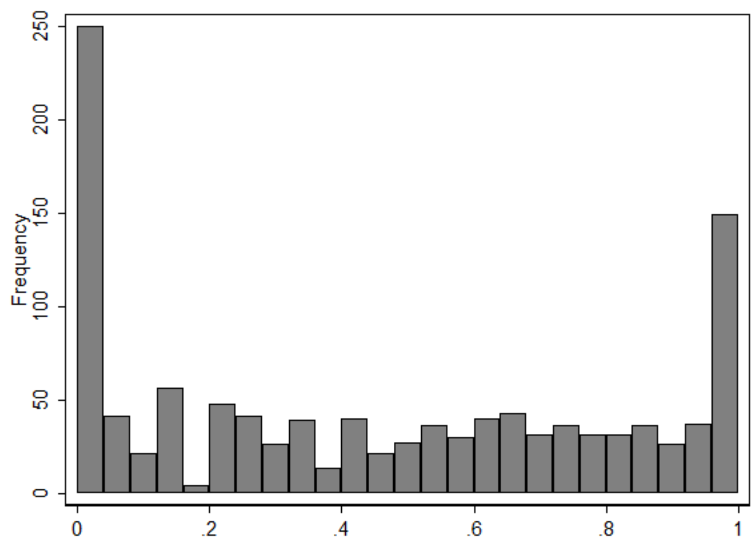

Figure 3: Distribution of BISWA Network
Data from winter 2008-09 from a supplemental sample of 1,153 households not affiliated to BISWA. Influential BISWA Network is defined as the fraction of baseline households from the same village whose opinions about ways of protecting oneself from malaria are taken into account by the respondent's household. The overall sample mean is 0.46.

Table 1: Determinants of BISWA Network Size

	(1) BISWA Network	(2) Close BISWA Network	(3) Influential BISWA Network
Log per capita expenditure	0.005	0.013	0.015
	(0.015)	(0.017)	(0.015)
Household size	0.003	-0.005	0.016*
	(0.007)	(0.007)	(0.009)
Fraction of male	-0.031	-0.035	0.077
	(0.047)	(0.058)	(0.052)
#Children under 5	-0.022	0.006	-0.025
	(0.013)	(0.014)	(0.016)
#Members completed some schooling	0.007	0.010	-0.006
	(0.008)	(0.008)	(0.008)
#Rooms in the dwelling	0.010**	0.001	0.004
	(0.004)	(0.006)	(0.006)
Access to electricity	0.016	0.004	0.022
	(0.020)	(0.023)	(0.023)
Scheduled tribe/caste	0.048**	0.060***	0.055**
	(0.021)	(0.021)	(0.027)
Constant	0.520***	0.303***	0.207**
	(0.095)	(0.110)	(0.103)
Village Fixed Effects	Yes	Yes	Yes
Observations	1,150	1,150	1,150
R-squared	0.020	0.005	0.014

Data from winter 2008-09 from a supplemental sample of 1,153 households non affiliated to BISWA. Robust standard errors clustered at village level are reported in parentheses. BISWA Network is calculated as the fraction of baseline households from the same village known to the respondent's households. Close BISWA Network is defined as the fraction of baseline households with whom the respondent's households interacts at least once a week. Influential BISWA Network is defined as the fraction of baseline households from the same village whose opinions about ways of protecting oneself from malaria are taken into account by the respondent's household.
*** p< 0.01, ** $p < 0.05$, * $p < 0.1$.

Table 2: Descriptive Statistics by Experimental Arm

	(1) Control		(2) Free		(3) MF		(4) All		p-value*
A: Households characteristics									
BISWA Network[a]	0.667	(0.318)	0.630	(0.335)	0.629	(0.300)	0.642	(0.318)	0.624
Close BISWA Network[b]	0.459	(0.343)	0.442	(0.335)	0.405	(0.305)	0.435	(0.328)	0.442
Influential BISWA Network[c]	0.499	(0.373)	0.467	(0.370)	0.403	(0.340)	0.455	(0.363)	0.107
Household size	5.192	(2.426)	5.342	(2.570)	5.203	(2.252)	5.247	(2.417)	0.709
Log monthly expenditure per capita	6.549	(0.641)	6.520	(0.667)	6.550	(0.695)	6.540	(0.668)	0.869
Fraction of males	0.511	(0.174)	0.485	(0.178)	0.508	(0.170)	0.501	(0.174)	0.915
# Children under 5	0.487	(0.787)	0.537	(0.806)	0.534	(0.798)	0.520	(0.797)	0.664
# Members, completed some schooling	3.468	(2.275)	3.485	(2.326)	3.453	(2.130)	3.469	(2.224)	0.985
# Rooms in the dwelling	2.995	(1.721)	3.169	(2.060)	3.059	(1.900)	3.075	(1.902)	0.647
Access to electricity	0.402	(0.491)	0.417	(0.494)	0.402	(0.491)	0.407	(0.492)	0.953
Scheduled tribe/caste	0.745	(0.745)	0.790	(0.790)	0.725	(0.725)	0.754	(0.431)	0.141
Observations	369		391		393		1153		
B: Village Characteristics									
Number of households	261.4	(327.7)	359	(501.6)	284.3	(0.484)	301.5	(383.8)	0.534
Total population (persons)	1180.6	(1483.2)	1664.3	(2416.5)	1258.4	(1312.9)	1367.8	(1803.7)	0.496
% BISWA members	22.51	(21.97)	20.53	(20.23)	19.98	(16.13)	21.00	(19.48)	0.894
Area of Village (in hectares)	413.1	(343.7)	476.4	(340.9)	417.4	(388.25)	435.64	(356.903)	0.615
Medical Facilities (Available/NA)	0.255	(0.441)	0.298	(0.462)	0.255	(0.441)	0.270	(0.445)	0.872
Forests: % village area	0.125	(0.174)	0.087	(0.125)	0.082	(0.143)	0.097	(0.149)	0.367
Irrigated area: % village area	0.151	(0.244)	0.188	(0.265)	0.183	(0.268)	0.174	(0.258)	0.733
Non-irrigated area: % village area	0.504	(0.242)	0.483	(0.250)	0.510	(0.283)	0.499	(0.257)	0.867
Observations	47		47		47		141		

Panel A: Data from winter 2008-09 from a supplemental sample of 1,153 households non affiliated to BISWA. Panel B: Data on village characteristics from Census of India, 2001. Standard deviations in parentheses. * p-value for the joint test of equality across the three experimental arms (robust to intra-village correlation of residuals. [a] BISWA Network is calculated as the fraction of baseline households from the same village known to the respondent's households. [b] Close BISWA Network is defined as the fraction of baseline households with whom the respondent's households interacts at least once a week. [c] Influential BISWA Network is defined as the fraction of baseline households from the same village whose opinions about ways of protecting oneself from malaria are taken into account by the respondent's household.

Table 3: Outcome Variables, by Experimental Arm

	(1) Control		(2) Free		(3) MF		(4) All		(5) p-value
Recently acquired at least one ITN[1]	0.092	(0.290)	0.072	(0.258)	0.142	(0.350)	0.102	(0.303)	0.089*
Recently acquired at least one bednet[2]	0.371	(0.484)	0.373	(0.484)	0.427	(0.495)	0.391	(0.488)	0.401
# recently acquired ITNs	0.192	(0.666)	0.123	(0.491)	0.298	(0.866)	0.205	(0.696)	0.079*
# recently acquired bednets	0.740	(1.164)	0.770	(1.312)	0.913	(1.314)	0.809	(1.269)	0.298
# recently purchased bednets	0.596	(1.072)	0.619	(1.243)	0.710	(1.222)	0.643	(1.183)	0.431
Total number of ITNs owned	0.352	(1.091)	0.302	(0.937)	0.461	(1.138)	0.372	(1.060)	0.382
Total number of bednets owned	1.621	(1.782)	1.601	(1.858)	1.784	(1.753)	1.670	(1.799)	0.743
% of members used ITN last night	0.039	(0.180)	0.043	(0.187)	0.047	(0.185)	0.043	(0.184)	0.382
% of members used any net last night	0.060	(0.269)	0.070	(0.279)	0.084	(0.334)	0.072	(0.296)	0.418
Malaria prevalence[3]	0.197	(0.398)	0.208	(0.406)	0.206	(0.404)	0.203	(0.403)	0.957
Hemoglobin[4]	11.5	(1.96)	11.4	(1.98)	11.4	(1.93)	11.5	(1.95)	0.628
Observations	369		391		393		1153		

Data from winter 2008-09 from a supplemental sample of 1,153 households non affiliated to BISWA. Standard deviations are reported in parentheses. The p-value in column (5) are for the joint test of equality across the three experimental arms (robust to intra-village correlation of residuals). * Indicates rejection at the 10% significance level. Notes:

[1] = 1 if the household purchased at least one ITN/bednet in the past 18 months.

[2] "Bednets" includes both treated and untreated bed nets.

[3] Malaria prevalence was measured using blood tests from 2,345 individuals of age below 10 or 18-40 from 876 households.

[4] Hemoglobin was measured using blood tests from 2,362 individuals of age below 10 or 18-40 from 881 households.

Table 4: Cross-arm Differences in Outcomes as a Function of Links to BISWA Households

Dependent variables	(1) Recently acquired at least one ITN	(2) Fraction of household members slept under ITN last night	(3) Recently acquired at least one net	(4) Fraction of household members slept under net last night
BISWA Network $\hat{\alpha}$	0.051	-0.043	0.050	-0.044
	(0.052)	(0.039)	(0.091)	(0.042)
BISWA Network×Free $\hat{\beta}$	-0.008	0.071*	0.183	0.157*
	(0.068)	(0.042)	(0.140)	(0.087)
Observations	759	759	759	759
R-squared	0.289	0.287	0.246	0.315

Data from winter 2008-09 from a supplemental sample of households non affiliated to BISWA. Only households in Free and Control village are included. Robust standard errors clustered at village level are reported in parentheses. All specifications include village fixed effects and the following control variables: household size, number of children under 5 years old, fraction of males in the household, number of household members who completed some schooling, number of rooms in the dwelling, access to electricity, and whether the household belonged to a scheduled tribe/caste. In columns 2 and 4, household-level observations on bednet usage rates are weighted by household size. *** $p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table 5: Alternative Characterizations of BISWA Networks

Dependent variable:	Recently acquired at least one ITN		Slept under ITNs last night		Recently acquired at least one net		Slept under nets last night	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Measure of network:	Close BISWA Network	Influential BISWA Network	Close BISWA Network	Influential BISWA Network	Close BISWA Network	Influential BISWA Network	Close BISWA Network	Influential BISWA Network
BISWA Network $\hat{\alpha}$	0.031	0.001	0.001	-0.003	-0.042	0.009	-0.033	-0.019
	(0.042)	(0.039)	(0.028)	(0.031)	(0.084)	(0.078)	(0.059)	(0.044)
BISWA Network × Free $\hat{\beta}$	-0.007	0.054	0.019	-0.009	0.183	0.212*	0.147	0.084
	(0.063)	(0.056)	(0.038)	(0.036)	(0.128)	(0.123)	(0.097)	(0.075)
Observations	759	759	759	759	759	759	759	759
R-squared	0.288	0.289	0.285	0.285	0.242	0.248	0.316	0.312

Data from winter 2008-09 from a supplemental sample of households non affiliated to BISWA. Only households in Free and Control village are included. Robust standard errors clustered at village level are reported in parentheses. All specifications include village fixed effects and the following control variables: household size, number of children under 5 years old, fraction of males in the household, number of household members who completed some schooling, number of rooms in the dwelling, access to electricity, and whether the household belonged to a scheduled tribe/caste. Close BISWA Network is defined as the fraction of baseline households with whom the respondent's households interacts at least once a week. Influential BISWA Network is defined as the fraction of baseline households from the same village whose opinions about ways of protecting oneself from malaria are taken into account by the respondent's household.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6: Peer Effects in Bednet Ownership and Usage

Dependent variable:	Recently purchased at least one ITN		Slept under ITNs last night		Recently purchased at least one net		Slept under nets last night	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
A:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Average per capita bednets	0.151**	-0.050	0.063**	0.020	0.346***	0.042	0.102*	0.056
owned by peers	(0.071)	(0.138)	(0.030)	(0.084)	(0.077)	(0.200)	(0.054)	(0.139)
Constant	0.032	0.126*	0.014	0.034	0.230***	0.372***	0.024	0.045
	(0.031)	(0.069)	(0.014)	(0.039)	(0.041)	(0.098)	(0.027)	(0.066)
First stage F stat		31.55		31.55		31.55		31.55
Hansen J Stat		3.817		0.100		1.848		0.416
p-value		(0.051)		(0.751)		(0.174)		(0.519)
Observations	1153	1153	1153	1153	1153	1153	1153	1153
B:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Average last night ITN usage	0.007	-0.107	0.024	0.007	0.071	-0.036	0.046	0.016
among peers	(0.046)	(0.090)	(0.023)	(0.062)	(0.069)	(0.137)	(0.043)	(0.100)
Constant	0.100***	0.134***	0.036***	0.041**	0.370***	0.402***	0.058***	0.067**
	(0.021)	(0.034)	(0.009)	(0.019)	(0.032)	(0.048)	(0.018)	(0.032)
First stage F stat		40.47		40.47		40.47		40.47
Hansen J Stat		2.809		0.154		1.634		0.558
p-value		(0.094)		(0.694)		(0.201)		(0.455)
Observations	1153	1153	1153	1153	1153	1153	1153	1153

Data from winter 2008-09 from a supplemental sample of households non affiliated to BISWA. Robust standard errors clustered at village level are reported in parentheses. *** p< 0.01, ** $p < 0.05$, * $p < 0.1$.