Mosquito Nets, Malaria Infection, and Schooling in a Developing Country

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Abstract

Malaria is one of the most serious health burdens in developing countries, especially in sub-Saharan Africa. We quantify the extent to which the use of long-lasting insecticide-treated mosquito nets (LLINs) affects school attendance by exploiting a large natural experiment in Madagascar. In 2009, the Malagasy government began the mass distribution of free LLINs to all families. We find that the use of these LLINs significantly reduced school absences among 6–12-year olds by approximately 19 days per year on average. According to the results based on rapid diagnosis tests, LLIN usage has reduced malaria infection rates significantly. Overall, the results show that the positive effects of the usage of mosquito nets on schooling are owing to the reduced rates of malaria infection. LLINs can thus be considered an effective policy instrument for raising the human capital accumulation of elementary school-aged children in areas with high malaria risk. Based on our estimation, we find that setting aside targeting costs, LLIN usage costs USD 11.12–13.49 for an additional year of schooling. Yet, universal coverage costs USD 630.22 for an additional year of schooling owing to the large inefficiencies associated with inclusion errors. We propose a simple targeting strategy based on the age of household members, which reduces the cost for an additional year of schooling to USD 273.57.

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1 Introduction

Improvements in health and education are regarded as critical policy targets in global development goals such as the Millennium Development Goals and Sustainable Development Goals. In the context of health, malaria is one of the most serious health burdens in developing countries, especially in sub-Saharan Africa (Rowe et al., 2006). Among interventions of malaria prevention, long-lasting insecticide-treated nets (LLINs) are considered a particularly powerful tool to prevent malaria infection because mosquitoes are nocturnally active (Lengeler, 2006; Sumitomo-Chemical, 2010; Bhatt et al., 2015; Pryce et al., 2018; Maskin et al., 2019).[1] There has been a set of illuminating existing studies on adoption decisions of LLINs which explore price elasticities, role of liquidity constraints, intrahousehold resource allocation, and present bias (Blackburn et al., 2008; Hoffmann, 2009; Cohen and Dupas, 2010; Tarozzi et al., 2014; Dupas, 2014b; Mahajan et al., 2020). Yet, to the best of our knowledge, no study has investigated the impact of LLINs on broader socio-economic outcomes. In this paper, we aim at filling this gap in the existing literature by evaluating impact of LLINs on school attendance following the recent literature on social impacts of health interventions such as Miguel and Kremer (2004), which finds that deworming treatments stimulate school attendance. Our study is also broadly linked with the literature on disease and development (Acemoglu et al., 2001; Bleakley, 2010; Almond, 2006; Alsan, 2015). While there is an emerging literature that causally link early childhood health interventions to academic performance later in life (Field et al., 2009; Bharadwaj et al., 2013), contemporaneous impacts of health interventions on schooling behavior have been underinvestigated.

Actually, it has been observed that school-aged children are less covered by mosquito nets, leading to higher incidence of malaria among these children (Noor et al., 2009). We confirm this pattern in our baseline data from Madagascar (Figure 1). This highlights the importance of intrahousehold decisions as well as age-specific targeting thorough, for example, schools although the governments and international organizations have promoted free distribution of LLINs to achieve universal coverage. In this respect, this study contributes to the research on anti-poverty targeting programs. A large portion of the literature studies the efficacy of targeting methods in developing countries, such as proxy means tests, self-targeting, geographic targeting, and community targeting in developing countries (Nichols and Zeckhauser, 1982; Coady et al., 2004; Elbers et al., 2007; Ravallion, 2009; Alatas et al., 2012; Brown et al., 2016). Unlike these studies, we will compare the cost of universal coverage with the cost of a perfectly targeted LLIN distribution to school-aged children.

[1]The previous generation of nets were also insecticide-treated; however, they required retreatment and users rarely retreated their nets in practice. This problem was partially solved by implementing a commitment approach, in which people paid for retreatment at the time of purchase (Tarozzi et al., 2014). LLINs overcome this technical shortcoming of the previous generation of nets.

Our analysis will also provide important insights for understanding basic income programs (Ravallion, 2017; Hanna and Olken, 2018; Banerjee et al., 2019).

Our research design exploits a natural experimental situation in Madagascar. In 2009, the Malagasy government introduced the free distribution of LLINs. Because of limited capacity, it distributed nets on different dates for different regions. To exploit this different distribution timing, we conducted our survey in the area around the border of two regions three times: before the first distribution, after the first distribution, and after both regions had received the nets. In these surveys, we collected household-level information about mosquito net usage and school attendance. We also gathered school attendance records from local schools. Furthermore, we examined the malaria infections among all the villagers by using a rapid diagnosis test (RDT).

Obtained by combining these data from households, schools, and health inspections, our main empirical results show that the distribution of LLINs significantly reduced school absences. This schooling effect seems to be caused by the increase in the use of LLINs and the decrease in the number of malaria infections. On average, using LLINs significantly reduced school absences among children aged 6–12 by approximately 19 days per year. On the contrary, we find no influence on school enrollment, adult malaria infection, or household income. An LLIN costs only USD 11.12–13.49 for each additional year of schooling, suggesting that LLINs, which have been recognized as a core preventive tool to tackle malaria infection, are also a cost-effective measure for enhancing school attendance rates. We also find that universal coverage costs USD 630.22 for an additional year of schooling owing to the large inefficiencies associated with substantial inclusion errors. To reduce these, we propose a feasible and simple targeting strategy: excluding households without any members aged 6–12. A net distribution with this strategy is estimated to only cost USD 273.57 for an additional year of schooling.

The rest of the article is organized as follows: Section 2 explains the data and provides descriptive statistics. Section 3 shows the empirical results, followed by concluding remarks in Section 4.

2 Data and Descriptive Statistics

2.1 Data

We conducted our survey in an area where malaria is prevalent, namely the border area between the Atsinanana and Analanjirofo regions along the east coast of Madagascar (Figure 2). There is distinct seasonality in this area: the wet season runs from December to May and the dry season from June to November. In the southern boundary area, Atsinanana, LLINs were distributed in December 2009. In the Analanjirofo region, located north of the boundary, LLINs were provided in June 2010. Based on this six-month gap in the distribution of nets, we label Atsinanana the treated group and Analanjirofo the control group. Figure 2 shows the location of our survey respondents: villages 1–12 are located in the Analanjirofo region and villages 13–18 are in the Atsinanana region. Our research strategy is to exploit this exogenous variation in the timing of the free distributions. Household surveys were conducted exclusively for our study by the Institut National de la Statistique de Madagascar (INSTAT) three times. We thus obtained panel data for a first wave (baseline; dry season), second wave (midline; rainy season), and third wave (endline; dry season) (Table 1).[2]

In the survey, we collected basic information on each household member, such as age, education level, malaria infection history, and mosquito net usage as well as household information such as income and assets. For children, we also asked about the number of days they were absent from school.[3] Although we gathered information on all household members, we focused on primary school-aged children (i.e., 6–12 years old).

To detect malaria infection,[4] we employed a validated RDT (Carestart®, by Access Bio).[5][6] If a person was detected to be malaria positive, we provided an ACT drug that treats malaria (Coarsucam ®). Thus, if a person was found to be infected by malaria in the second (third) survey, we can assume that he or she was infected for the first time between the first and second (second and third) surveys.

2.2 Baseline Balance and Attrition

We performed baseline balancing tests by comparing the treatment group (South) to the control group (North). Panel A in Table 2 shows the comparison of children aged 6–12 in these two regions using our survey data. In the control region, the malaria infection rate is higher and the mosquito net usage rate is lower. In particular, all the high malaria risk villages are in the control group. Based on a regression of the

[2]Although women who go to the CSB (public medical center) for childbirth can obtain nets there, during the free distribution, there was no other way to receive nets for free. People in both areas can buy mosquito nets from NGOs for 3000 Ariary.

[3]In the first wave, we only asked this question in reference to the previous month. In the second and third waves, we asked the same question in reference to the six months since the previous wave.

[4]Detecting symptoms such as anemia (Cohen and Dupas, 2010) or using a hemoglobin test (Blackburn et al., 2008) can be used as a proxy.

[5]This test has over 90% sensitivity (i.e., 1 minus the type I error probability) and specificity (i.e., 1 minus the type II error probability) for detecting a malaria infection.

[6]Because the data in the first wave do not distinguish among the different types of malaria, “malaria positive,” as used in this study, includes all four types of *Plasmodium* species in addition to *Plasmodium falciparum* (pf), the most serious one. Of the 272 malaria-positive cases in the second wave, 179 are pf positive, 81 are mixed positive, and 12 are non-pf positive.

treatment group dummy on a set of these observables, treatment status is orthogonal to the observables conditional on malaria risk. Panel B in Table 2 displays the household-level variables: number of nets owned, household size, number of members who sleep under nets, household knowledge about the effectiveness of mosquito nets for malaria prevention, and altitude.[7] Except for the number of nets and altitude, these variables were well-balanced. According to a joint test, the orthogonality of treatment status is not necessarily rejected. In any case, to cope with the potential bias arising from baseline imbalance, we controlled for the dummies representing the high malaria risk village, adding their interaction terms with the wave fixed effects.[8] To examine the pre-trend, we visited two local clinics on the main road to obtain the date of the malaria infection patterns before the LLIN distribution. Figure 3 shows that the two regions exhibit largely similar trends, supporting the parallel trend assumption.

To check the systematic attrition of the panel, we regressed a dummy variable, which takes the value of one if a household has dropped from the panel and zero otherwise, on a set of observed variables such as the RDT-positive dummy. We found no correlation between the attrition dummy variable and key explanatory Variables, except household size, which showed a weak correlation.[9] However, if a household member was absent, we could not collect data on malaria infection by using the RDT. Nonetheless, the proportion of unavailable RDT cases was very low, approximately 3.2%.

2.3 School Attendance Record

Panels C and D in Table 2 show the descriptive statistics for the data from the school attendance records. To examine school absences and the absence rate, Panel C compares the January 2009 data to the June 2009 data.[10] Neither of these variables shows significant differences between the two regions. Panel D presents the comparison for June 2009 and December 2009, which corresponds to the first wave, showing nonsignificant differences. The histogram of the number of absence days during the first wave also confirms a baseline balance (Figure A1).

The number of absence days is smaller in the school record data than that in our household survey: attendance books record fewer than one absence day per month, whereas the survey data report more than one absence day per month. This discrepancy may be attributed to the nature of each dataset. Although

[7] B 淡 dker et al. (2003) show that altitude influences the risk of contracting malaria, but all households in our study were located below an altitude of 100 meters, which is a much lower variation than that considered by B 淡 dker et al. (2003).

[8] See the Appendix for the results when controlling for the other village-level variables.

[9] See Table A.1 for the results.

[10] The absence rate is adjusted for holidays and school closures.

absences in the household survey included cases where the teacher was absent, the school attendance sheet recorded teacher absences as holidays. However, absences due to sickness are captured well in both datasets.

3 Empirical Results

3.1 Effect of Free LLIN Distribution on Schooling

To investigate the impact of the free distribution of LLINs on school absences, we postulate the following econometric model:

$$\text{Absence}_{it} = \alpha_0 + \alpha_1 \text{ Distributed}_{it} + \text{Individual Fixed Effects}_i + \text{Wave Fixed Effects}_t + \epsilon_{it} \quad (1)$$

where Absence$_{it}$ is the number of absence days due to sickness in the past six months, Distributed$_{it}$ is an indicator variable that takes the value of one if an LLIN has been freely distributed and zero otherwise, and ϵ_{it} is the error term.[11]

Table 3 shows the estimation results based on our survey data; LLIN distribution decreased the number of absence days from school by 1.36 days per six months (see column (1)). The result is robust even after controlling for the village-level average number of absence days in the first wave (column (2)) as well as the time-variant effect of the high malaria risk dummy that takes the value of one if a village's malaria infection rate in the first wave exceeded 10% (column (3)). In columns (4)–(6), we employ school enrollment as an outcome variable. The point estimate of the treatment effect is not statistically significant in all the specifications. Since we have only 18 villages, the cluster-robust standard errors might over-reject the null hypothesis—even when using the finite sample adjustment. Following Cameron et al. (2008), we also show the p-value for testing the null hypothesis that the treatment variable has no effect, using the wild bootstrap method, but the qualitative results remain the same regardless of the standard errors used.[12]

As an additional robustness check, especially against any possible recall errors, we analyze the school attendance book data.[13] By using the number of absence days per month as the outcome variable, we estimate equation (1). Table 4 summarizes the results for absence days.[14] In addition to the school fixed effects and

[11] Because, in the first wave, we did not ask in our survey about absence days in the previous six months, we only employ the second and third wave data to estimate this model.

[12] In Table A.3, we also control for the variables that did not pass the baseline balancing tests, but the result is unchanged.

[13] Primary school students are mainly 6–12 years old. Table A.2 shows the relationships between age and grade in the second wave of survey data.

[14] Table A.4 shows the results using the monthly absence rate as the outcome variable.

the wave effects, we consider seasonal fixed effects, a school-level trend, and school-level seasonal fixed effects. The average effect of LLIN use on absence days ranges from -0.116 to -0.191 days per month, which corresponds to -0.696 to -1.146 days per six months. These numbers are largely consistent with the point estimates we obtained from the survey data reported in Table 3.

3.2 Effect of Free Distribution on Mosquito Net Use

Figure 4 shows the usage rate of mosquito nets by age based on the 24-hour recall data. We can easily verify that people aged 6–20 did not use nets in the first wave (panels (C1) and (T1)). However, in the second wave, most young people in the treated group began to use a mosquito net (panel (T2)). To capture this more precisely, we regress the LLIN usage indicator variable on the treatment variable by using the full sample as well as the sample of children aged 6–12. The results reported in Panel A of Table 5 confirm that the distribution of free LLINs increased net usage, especially for children aged 6–12.[15] To explore the reasons behind the lack of mosquito net usage by children aged 6–12, we examine the responses to the subjective questions on mosquito net use conditions. According to the regression analysis, these children are not using nets partially because they sleep on the floor and with fewer parents.[16][17]

In Panel B of Table 5 , we only use households that had more than 0.5 nets per member in the first stage. Because typically about two person can share one mosquito net, these households had sufficient number of mosquito nets before the free distribution. As a result, we do not see an economically and statistically significant increase in net usage after the free distribution.

3.3 Effect of Mosquito Net Usage on the Infection Rate

To understand the link between mosquito net use and schooling, it is critical to determine whether using a mosquito net can reduce the malaria infection rate. First, we adopt the two-stage least squares (2SLS) approach by regressing a binary variable for malaria infection on a binary net-use variable, instrumented by the LLIN treatment variable, using the second wave data. Since these two variables are binary, the estimated coefficient is the local average treatment effect (LATE) of Angrist and Imbens (1995). As shown in

[15] See Table A.5 for additional robustness check results.

[16] Table A.6 shows the results. Column (1) shows these children's lower usage rate but the coefficient becomes close to zero once we control for a dummy variable indicating whether they sleep on the floor or the number of parents they sleep with in columns (2)–(4). In column (5), we include the interaction terms of these controls with the dummy for children aged 6–12 but we find quantitatively small coefficients for these interactions, which implies that these controls explain well why children aged 6–12 are not using mosquito nets.

[17] Another finding in Figure 4 is that the treatment group's usage rate decreased in the third wave, suggesting negative learning effects. We will discuss how this affects our analysis using an instrumental variable (IV) in Subsection 3.4.

column (1) of Table 6, we find a significantly negative coefficient. The result is robust even after additionally controlling for the high malaria risk village dummy (column (2)) and the malaria infection rate at the village level (column (3)).

We also undertake an IV estimation, using the data from all the waves (see columns (4)–(6) in Table 6), finding significantly negative treatment effects. Considering that the baseline infection rate is 13%, the effect size seems to be substantial, ranging from -0.62 to -0.55. Overall, we find that LLIN usage decreased malaria infection almost consistently.[18]

3.4 Effect of Mosquito Net Use or Malaria Infection on Schooling

We also perform a regression analysis to uncover the effect of mosquito net usage on the number of absence days. By employing a cross-sectional IV estimation using the second-wave data, we find that using nets decreases sick absences by 11.09–13.45 days in six months (see columns (1)–(3) in Table 7). The effect size corresponds to 2.2–2.67 standard deviations of the sick absence variable in the second wave. To incorporate individual time-invariant heterogeneities, we conduct an FE-IV analysis using the second and third waves (see columns (4)–(6)). The effect size in columns (1)–(3) is larger than that in columns (4)–(6). This difference can be explained by the negative learning effects of net usage: among those who had received nets during the second wave, the proportion of people using nets declined in the third wave (See Section A.1 in the Appendix for the detail). When we only use the sample of those using nets in the third wave, the point estimates approach those reported in columns (1)–(3).[19]

There are two caveats in our results. First, the negative learning effect may be seen as inconsistent with Dupas (2014b), but the different baseline usage rates can cause this. The shares of household members that slept under a net the previous night in Dupas (2014a) and in our data are 0.41 and 0.78, respectively. Therefore, even if we assume the same distribution of learning effects, those who started to use nets in our setting would have systematically weaker learning effects than those in the setting of Dupas (2014a). Second, even with the negative learning effects due to the high baseline net usage rate, we find that using nets can still generate positive impacts on human capital accumulation.

To assess the direct effect of malaria infection on absences, we use the malaria infection variable as an independent variable and distribution as an IV. The results reported in Panel B of Table 7 show that malaria

[18]See Table A.7 for additional robustness checks.

[19]For example, -5.701 in column (4) becomes -9.70. The other results are available upon request.

infection increases absence days for children aged 6–12.[20]

3.5 Robustness Check

Since the mosquito nets (Olyset net ®, Sumitomo Chemical) distributed in the campaign are treated by insecticides, there could be positive externalities of the treatment across the regional border (Hawley et al., 2003), potentially causing our treatment effect estimates to be downward biased. According to Gimnig et al. (2003), an LLIN's externality is valid within 600 meters. The boundary was marked by a river, and the distance between the households on the different sides of the river was over 600 meters in most cases. Hence, in our setting, positive externalities are not necessarily severe. In contrast, another type of externality may exist among the individuals in each household. We thus examine the existence of such externalities based on pre-distribution information, using the following specification:[21]

$$\begin{aligned} \text{RDT}_{it} &= \alpha_0 + \alpha_1 \text{Distributed}_{it} + \alpha_2 \text{Distributed}_{it} * \text{Non-User Prop in HH}_i \qquad (2) \\ &+ \text{Wave Fixed Effects}_t + \text{Individual Fixed Effects}_t + \text{Wave Fixed Effects}_t * \text{Non-User Prop in HH}_i + \epsilon_{it}, \end{aligned}$$

where RDT_{it} is the malaria infection dummy and Non-User Prop in HH_i is a variable defined as the proportion of household members not using nets in the first wave. If there are positive externalities from using nets, the coefficient α_2 will be negative; if a respondent is surrounded by non-user household members, the gain from the externalities will be larger. As shown in Table 8, in all specifications, the estimated coefficient, α_2, is not statistically significant, lending no support to the existence of positive externalities. In columns (3) and (4), to detect any externalities within each household, we also use another variable, Infection Rate in HH_i, which represents the household-level infection rate during the first wave. The results again do not support the existence of positive externalities.

Another way to test the externalities of LLIN through the pesticides is to use an only within-household variation of usage of the mosquito nets to estimate the effect on the malaria infection. Consider the following

[20] See Table A.8 for additional robustness checks.

[21] We also tried to estimate this externality by using the distance from the border as an instrument for the neighbors' usage rate. However, because the location of houses does not exhibit a wide distribution, as seen in Figure 2, there was insufficient variation in the distance from the border to estimate the effect.

model for the second period as being the true model:

$$\text{RDT}_{ih} = \beta_0 + \beta_1 \text{Sleep in a Net}_{ih} + \beta_2 \text{Sleep in a Net}_h + \epsilon_{ih}, \tag{3}$$

where Sleep in a Net_h is the usage rate of mosquito nets in i's household h.

Then, if we estimate the following model using Distributed_{ih} as an instrument,

$$\text{RDT}_{ih} = \beta_0 + \beta_1 \text{Sleep in a Net}_{ih} + \epsilon_i, \tag{4}$$

the estimated β_1 (i.e., $\hat{\beta}_1$) might be biased due to omitting β_2Sleep in a Net_h, which Distributed_{ih} also affects. On the other hand, if we take the within-household difference

$$\text{RDT}_{ih} - \text{RDT}_{jh} = \beta_1(\text{Sleep in a Net}_{ih} - \text{Sleep in a Net}_{jh}) + \epsilon_{ih} - \epsilon_{jh}, \tag{5}$$

where j refers to the household head in i's household without loss of generality, we obtain a consistent estimate of β_1 ($= \tilde{\beta}_1$) by 2SLS.

If the externalities play a key role in reducing malaria infection, $\hat{\beta}_1$ would be much smaller than $\tilde{\beta}_1$. Table 9 shows the comparison; the values of $\hat{\beta}_1$ and $\tilde{\beta}_1$ are similar.[22] Overall, these two analyses do not support the existence of externalities at the household level.

We also investigate whether bias arises from the potential "announcement effect," namely, that people might have known about the free distribution campaign more than six months in advance and this prevented them from buying nets. This effect would lead to overestimation of the treatment effect of the free distribution on mosquito net usage. In the third wave, we asked respondents whether they knew about the free distribution campaign and its timing beforehand. Only 4% of households knew about the forthcoming free distribution. However, even without these households with prior knowledge of the free distribution, the estimation results remain the same as the original results (Table A.9). Therefore, any potential bias arising from the announcement effect is not necessarily serious.

To shed light on the direction of the endogeneity, we compare the OLS and IV estimations of the effect of mosquito net use on malaria infection and school absences (Table A.10). We find that the OLS coefficients

[22]A constant term can be added to the within-household specification when considering different baseline malaria risks between children aged 6–12 and their household head. In column (2), we keep the constant term in the main equation while we use the constant term as an additional instrument in column (3). In columns (4) – (6), we additionally control for the lag of malaria infection, but the results do not change substantially.

are consistently larger than the IV coefficients, suggesting that the OLS estimates are upward biased.

We also seek other possible explanations for the net usage effect on school absences. An alternative channel is income: net usage by the adult members of a household may increase labor supply and income, thereby raising school attendance rates. This channel may lead to the overestimation of the IV results because the distribution would positively affect the adults' labor supply or income, which is not captured directly in our regression models. To examine this channel, we analyze the impact of the distribution of LLINs on adults' malaria infection and household-level income. We find that net distribution nonsignificant effects on adults' malaria infection (see Table 10, columns (1) and (2)) and on household income (columns (3) and (4)). Therefore, this alternative channel does not necessarily explain the impact. Again, this finding is consistent with the results in Table 3: we find no effect on enrollment, which will be associated with household-level income.

3.6 Cost–Benefit Analysis

We perform three types of cost–benefit analyses. First, we restrict our attention to children aged 6–12. Based on our largest (smallest) estimated impact of the nets, we can calculate the lower (upper) bound of the cost for an additional year of schooling. As shown in Table 7, the largest (smallest) impact of using a net on school absences is -13.45 (-11.09) days in the rainy season.[23] Assuming there are 220 school days in a year and a zero effect in the dry season, to be conservative,[24] an additional year of schooling requires an additional 16.36 (19.84) net users among our survey respondents aged 6 to 12 years old. Since the cost of adopting nets is USD 0.68 per person annually (Sumitomo-Chemical (2010)),[25] this results in an estimated cost of USD 11.12 (13.49). This amount is comparable with a deworming treatment in Kenya (Miguel and Kremer, 2004; The Abdul Latif Jameel Poverty Action Lab, 2017) but is also more cost-effective than other policy interventions such as conditional cash transfers (see Figure 5).

Second, we estimate the total costs and benefits of a free universal distribution. Of our survey respondents, 561 pupils aged 6–12 are enrolled in school. Based on the results in Table 3, which capture the LLIN distribution effects on attendance in the rainy season of the second wave, the free distribution decreased absences by 1.362 days per person. Assuming zero effects in the dry season again, this implies that, over one year, it increased days of schooling by 561* 1.517 = 764.08 days (i.e., 3.47 years). On the contrary, 1608

[23] We are not able to capture the effect in the dry season. See Section A.1 in the Appendix for a discussion.

[24] Of the 52 weeks in the year, eight weeks are vacation periods and there are two weekend school holidays per week. Thus, there are 364-56-(52-8)*2=220 school days.

[25] This figure assumes nets last for five years when used by two people at the same time.

nets were actually distributed in the second and third waves, costing USD 2186.88 annually. Therefore, ignoring the delivery cost, 2186.88 / 3.47 = USD 630.22 would be needed for an additional year of schooling. This cost is much higher than the net usage cost of USD 11.12 – 13.49 required for an additional year of schooling, making the LLIN free distribution program one of the least cost-effective interventions listed in Figure 5. This high cost can be attributed to the substantial inefficiency in targeting households with children aged 6–12.[26] As we can see from Figure 4, except for those aged 6–20 years old, the respondents' usage rate is as high as 80%, indicating that the universal free distribution program involves large inclusion errors.[27]

Finally, we suggest a simple but more efficient targeting strategy: excluding households that do not have any children aged 6–12 from the distribution. This strategy is feasible if the government knows citizens' ages or can distribute the mosquito nets at schools. Also, this simple targeting strategy does not require the information about the number of nets possessed by each household. By excluding households without children aged 6—12 from the distribution campaign, the total number of mosquito nets that the government would have to distribute would decrease from 1608 to 698. This would reduce the distribution cost from USD 2186.88 to USD 949.28, which means that we would need 949.28/ 3.47 = USD 273.57 for an additional year of schooling. This is still much higher than USD 11.12 (or USD 13.49) but this targeted distribution improves efficiency by reducing inclusion errors and becomes more effective than, for example, the fellowship schools program in Pakistan aiming to reduce travel time (Figure 5).

4 Conclusion

By exploiting the natural experiment of the distribution of free LLINs to residents in Madagascar, we estimated the effect of mosquito net use on schooling. The results from two data sources, our own panel surveys and school attendance records, show that the LLIN distribution decreased the school absences of pupils aged 6–12 significantly. Our results suggest that this effect was driven by enhanced mosquito net use and better-controlled malaria infection. Using a mosquito net decreases school absences due to sickness by 16 days per child annually. On the contrary, we found no impact on school enrollment and household-level income, which may affect school enrollment indirectly. Moreover, our finding does not support the existence of any

[26] Universal coverage would be less efficient than a perfectly targeted distribution even if we included the targeting cost. Our household survey cost USD 50,467 during the first wave and there were 216 pupils aged 6—12 years old who were not using nets. Therefore, we only need 50,467 / 216 + 11.12 (or 13.49) = USD 244.76 (or USD 247.13) for an additional year of schooling by targeting pupils aged 6—12 who are not using nets, which is USD 407.70 (or USD 410.07) cheaper than universal coverage.

[27] Admittedly, the comparison assumes there is no social benefit from the free distribution other than the schooling effect for 6–12-year-olds, but it highlights the cost of universal coverage.

positive externalities arising from LLIN usage. According to our cost–benefit analysis, LLINs costing USD 11.12–13.49 provide an additional year of schooling to pupils aged 6–12. Hence, mosquito nets can be a powerful policy tool for decreasing malaria infections as well as for encouraging human capital investment. In contrast, a universal distribution campaign would cost USD 630.22 for an additional year of schooling, which highlights the significance of its inclusion errors. Such inclusion errors can be reduced when a simple and feasible targeting strategy is adopted: excluding households that do not have any members within the age range targeted by the distribution campaign. This targeting strategy would cut the cost for an additional year of schooling to USD 273.57.

A caveat in this study is that the external validity of these effects and the estimated costs depend on the local climate conditions (e.g., the length of the rainy season). Moreover, the efficiency of the distribution of LLINs depends on the potential mosquito-net usage rate and intra-household allocation to predict who would benefit from such a distribution. These issues are important future research topics.

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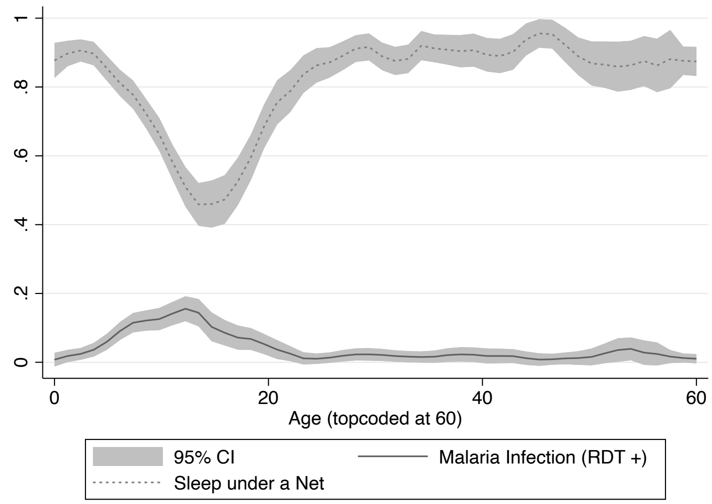
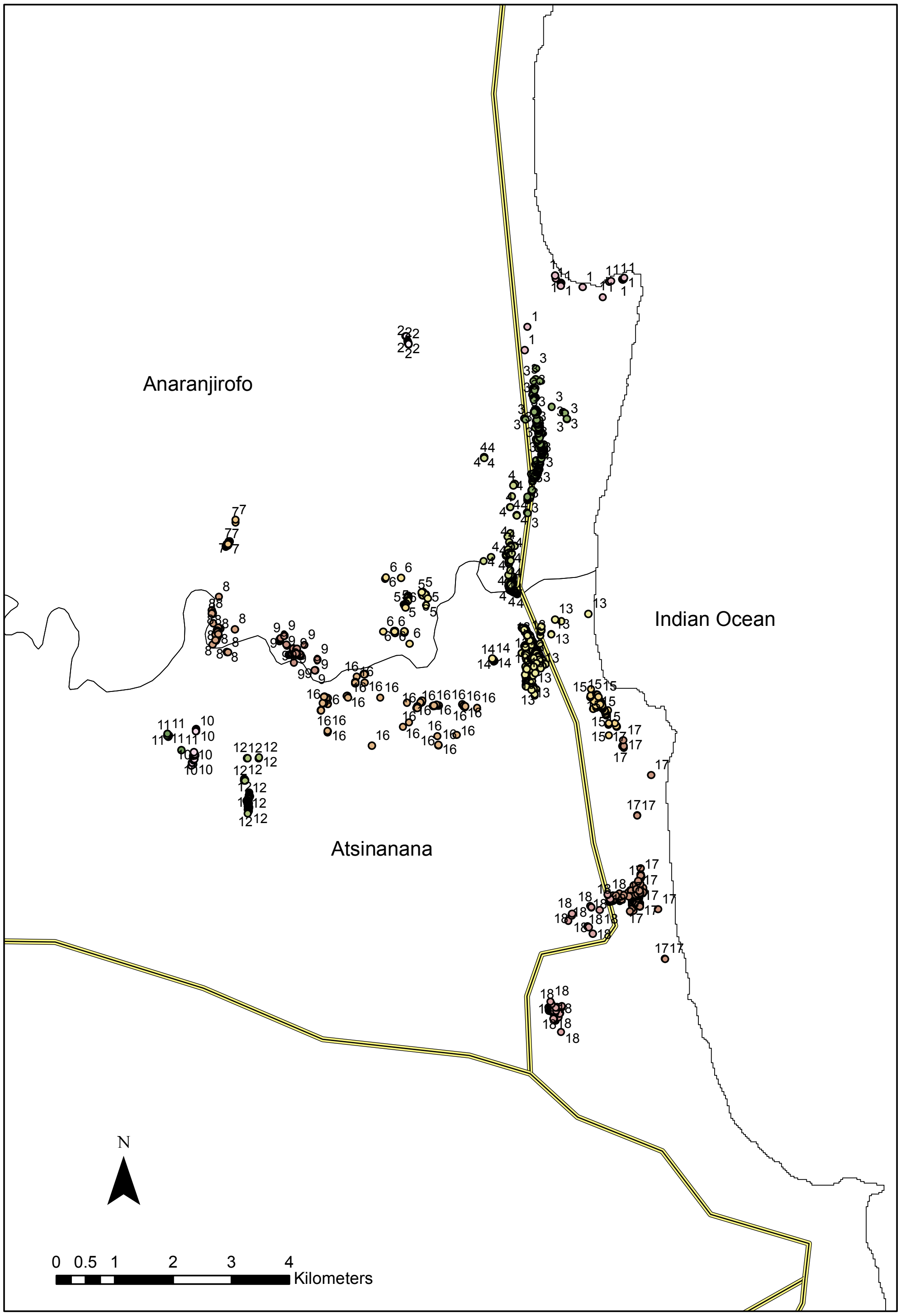
Figure 1: Malaria Infection and Mosquito Net Usage



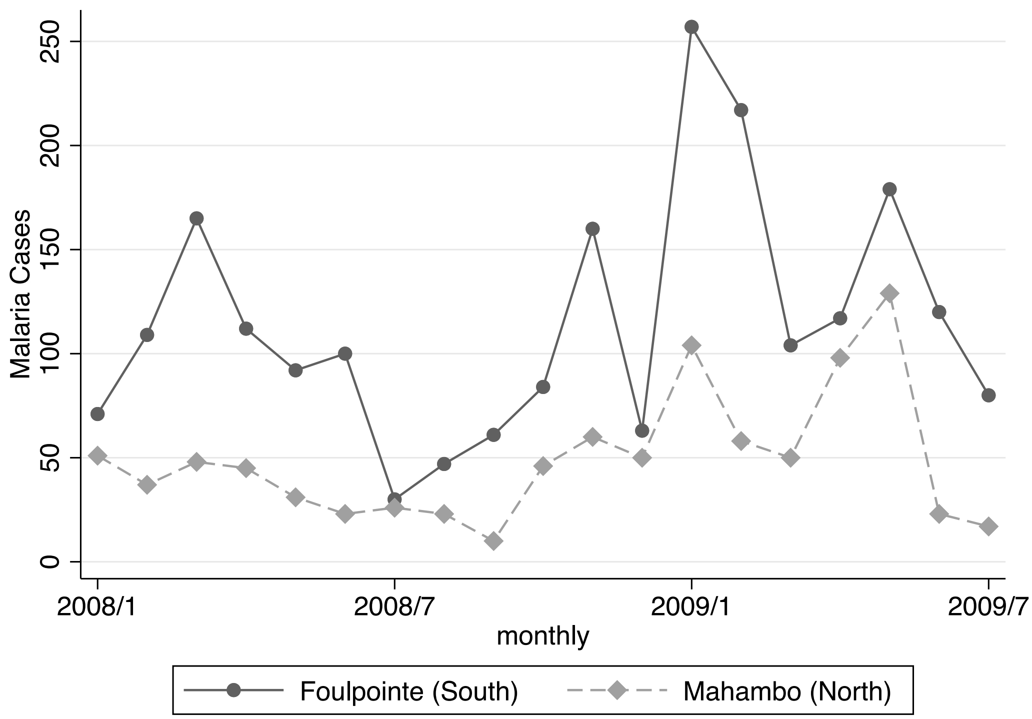
Notes: The two lines show the relationships between malaria infection (mosquito net usage) and age in our baseline survey respectively based on local polynomial regressions. We collect the data of mosquito net usage and malaria infection by household survey and rapid diagnosis test (RDT). See section 2 for the details.

Figure 2: Map of Households and Villages



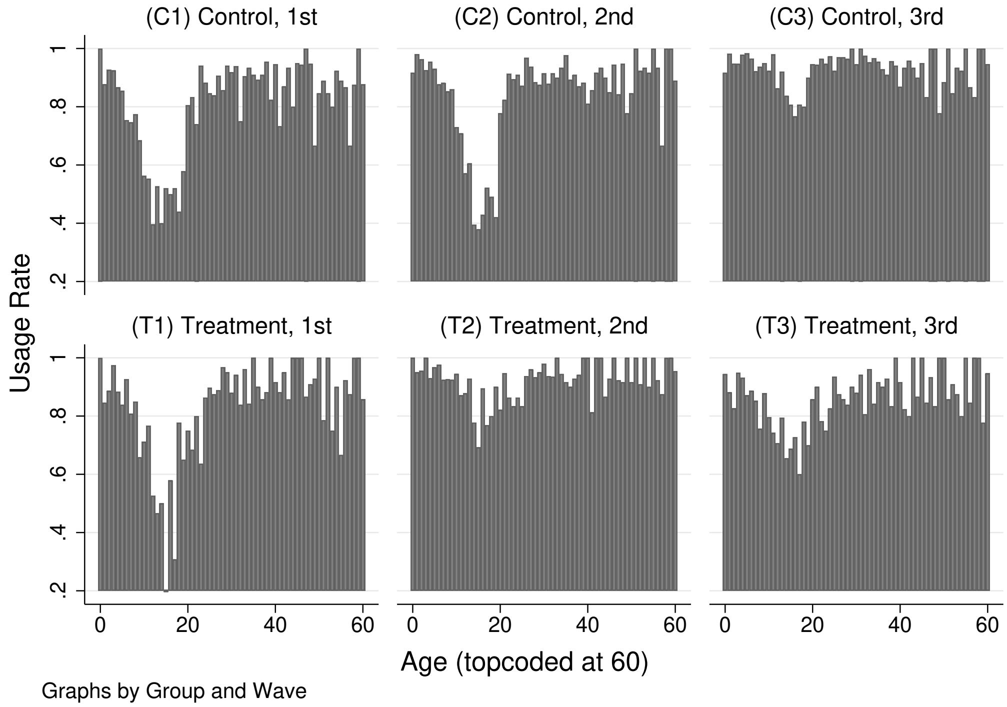
Notes: Yellow lines show main roads, the black line in the middle shows the boundary between the two provinces. Circles show households in our sample and the numbers above the circles indiate the villages.

Figure 3: Malaria Cases in Local Clinics



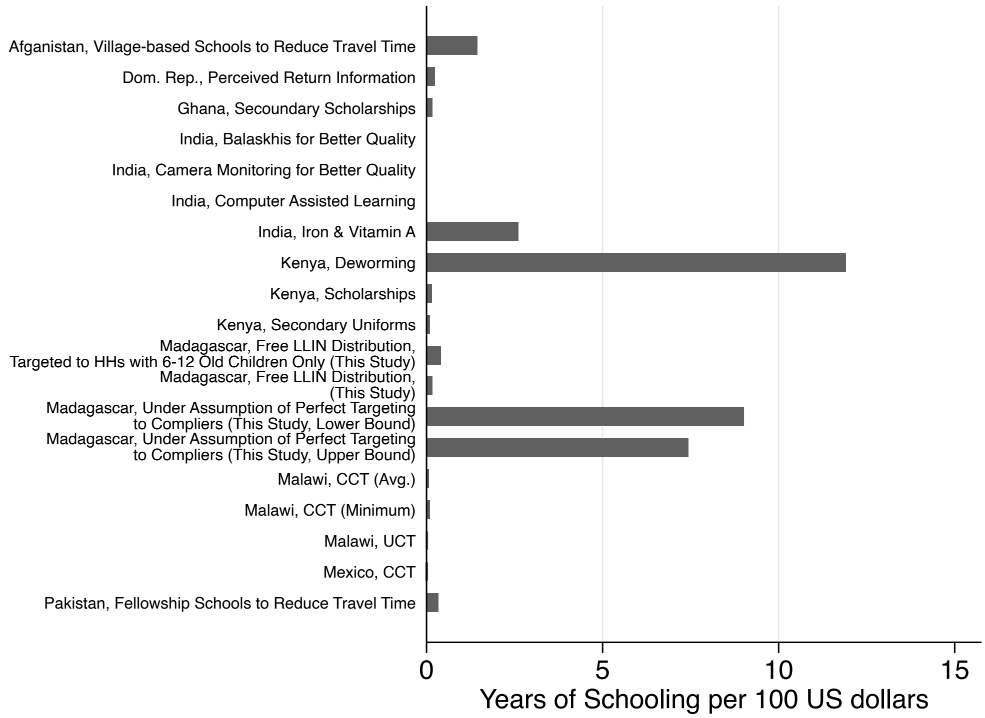
Notes: This graph shows the malaria cases in clinics in Foulpointe and Mahambo respectively.

Figure 4: Age and Net Usage on the Previous Night



Notes: This graph shows the usage rate of mosquito nets in each age, group and wave. Age is topcoded at 60.

Figure 5: Comparison of an Additional Year of Schooling for different countries and programs (in hundreds of USD)



Note: The other figures are taken from The Abdul Latif Jameel Poverty Action Lab (2017). Note that these figures take into account a discount rate and are based on 2004 US dollars. For simplicity, our study does not adjust these, but our estimated years of schooling would not change qualitatively, even with these adjustments.

Table 1: Data Collection and Free Distribution

	Dry season	Wet season		Dry season	
	Dec. 2009	Dec. 2009-Jan. 2010	May 2010	June 2010	Dec. 2010
Atsinanana Region	First Wave	Free Distribution	Second Wave		Third Wave
Analanjirofo Region	First Wave		Second Wave	Free Distribution	Third Wave

Table 2: Descriptive Statistics

	Mean of Control	Mean of Treatment	Diff	s.e.	Control N	Treatment N
Data from Our Survey						
Panel A: 6–12 Years Old						
RDT +	.1002278	.0639269	.0363008	.0185199	439	438
Sleep in a Net	.6219239	.6748879	-.052964	.031943	447	446
Absence Last Month	1.181495	1.152174	.0293207	.2789071	281	276
Sick Absence Last Month	.5907473	.692029	-.1012817	.1543376	281	276
Observations	893					
Panel B: Household-level Variables						
N. of Possesed Net	1.338594	1.53484	-.196246	.0458328	697	531
HH Size	3.466284	3.551789	-.085505	.106751	697	531
Members Using Net	2.647059	2.708098	-.0610391	.0941437	697	531
Knowledge of Usefulness	.7099494	.6795132	.0304362	.0280425	593	493
Altitude	57.01722	19.25424	37.76298	23.1725	697	531
Observations	1228					
Data from the School Attendance Records						
Panel C: Phase 0 (January/2009 – June/2009)						
Absence per Month	.3898121	.2977575	.0920547	.0340744	2768	2408
Absence Rate	.0253459	.0231562	.0021898	.0024031	2343	2032
Observations	5176					
Panel D: Phase 1 (June/2009 – December/2009)						
Absence per Month	.2286567	.235363	-.0067063	.0307002	1675	1708
Absence Rate	.0160048	.0185812	-.0025764	.0025488	1481	1611
Observations	3383					

The standard errors are the 18 village-level cluster-robust standard errors in Panels A and B and the 31 grade * school-level cluster-robust standard errors in Panels C and D. $^{+}$ $p < .10$, * $p < .05$, ** $p < .01$, and *** $p < .001$

Joint F-test for Age, Enrollment, RDT+, and Net Use, conditional on High Malaria Risk Village: 1.52, p-value = 0.24 (0.40 with wild bootstrap). Joint F-test for all the variables conditional on High Malaria Risk Village: 2.98, p-value = 0.04 (0.16 with wild bootstrap).

Table 3: Effect of Net Distribution on Schooling (Based on the Survey Data)

	Sick Absence (6 Months)			Enrollment		
	(1)	(2)	(3)	(4)	(5)	(6)
Distributed	-1.362*	-1.363*	-1.204*	-0.0384	-0.0311	-0.0305
	(0.602)	(0.576)	(0.468)	(0.0222)	(0.0238)	(0.0244)
Individual Fixed Effects and Wave Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Pre-village-level Absence * Wave Fixed Effects	No	Yes	Yes	No	No	No
Pre-village-level Enrollment * Wave Fixed Effects	No	No	No	No	Yes	Yes
Pre-high Malaria Risk Village * Wave Fixed Effects	No	No	Yes	No	No	Yes
p-value	0.038	0.031	0.020	0.102	0.209	0.229
Wild Bootstrap p-value	0.045	0.043	0.062	0.064	0.220	0.259
Number of Observations	1122	1122	1122	2147	2147	2147

Cluster-robust standard errors (18 village-level with a finite sample adjustment) are in parentheses. $^{+}$ $p < .10$, * $p < .05$, ** $p < .01$, and *** $p < .001$. In columns (1)–(3), we only use the second and third waves because we obtain data on the outcome variable after the first wave. In columns (4)–(6), we use all the waves. The *p-value* and *Wild Bootstrap p-value* are used to test whether the treatment coefficient is zero, using the standard error in the table and the cluster-robust wild bootstrap, respectively.

Table 4: Effect of Net Distribution on Absence Days (Based on the School Attendance Book)

	Absence per Month				
	(1)	(2)	(3)	(4)	(5)
Distributed	-0.191*	-0.167+	-0.159+	-0.145+	-0.116
	(0.0919)	(0.0853)	(0.0811)	(0.0809)	(0.115)
School Fixed Effects	Yes	Yes	Yes	Yes	Yes
Wave Fixed Effects	Yes	Yes	Yes	Yes	Yes
Season Fixed Effects	No	No	Yes	Yes	Yes
Linear Trend * School Fixed Effects	No	No	No	Yes	No
Season * School Fixed Effects	No	No	No	No	Yes
p-value	0.047	0.059	0.059	0.083	0.321
Wild Bootstrap p-value	0.082	0.095	0.095	0.161	0.369
Number of Observations	13928	19104	19104	19104	19104

The 31 grade * school-level cluster-robust standard errors are in parentheses. The *p-value* and *Wild Bootstrap p-value* are used to test whether the treatment coefficient is zero, using the standard error in the table and the cluster-robust wild bootstrap, respectively. $^{+}$ $p < .10$, * $p < .05$, ** $p < .01$, and *** $p < .001$. Column (1) uses only data after June 2009 as was done with the survey data, whereas the other columns include December 2008–June 2009. *Season Fixed Effects* capture monthly seasonal effects, taking the value of one for January, for example.

Table 5: Effect of Net Distribution on the Usage Rate

	Sleep under a Net			
	All Ages		6–12 Years Old	
	(1)	(2)	(3)	(4)
Panel A: All Households				
Distributed	0.129***	0.129***	0.154**	0.154**
	(0.0247)	(0.0241)	(0.0413)	(0.0433)
Panel B: All Households with More than 0.5 Nets per Member in the First Wave				
Distributed	0.0437	0.0434	-0.0561	-0.0515
	(0.0368)	(0.0369)	(0.0464)	(0.0482)
Individual Fixed Effects and Wave Fixed Effects	Yes	Yes	Yes	Yes
Pre-village-level Net Usage Rate * Wave Fixed Effects	No	Yes	No	Yes
p-value in Panel A	0.000	0.000	0.002	0.002
Wild Bootstrap p-value in Panel A	0.000	0.000	0.017	0.049
Number of Observations in Panel A	11232	11232	2148	2148
Observations in Panel B	1671	1671	181	181

Cluster-robust standard errors (18 village-level with a finite sample adjustment) are in parentheses. In panel A, we use all households, while we only use households that had more than 0.5 nets per member in the first wave per member. For the results in columns (1) and (2) all the samples are used, whereas for those in columns (3) and (4), the 6—12-year-old sample is used. The *p-value* and *Wild Bootstrap p-value* are used to test whether the treatment coefficient is zero, using the standard error in the table and the cluster-robust wild bootstrap, respectively. Data from all the waves are used.

$^{+}$ $p < .10$, * $p < .05$, ** $p < .01$, and *** $p < .001$

Table 6: Effect of Mosquito Net Use on Malaria Infection

	RDT +					
	2SLS (Second Wave)			FE-IV (All Waves)		
	(1)	(2)	(3)	(4)	(5)	(6)
Sleep under a Net	-1.094*	-0.735**	-0.452**	-0.620*	-0.554**	-0.545*
	(0.462)	(0.197)	(0.148)	(0.254)	(0.173)	(0.220)
Individual Fixed Effects and Wave Fixed Effects	No	No	No	Yes	Yes	Yes
Pre-high Malaria Risk Village * Wave Fixed Effects	No	Yes	Yes	No	Yes	Yes
Pre-village-level Infection Rate * Wave Fixed Effects	No	No	Yes	No	No	Yes
p-value	0.030	0.002	0.007	0.026	0.005	0.024
Wild Bootstrap p-value	0.008	0.004	0.036	0.014	0.017	0.065
Number of Observations	696	696	696	2043	2043	2043

Cluster-robust standard errors (18 village-level with a finite sample adjustment) are in parentheses. The *p-value* and *Wild Bootstrap p-value* are used to test whether the treatment coefficient is zero, using the standard error in the table and the cluster-robust wild bootstrap, respectively. In columns (1)–(3), we only use the second wave. In columns (4) – (6), we use all the waves.

$^{+}$ $p < .10$, * $p < .05$, ** $p < .01$, and *** $p < .001$

Table 7: Effect of Mosquito Net Use or Malaria Infection on Schooling

	Sick Absences (6 months)					
	2SLS (Second Wave)			FE-IV (Second & Third Waves)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Effect of Mosquito Net Use						
Sleep under a Net	-13.45^{+}	-13.45^{+}	-11.09*	-5.701^{+}	-5.704^{+}	-5.310^{+}
	(7.051)	(7.059)	(3.941)	(2.940)	(2.762)	(2.523)
Panel B: Effect of Malaria Infection						
RDT +	11.26*	11.26*	15.88**	13.92^{+}	14.21^{+}	14.58^{+}
	(4.507)	(4.519)	(4.556)	(7.313)	(7.230)	(8.237)
Individual Fixed Effects and Wave Fixed Effects	No	No	No	Yes	Yes	Yes
Pre-village-level Absence * Wave Fixed Effects	No	Yes	Yes	No	Yes	Yes
Pre-high Malaria Risk Village * Wave Fixed Effects	No	No	Yes	No	No	Yes
p-value in Panel A	0.0745	0.0748	0.0125	0.0704	0.0555	0.0515
Wild Bootstrap p-value in Panel A	0.051	0.073	0.056	0.066	0.062	0.097
Number of Observations in Panel A	573	573	573	1122	1122	1122
p-value in Panel B	0.024	0.024	0.003	0.075	0.067	0.096
Wild Bootstrap p-value in Panel B	0.013	0.022	0.027	0.027	0.033	0.068
Number of Observations in Panel B	563	563	563	1002	1002	1002

Cluster-robust standard errors (18 village-level with a finite sample adjustment) are in parentheses. We use distribution as an instrument. The *p-value* and *Wild Bootstrap p-value* are used to test whether the treatment coefficient is zero, using the standard error in the table and the cluster-robust wild bootstrap, respectively. In columns (1)–(3), we only use the second wave. In columns (4)–(6), we use the second and third waves.

$^{+}$ $p < .10$, * $p < .05$, ** $p < .01$, and *** $p < .001$

Table 8: Test of the Externalities of Net Distribution on Malaria Infection (1)

	RDT +			
	(1)	(2)	(3)	(4)
Distributed	-0.0724	-0.0826	-0.109**	-0.102**
	(0.0513)	(0.0492)	(0.0332)	(0.0306)
Distributed * Non-User Prop. in Household	-0.0436	-0.0499		
	(0.108)	(0.110)		
Distributed * Infection Rate in Household			-0.232	-0.198
			(0.285)	(0.305)
Individual Fixed Effects and Wave Fixed Effects	Yes	Yes	Yes	Yes
Pre-village-level Infection Rate * Wave Fixed Effects	No	Yes	No	Yes
Non-User Prop. in Household or Infection Rate in Household * Wave Fixed Effects	Yes	Yes	Yes	Yes
Number of Observations	2043	2043	2043	2043

Cluster-robust standard errors (18 village-level with a finite sample adjustment) are in parentheses. Data from all the waves are used. $^{+}$ $p < .10$, * $p < .05$, ** $p < .01$, and *** $p < .001$.

Table 9: Test of the Externalities of the Distribution on Malaria Infection (2)

	RDT +	Δ^{HH} RDT+		RDT +	Δ^{HH} RDT+	
	(1)	(2)	(3)	(4)	(5)	(6)
Sleep in a Net	-1.094*			-1.034*		
	(0.462)			(0.392)		
Δ^{HH} Sleep in a Net		-1.069**	-1.157***		-0.880**	-1.163***
		(0.339)	(0.234)		(0.256)	(0.274)
Constant	1.080*	0.0110		1.038*	0.0415***	
	(0.413)	(0.0165)		(0.365)	(0.00987)	
L.RDT +	No	No	No	Yes	No	No
L.Δ^{HH} RDT+	No	No	No	No	Yes	Yes
Number of Observations	696	670	670	663	623	623
Wild Bootstrap p-value	0.00400	0	0	0.00600	0.00200	0.00200

Cluster-robust standard errors (18 village-level with a finite sample adjustment) are in parentheses. $^{+}$ $p < .10$, * $p < .05$, ** $p < .01$, and *** $p < .001$.
Column (1) is a copy of column (1) in Table 6.
We use only the second wave.

Table 10: Effect of Net Distribution on Adults' Malaria Infection and Household-level Income

	RDT +		Log Household Income	
	(1)	(2)	(3)	(4)
Distributed	-0.0132^{+}	-0.00405	0.0234	-0.0400
	(0.00683)	(0.00880)	(0.0846)	(0.0681)
Individual Fixed Effects and Wave Fixed Effects	Yes	Yes	Yes	Yes
Pre-village-level Infection Rate * Wave Fixed Effects	No	Yes	No	No
Pre-Log Household Income * Wave Fixed Effects	No	No	No	Yes
Number of Observations	5112	5112	3167	3165

Cluster-robust standard errors (18 village-level with a finite sample adjustment) are in parentheses. $^{+}$ $p < .10$, * $p < .05$, ** $p < .01$, and *** $p < .001$. Columns (1) and (2) use the sample of adults over 20 years old. Columns (3) and (4) are measured at the household level. Data from all the waves are used.

A Appendix

This appendix contains a discussion of our estimator, a supplementary analysis, and robustness checks for the main results. Figure A1 displays the distribution of absence days in the first wave using the attendance book data. Table A.1 analyzes attrition. Table A.2 shows a classification of students in the second wave by age and grade. Table A.3, Table A.4, Table A.5, Table A.7, and Table A.8 present robustness checks for Table 3, Table 4, Table 5, Table 6, and Table 7, respectively. Table A.6 analyzes why children aged 6–12 did not use mosquito nets, based on the first-wave data. Table A.9 examines the announcement effect by excluding households that knew about the free distribution campaign one wave before it was conducted. Table A.10 compares the OLS and IV regression results.

A.1 Seasonality and Learning Effects

A.1.1 Setting

We reintroduce a true model to focus on what the Difference-in-Differences or IV estimators in Table 7 capture. Suppose the following model is the true model describing the relationship between the outcome (attendance, Y) and treatment variable (net use, N).

$$E[Y_{ijt}] = \alpha_{it} N_{ijt} + \eta_t + \mu_j$$

where i represents an individual, j the village, and t is time. η_t and μ_j are fixed effects. The DID reduced-form effect using the 2nd and 3rd waves is

$$\begin{aligned}
&E[Y_{ij3}|G = 0] - E[Y_{ij2}|G = 0] - \Big(E[Y_{ij3}|G = 1] - E[Y_{ij2}|G = 1]\Big) \\
&= E[Y_{ij2}|G = 1] - E[Y_{ij2}|G = 0] + \Big(E[Y_{ij3}|G = 0] - E[Y_{ij3}|G = 1]\Big) \\
&= \Big(E[\alpha_{i2} N_{ij2}|G = 1] - E[\alpha_{i2} N_{ij2}|G = 0]\Big) + \Big(E[\alpha_{i3} N_{ij3}|G = 0] - E[\alpha_{i3} N_{ij3}|G = 1]\Big), \qquad \text{(A.1)}
\end{aligned}$$

where $G = 1$ ($G = 0$) is south (north), which received nets in the second (third) wave.

Suppose the following model is the true model describing the relationship between the treatment variable and the distribution (D).

$$E[N_{ijt}] = \beta_{it} D_{jt} + \gamma_{it} L.D_{jt} + \xi_t + \psi_j,$$

where ξ_t and ψ_j are fixed effects and $L.D$ is a lag of D. γ_{it} is a learning effect: some people may stop using nets half a year after the distribution, after realizing their low effectiveness. The DID estimator for the first stage is

$$\begin{aligned}
&E[N_{ij3}|G=0] - E[N_{ij2}|G=0] - \Big(E[N_{ij3}|G=1] - E[N_{ij2}|G=1]\Big) \\
&= \Big(E[\beta_{i2}D_{j2}|G=1] - E[\beta_{i2}D_{j2}|G=0]\Big) + \Big(E[\beta_{i3}D_{j3}|G=0] - E[\beta_{i3}D_{j3}|G=1]\Big) \\
&\quad + \Big(E[\gamma_{3i}L.D_{j3}|G=0] - E[\gamma_{3i}L.D_{j3}|G=1]\Big).
\end{aligned}$$

The second term will be zero because the distribution effect β_{i3} is assumed to be the same in the two regions. The third term will be equal to $-E[\gamma_{3i}L.D_{j3}|G=1]$ because the learning effect will be zero in the third wave for $G=0$, as households in this region have only recently received nets. Therefore, the DID estimator for the first stage will correspond to $\Big(E[\beta_{i2}D_{j2}|G=1] - E[\beta_{i2}D_{j2}|G=0]\Big) - E[\gamma_{3i}L.D_{j3}|G=1]$.

A.1.2 Zero-Learning Effect Case

When $\gamma_{it}=0$ for all individuals, $-E[\gamma_{3i}L.D_{j3}|G=1]=0$. Further, $\Big(E[\alpha_{i3}N_{ij3}|G=0] - E[\alpha_{i3}N_{ij3}|G=1]\Big)$ in equation (1) will be zero because both regions would show the same level of attendance in the third phase after controlling for fixed effects.

The Wald estimator (equal to 2SLS after taking the first difference) using the DID estimators above as the denominator and numerator becomes

$$\frac{E[\alpha_{i2}N_{ij2}|G=1] - E[\alpha_{i2}N_{ij2}|G=0]}{E[\beta_{i2}D_{j2}|G=1] - E[\beta_{i2}D_{j2}|G=0]}.$$

and this will be the LATE for the effect in the second wave for people who will start using a net because of the distribution in the second wave. This is the standard result in the literature. A corollary of this result is that we only capture the effect in the second wave (rainy season), not in the third wave. Therefore, when we calculate the cost effectiveness per year, we have to assume a value for the effect in the dry season; in the main text, we used 0 to be conservative.

A.1.3 Non-zero Learning Effect Case

When γ_{it} is not zero, the Wald estimator will be

$$\frac{\left(E[\alpha_{i2}N_{ij2}|G=1]-E[\alpha_{i2}N_{ij2}|G=0]\right)+\left(E[\alpha_{i3}N_{ij3}|G=0]-E[\alpha_{i3}N_{ij3}|G=1]\right)}{\left(E[\beta_{i2}D_{j2}|G=1]-E[\beta_{i2}D_{j2}|G=0]\right)-E[\gamma_{3i}L.D_{j3}|G=1]}. \tag{A.2}$$

Because some people stopped using nets in $G = 1$ ($\gamma_{it} < 0$ on average) in the third wave, the second term in the denominator will be positive and the denominator will be larger. The numerator will be also larger because N_{ij3} will be smaller for $G = 1$ on average. Therefore, we cannot predict whether the DID estimator might show a larger or smaller effect compared to the zero-learning effect case.

However, consider a very plausible case: when people stop using nets ($\gamma_{3i} < 0$) only if their effect (α_{i3}) is zero and other people keep using nets, $\gamma_{3i} = 0$ because $\alpha_{i3} > 0$. We call this the case of disappointment. Then,

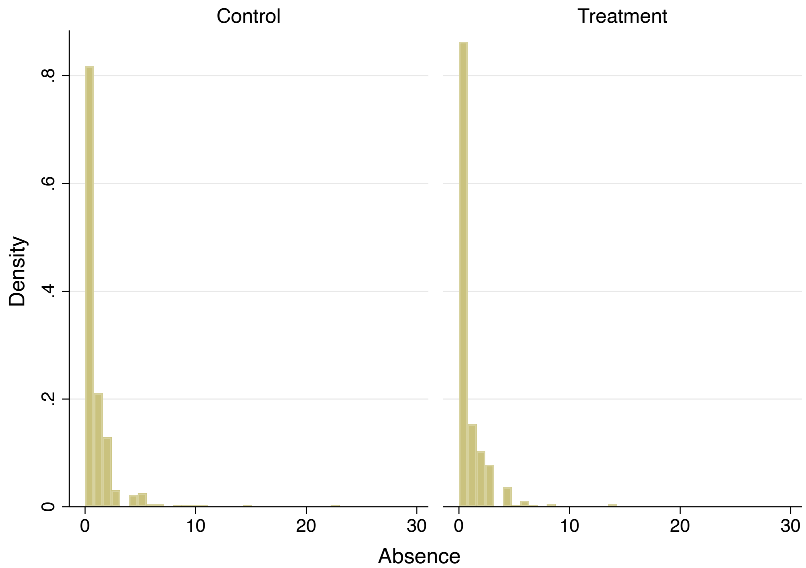
$$\begin{aligned}
&E[\alpha_{i3}N_{ij3}|G=0]-E[\alpha_{i3}N_{ij3}|G=1]\\
&=Pr(\gamma_{i3}=0)\left[E[\alpha_{i3}N_{ij3}|\gamma_{3i}=0,G=0]-E[\alpha_{i3}N_{ij3}|\gamma_{3i}=0,G=1]\right]\\
&+Pr(\gamma_{i3}<0)\left[E[\alpha_{i3}N_{ij3}|\gamma_{3i}<0,G=0]-E[\alpha_{i3}N_{ij3}|\gamma_{3i}<0,G=1]\right]\\
&=Pr(\gamma_{i3}=0)\left[E[\alpha_{i3}N_{ij3}|\gamma_{3i}=0,G=0]-E[\alpha_{i3}N_{ij3}|\gamma_{3i}=0,G=1]\right]\\
&+Pr(\gamma_{i3}<0)\left[E[\alpha_{i3}N_{ij3}|\alpha_{i3}=0,G=0]-E[\alpha_{i3}N_{ij3}|\alpha_{i3}=0,G=1]\right]\\
&=0+0
\end{aligned}$$

assuming $F(\gamma_{it}|G) = F(\gamma_{it})$ and $F(\alpha_{i3}N_{ij3}|\gamma_{3i}, G) = F(\alpha_{i3}N_{ij3}|\gamma_{3i})$. The first term is zero because the terms in the brackets will cancel each other out, and the second term is zero because $\alpha_{i3} = 0$. Therefore, in this case, only the denominator will increase and the Wald estimator will show a smaller effect than that in the zero-learning effect case.

When using the data in the second wave to conduct a cross-sectional 2SLS, we will estimate the LATE without a learning effect. In that case, the difference between the cross-sectional 2SLS and DID-IV estimates (columns (1)–(3) and (4)–(6) in Panel A of Table 7) would be explained by the population parameter to be estimated and the bias would stem from the fixed effects. If the bias is not the issue (the cross-sectional 2SLS identifies the LATE), then we will obtain a point estimate similar to that with DID-IV by restricting the sample to those using nets in the third period ($N_{i3} = 1$) because these people would exhibit a small learning effect.

In Panel B of Table 7, we use malaria infection as a treatment variable and do not observe a difference between columns (1)–(3) and (4)–(6). This is because the presence of learning effects will not change the reduced-form impacts on school absences, the numerator of the Wald estimator. The denominator of the Wald estimator in Panel B, the reduced-form impact on malaria infection, will not change by the learning effect when we consider the case of disappointment in malaria infection. Therefore, both the cross-sectional 2SLS and DID-IV will estimate the same population parameter.

Figure A1: Distribution of Absence Days in the First Wave (Based on Attendance Book Data)



Notes: This graph shows the distribution of absence days recorded in the attendance book in the first wave (June 2009 – December 2009).

Table A.1: Attrition

	Dropped After the First Wave				
Variables in the First Wave	(1)	(2)	(3)	(4)	(5)
RDT +	0.0381				0.0306
	(0.0230)				(0.0215)
Sleep under a Net		-0.000607			0.00557
		(0.0102)			(0.00874)
Absence Last Month			0.00377		0.00340
			(0.00297)		(0.00299)
Household size				-0.00654^{+}	-0.00459
				(0.00316)	(0.00347)
Constant	0.0181**	0.0231*	0.0126	0.0564*	0.0292
	(0.00555)	(0.0107)	(0.00879)	(0.0213)	(0.0237)
N	696	704	557	704	550
F_test					1.778

Cluster-robust standard errors (18 village-level with a finite sample adjustment) are in parentheses. We only use data from the first wave.

Table A.2: Classification by Age and Grade

	Grade				
	1	2	3	4	5
Age					
1	1	0	0	0	0
4	1	0	1	0	0
5	13	0	0	0	0
6	38	2	1	0	1
7	70	23	1	0	1
8	44	39	5	0	0
9	25	35	30	0	0
10	8	39	42	14	2
11	5	12	33	19	1
12	5	12	29	16	12
13	3	5	12	23	7
14	0	3	9	13	9
15	0	0	3	2	6
16	0	1	3	1	1
17	0	0	1	1	0
18	0	0	0	1	0

We used the pupils from the second wave.

Table A.3: Robustness Check: Effect of Net Distribution on Schooling (Based on the Survey Data)

	Sick Absence (6 Months)			Absence (6 Months)		
Panel A: Absence	(1)	(2)	(3)	(4)	(5)	(6)
Distributed	-1.362*	-1.242*	-1.216*	-3.095*	-1.222	-0.914
	(0.602)	(0.462)	(0.487)	(1.432)	(0.888)	(0.774)
Individual Fixed Effects and Wave Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Pre-village-level Absence * Wave Fixed Effects	No	Yes	Yes	No	Yes	Yes
Pre-high Malaria Risk Village * Wave Fixed Effects	No	No	Yes	No	No	Yes
Pre-village-level Net Use * Wave Fixed Effects	No	Yes	Yes	No	Yes	Yes
Altitude * Wave Fixed Effects	No	No	Yes	No	No	Yes
p-value	0.038	0.016	0.024	0.046	0.188	0.255
Wild Bootstrap p-value	0.045	0.065	0.076	0.072	0.301	0.318
Number of Observations	1122	1122	1122	1122	1122	1122

	Enrollment		
Panel B: Enrollment	(1)	(2)	(3)
Distributed	-0.0384	-0.0305	-0.0288
	(0.0222)	(0.0244)	(0.0262)
Individual Fixed Effects and Wave Fixed Effects	Yes	Yes	Yes
Pre-village-level Enrollment * Wave Fixed Effects	No	Yes	Yes
Pre-high Malaria Risk Village * Wave Fixed Effects	No	Yes	Yes
Pre-village-level Net Use * Wave Fixed Effects	No	Yes	Yes
Altitude * Wave Fixed Effects	No	No	Yes
p-value	0.102	0.228	0.287
Wild Bootstrap p-value	0.064	0.270	0.431
Number of Observations	2147	2147	2147

Cluster-robust standard errors (18 village-level with a finite sample adjustment) are in parentheses. $^{+}$ $p < .10$, * $p < .05$, ** $p < .01$, and *** $p < .001$. In Panel A, we only use the second and third waves. In Panel B, we use all the waves.

Table A.4: Effect of Net Distribution on the Absence Rate (Based on the School Attendance Book Data)

	Monthly Absence Rate				
	(1)	(2)	(3)	(4)	(5)
Distributed	-0.0131*	-0.0127*	-0.0125*	-0.0102^{+}	-0.0167^{+}
	(0.00557)	(0.00597)	(0.00588)	(0.00570)	(0.00978)
School Fixed Effects	Yes	Yes	Yes	Yes	Yes
Wave Fixed Effects	Yes	Yes	Yes	Yes	Yes
Season Fixed Effects	No	No	Yes	Yes	Yes
Linear Trend * School Fixed Effects	No	No	No	Yes	No
Season * School Fixed Effects	No	No	No	No	Yes
p-value	0.026	0.042	0.043	0.085	0.097
Wild Bootstrap p-value	0.055	0.059	0.064	0.152	0.175
Number of Observations	13928	19104	19104	19104	19104

The 31 grade * school-level cluster robust standard errors are in parentheses. The *p-value* and *Wild Bootstrap p-value* are used to test whether the treatment coefficient is zero, using the standard error in the table and the cluster-robust wild bootstrap, respectively. $^{+}$ $p < .10$, * $p < .05$, ** $p < .01$, and *** $p < .001$. Column (1) uses data only after June 2009 as in the survey data results, whereas the other columns include December 2008–July 2009. *Season Fixed Effects* capture monthly seasonal effects, taking the value of one for January, for example.

Table A.5: Robustness Check: Effect of Net Distribution on the Usage Rate

	Sleep under a Net			
	All Ages		6–12 Years Old	
	(1)	(2)	(3)	(4)
Panel A: All Households				
Distributed	0.129***	0.119***	0.154**	0.190***
	(0.0247)	(0.0278)	(0.0413)	(0.0373)
Panel B: All Households with More than 0.5 Nets per Member in the First Wave				
Distributed	0.0437	0.00549	-0.0561	-0.0490
	(0.0368)	(0.0366)	(0.0464)	(0.0620)
Individual Fixed Effects and Wave Fixed Effects	Yes	Yes	Yes	Yes
Pre-village-level Absence * Wave Fixed Effects	No	Yes	No	Yes
Pre-high Malaria Risk Village * Wave Fixed Effects	No	Yes	No	Yes
Pre-village-level Net Use * Wave Fixed Effects	No	Yes	No	Yes
Altitude * Wave Fixed Effects	No	Yes	No	Yes
p-value in Panel A	0.000	0.001	0.002	0.000
Wild Bootstrap p-value in Panel A	0.000	0.012	0.017	0.020
Number of Observations in Panel A	11232	11232	2148	2148
Number of Observations in Panel B	1671	1671	181	181

Cluster-robust standard errors (18 village-level with a finite sample adjustment) are in parentheses. In panel A, we use all households, while we only use households that had more than 0.5 nets per member in the first wave per member. Columns (1) and (2) use the sample of children aged 6–12 and the other samples whereas columns (3) and (4) only use the sample of children aged 6–12. The *p-value* and *Wild Bootstrap p-value* are used to test whether the treatment coefficient is zero, using the standard error in the table and the cluster-robust wild bootstrap, respectively. Data from all the waves are used. $^{+}$ $p < .10$, * $p < .05$, ** $p < .01$, and *** $p < .001$.

Table A.6: Why Do Children Not Sleep under Mosquito Nets?

	Sleep under a Net				
	(1)	(2)	(3)	(4)	(5)
6–12 years old	-0.133***	-0.0817**	-0.127***	-0.0770*	-0.0716*
	(0.0263)	(0.0280)	(0.0280)	(0.0295)	(0.0281)
Sleep on the Floor		-0.195***		-0.193***	-0.206***
		(0.0253)		(0.0247)	(0.0287)
No. of Household Members Children Sleep With			0.0440*	0.0415*	0.108***
			(0.0168)	(0.0164)	(0.0193)
Household FEs	No	No	No	No	Yes
Number of Observations	3249	3249	3249	3249	3249

Cluster-robust standard errors (18 village-level with a finite sample adjustment) are in parentheses. $^{+}$ $p < .10$, * $p < .05$, ** $p < .01$, and *** $p < .001$. We use the first-wave data.

Table A.7: Robustness Check: Effect of Mosquito Net Use on Malaria Infection

	RDT +					
	2SLS (Second Wave)			FE-IV (All Waves)		
	(1)	(2)	(3)	(4)	(5)	(6)
Sleep under a Net	-1.094*	-0.713**	-0.415*	-0.620*	-0.528*	-0.533*
	(0.462)	(0.187)	(0.151)	(0.254)	(0.225)	(0.231)
Individual Fixed Effects and Wave Fixed Effects	No	No	No	Yes	Yes	Yes
Pre-high Malaria Risk Village * Wave Fixed Effects	No	Yes	Yes	No	Yes	Yes
Pre-village-level Infection Rate * Wave Fixed Effects	No	No	Yes	No	No	Yes
Pre-village-level Absence * Wave Fixed Effects	No	Yes	Yes	No	Yes	Yes
Pre-village-level Net Use * Wave Fixed Effects	No	Yes	Yes	No	Yes	Yes
Altitude * Wave Fixed Effects	No	Yes	Yes	No	Yes	Yes
p-value	0.030	0.001	0.014	0.026	0.032	0.034
Wild Bootstrap p-value	0.008	0.006	0.047	0.014	0.055	0.082
Number of Observations	696	696	696	2043	2043	2043

Cluster-robust standard errors (18 village-level with a finite sample adjustment) are in parentheses. $^{+}$ $p < .10$, * $p < .05$, ** $p < .01$, and *** $p < .001$. In columns (1)–(3), we only use the second wave. In columns (4)–(6), we only use the second and third waves.

Table A.8: Robustness Check: Effect of Mosquito Net Use or Malaria Infection on Schooling

	Sick Absence (6 months)					
	2SLS (Second Wave)			FE-IV (second & third Waves)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Effect of Mosquito Net Use						
Sleep under a Net	-13.45^{+}	-10.33^{**}	-10.47^{**}	-5.701^{+}	-5.038^{**}	-5.130^{*}
	(7.051)	(3.117)	(3.396)	(2.940)	(1.592)	(2.148)
Panel B: Effect of Malaria Infection						
RDT +	11.26^{*}	9.840^{*}	14.68^{***}	13.92^{+}	16.95^{+}	18.56^{+}
	(4.507)	(4.356)	(3.483)	(7.313)	(8.381)	(10.41)
Individual Fixed Effects and Wave Fixed Effects	No	No	No	Yes	Yes	Yes
Pre-village-level Absence * Wave Fixed Effects	No	Yes	Yes	No	Yes	Yes
Pre-high Malaria Risk Village * Wave Fixed Effects	No	No	Yes	No	No	Yes
Pre-village-level Net Use * Wave Fixed Effects	No	Yes	Yes	No	Yes	Yes
Altitude * Wave Fixed Effects	No	Yes	Yes	No	Yes	Yes
p-value in Panel A	0.074	0.004	0.007	0.070	0.006	0.030
Wild Bootstrap p-value in Panel A	0.051	0.065	0.094	0.066	0.031	0.082
Number of Observations in Panel A	573	573	573	1122	1122	1122
p-value in Panel B	0.024	0.038	0.001	0.075	0.060	0.094
Wild Bootstrap p-value in Panel B	0.013	0.145	0.026	0.027	0.053	0.081
Number of Observations in Panel B	563	563	563	1002	1002	1002

Cluster-robust standard errors (18 village-level with a finite sample adjustment) are in parentheses. We use distribution as an instrument. $^{+}$ $p < .10$, * $p < .05$, ** $p < .01$, and *** $p < .001$. In columns (1)–(3), we only use the second wave. In columns (4)–(6), we only use the second and third waves.

Table A.9: Result Excluding Households that Knew About the Free Distribution One Wave Before it was Conducted

	FE		FE-IV	
	Sick Absence (6 months)	Sleep under a Net	RDT +	Sick Absence (6 months)
	(1)	(2)	(3)	(4)
Distributed	-1.214^{+}	0.161**		
	(0.609)	(0.0464)		
Sleep under a Net			-0.553*	-4.912^{+}
			(0.235)	(2.749)
Wave Fixed Effects and Individual Fixed Effects	Yes	Yes	Yes	Yes
p-value	0.064	0.003	0.031	0.093
Wild Bootstrap p-value	0.073	0.015	0.011	0.093
Number of Observations	1068	2016	1926	1068

Cluster-robust standard errors (18 village-level with a finite sample adjustment) are in parentheses. $^{+}$ $p < .10$, * $p < .05$, ** $p < .01$, and *** $p < .001$. Columns (1) and (4) use the second and third waves and columns (2) and (3) use all waves.

Table A.10: Comparison between IV and OLS

	RDT +		Sick Absence (6 Months)	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Sleep under a Net	-0.150***	-0.442**	0.980*	-13.45+
	(0.0312)	(0.132)	(0.459)	(7.059)
Village-level Infection in the First Wave	Yes	Yes	No	No
Village-level Sick Absence in the First Wave	No	No	Yes	Yes
Number of Observations	696	696	573	573

Cluster-robust standard errors (18 village-level with a finite sample adjustment) are in parentheses. $^{+}$ $p < .10$, * $p < .05$, ** $p < .01$, and *** $p < .001$ We only use the second wave of data.