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Agricultural extension, intra-household allocation and malaria[☆]

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ABSTRACT

Can agricultural development programs improve health-related outcomes? We exploit a spatial discontinuity in the coverage of a large-scale agricultural extension program in Uganda to causally identify its effects on malaria. We find that eligibility for the program reduced the proportion of household members with malaria by 8.9 percentage points, with children and pregnant women experiencing substantial improvements. An examination of the underlying mechanisms indicates that an increase in income and the resulting increase in the ownership and usage of bednets may have played a role. Taken together, these results signify the importance of financial constraints in investments for malaria prevention and the potential role that agricultural development can play in easing it.

1. Introduction

Developing countries continue to bear a high share of the global infectious disease burden despite a number of large-scale interventions. This is particularly true in the case of malaria where, while there has been a rapid decline over the last decade, Sub-Saharan Africa accounts for approximately 90% of malaria-related deaths (WHO, 2015). Given that there may be substantial positive externalities associated with prevention of infectious diseases, an effective public health policy is crucial and this warrants a better understanding of individual demand for preventive health products.

Notwithstanding the substantial benefits, investments in health products remain low in developing countries (Dupas, 2011; Thurber et al., 2013). With financial constraints being one of the primary reasons for low uptake, the current debate has centered on the extent of subsidy - whether such health products should be provided for free or at some cost to the users (Sachs, 2005; Dupas, 2014). On the one hand, concerns over free provision include the cost of heavy subsidization, provision of wrong incentives in product usage, and the possibility that it might lead to expectations of free provision in the future. On the other hand, the neediest are often unable to afford even highly subsidized products. Further, households with inadequate resources may be forced to choose between protecting economically valuable members and others (Strauss and Thomas, 1995; Mwabu, 2007). One element missing from this discussion has been the possible role of concurrent income generating policies in easing these trade-offs. In this paper, we contribute to the literature by examining how a large-scale agricultural development intervention in Uganda reduced malaria, and if the most vulnerable members of the household benefit more from such interventions.

While many scholars believe that low income can increase malaria incidence (Berthélemy et al., 2013; Worrall et al., 2005) by limiting households' capacity to buy bednets, ensure adequate nutritional

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intake, and gain access to health care, causally testing this channel is a challenge due to reverse causality. In particular, malaria-related morbidity can reduce earnings through decreased labor productivity and workdays lost to disease (Fink and Masiye, 2015; World Bank, 2008). Existing studies that are closest to ours focus on interventions to relax credit constraints specifically for the purchase of preventive health products.[1] However, health product linked cash grants, by making health salient, could encourage take-up and thus overestimate the effect of income, and there is little evidence of the effect of a general increase in income on malaria (Dupas, 2011).

This paper tests if income generating programs in agriculture can play a role in malaria reduction. This is of tremendous policy relevance given that agriculture is the dominant source of income in Sub-Saharan Africa. In particular, we exploit an arbitrary distance-to-branch threshold that determines village eligibility for a large-scale agricultural extension program operated by BRAC Uganda, to causally identify the effects on malaria and explore possible channels. Using a regression discontinuity framework, we find that the agricultural extension program led to a reduction in prevalence of malaria in Uganda. For households residing in eligible villages, household level malaria prevalence (proportion of household members affected) reduced by 8.9 percentage points (or 29% of the control mean). Further, we observe substantial reductions in the prevalence of malaria are for children under 5, the most vulnerable group in the population. While there are no gender differences, pregnant women experience considerable reduction in malaria as well. These findings are robust to a number of checks standard in the literature. As exposure to health shocks in early life can have considerable economic losses over time, our finding that the program reduced malaria prevalence among pregnant women and children under 5 indicates that the extension services program is associated with substantial and potentially lifelong benefits that are not accounted for in standard cost-benefit analyses.

Upon carefully examining the underlying channels, we find an increase in income and bednet ownership to be among the possible explanations for this decline in malaria. While pre-intervention information indicates no discontinuity in income and bednet ownership, access to the agricultural extension services program increased both. Further highlighting the role of income in malaria reduction, we find that these effects are driven by program effects in areas that were poorer in the pre-intervention period. Lastly, we also examine and discuss several alternative mechanisms such as discontinuous access to various forms of health care, demand for health care and health-related information, agricultural practices, female bargaining power, fertility and mortality.

In sum, this paper makes the following key contributions. First, we contribute to the literature on the adoption of preventive health technologies by highlighting the role of financial constraints. Overall, liquidity is found to be an important factor constraining household investment in preventive health products (Devoto et al., 2012; Beltramo et al., 2015; Meredith et al., 2013; Guyatt et al., 2002). In the context of malaria prevention, sleeping under an insecticide treated bednet (ITN) is considered to be highly effective but usage remains low. Several empirical studies trace the low uptake of ITNs to financial constraints faced by low income households. For example, in an experimental setting in Uganda, Hoffmann et al. (2009) estimate that the income elasticity of the willingness to pay for an ITN is high (0.25) and find that it is the lack of cash, rather than a low willingness to pay the market price, that explains the low demand for bednets.[2] While this study and others such as Dupas (2009) and Tarozzi et al. (2014) relaxed the credit constraint specifically for the purchase of preventive health products (for example, micro-loans for bednets or the provision of free/subsidized bednets), there is little evidence of the effect of a general increase in income on preventive health investments (Dupas, 2011; Dupas and Miguel, 2017).[3] We fill this gap as we find that access to an agricultural extension program translated into reduction in malaria prevalence through an improvement in economic status and an increase in the number of bednets owned per capita.

Second, this research is linked to the literature on the intra-household allocation of health resources (see Strauss et al., 2000 and Mwabu, 2007 for recent overviews). Particularly, the distribution of bednets within households with few bednets is of considerable importance as households might have to decide between protecting those that are economically important (working-age adults) and those that biologically require more protection due to naturally lower immunity (children and pregnant women). Previous evidence from Uganda suggests that adults may receive priority over children in households with few bednets (Lam et al., 2014; Hoffmann, 2009). This paper sheds further light on this as our results indicate that exposure to the agricultural extension program (and the resulting easing of the income constraint) led to greater allocation of bednets to children.

Finally, while the possibility that an increase in the income generating capacity of agriculture can reduce malaria rates has been discussed widely in the literature (Asxenso-Okyere et al., 2011; World Bank, 2008; van der Hoek, 2004; Ijumba and Lindsay, 2001), causal identification has been challenging due to concerns related to omitted variable bias and reverse causality. We contribute towards filling this gap with evidence from a large-scale agricultural extension program that provides agricultural training and easier access and affordability of high yield variety seeds with the ultimate objective of improving food security and helping households graduate out of poverty.

The rest of the paper is organized as follows. Section 2 provides a background on malaria in Uganda and BRAC's agricultural extension program. Section 3 describes the data and outlines the estimation strategy. The main regression results on malaria are provided in Section 4. Section 5 explores the underlying mechanisms and Section 6 concludes.

2. Background

2.1. Malaria in Uganda

Plasmodium falciparum, the most deadly of the five human malaria parasites, is endemic in Uganda. Malaria spreads to people through the bites of infected female mosquitoes with the most common malaria transmitting vectors in Uganda being *Anopheles Gambiae*. Malaria is endemic in Uganda with over 90% of the population experiencing high transmission rates and malaria incidence, 232 cases per 1,000 population, is one of the highest in the world (WHO, 2015). Further, endemicity of malaria is quite stable - there are very few areas of unstable transmission and epidemics are uncommon. There is some amount of cyclicity as malaria incidence peaks during the two rainy seasons (March to May and September to December). In Uganda, Hoffmann et al. (2009) estimate the economic costs of a malaria episode - medical cost and the value of labor income lost - to be \$17.85 (or 7.2% of the average annual per capita non-health consumption

[1] More general studies exploit exogenous variations in household income to identify effects on health. For example, Duflo (2003) finds that an expansion in the coverage of the pension program in South Africa improved health outcomes for girls. Conversely, negative income shocks due to economic crises (Paxson and Schady, 2005), weather shocks (Jensen, 2000) or declines in output prices are found to adversely affect health (see Strauss and Thomas, 2007 for an overview).

[2] In related work, Cohen and Dupas (2010) find the demand for bednets to be highly price elastic. See Dupas (2011) for a review of the literature on health seeking behavior in developing countries.

[3] While there is some evidence of unconditional cash transfers on health (Baird et al., 2012; Haushofer and Shapiro, 2016), the effects of an income generating program - where income is earned and increases more permanent - may be different (Dasgupta and Mani, 2015).

expenditure). Further, the burden of malaria is disproportionately borne by poor households, largely due to the fact that they are unable to invest in malaria prevention methods such as bednets (UBOS and ICF, 2012; Uganda Ministry of Health, 2008).

The strategies to combat malaria are coordinated by the Uganda National Malaria Control Program (NMCP) and largely center around increasing the availability and usage of ITNs through a mix of commercial sale (full and subsidized prices) and free distribution to vulnerable groups (such as pregnant women through antenatal clinics). According to the Uganda Malaria Indicator Survey in 2009, the most common way of obtaining a bednet was through the open market, shops and pharmacies (UBOS and ICF Macro, 2010). While households' access to bednets has increased rapidly over the last decade, the proportion of population sleeping under a bednet is still low. The Uganda DHS, 2011 estimated that 74% of households reported owning at least one bednet but only 45% of the population had slept under a bednet the night preceding the survey.

Another strategy for vector control is to spray insecticide on the interior walls of a dwelling. According to the Uganda DHS in 2011, 7.2% of the households reported having had their houses sprayed in 2010–2011 (UBOS and ICF, 2012). While the overall use of indoor residual spraying (IRS) is low due to cost considerations, the government has from time to time funded regional mass spraying. An early experimentation with mass IRS took place in the southwestern district of Kigezi in 1959–61, but these efforts were not scaled up (Uganda Ministry of Health, 2008; Barofsky et al., 2015). More recently, a similar IRS program has been supported by the President's Malaria Initiative (PMI) and the Government of Uganda in 10 districts in the Northern Region of Uganda from October 2009 onwards (PMI, 2013). The survey data used in this study do not overlap with any of these districts.[4]

2.2. BRAC's agricultural extension program

Agricultural extension seeks to improve agricultural productivity by promoting the adoption of modern agricultural technologies via training and demonstrations. Given the potential to improve yields, decrease production cost, increase incomes and reduce poverty, agricultural extension programs have become a popular form of agricultural intervention in developing countries (Godtland et al., 2004; Kassie et al., 2011; Dercon et al., 2009; World Bank, 2008).

BRAC's agricultural extension program in Uganda was launched in 2008 and operated through 60 branches in 41 districts.[5] The program aims to increase the productivity of small, low-income women farmers by encouraging the adoption of modern cultivation techniques. This was done through two complementary arms - one provided agricultural training and the other improved access to inputs. In the first component, "model farmers" were selected, trained in modern cultivation techniques and then required to pass on that training to other peer farmers in the village. In the second component, community agriculture promoters (CAP) were selected from the same villages and provided subsidized HYV seeds to sell in their villages. There was no cap on the selling price and the objective was to increase the availability of HYV seeds in the village and at the same time help improve the entrepreneurial skills of the CAP. Between 2008 and June 2011, the program had engaged 1200 "model farmers" and reached almost 64,000 general farmers.[6]

The agricultural extension program was limited to villages lying within a radius of 6 km from each BRAC branch office as BRAC chose to evaluate the program using a spatial regression discontinuity design. In an effort to balance concerns regarding transportation costs for program assistants and trying to reach enough farmers, BRAC selected the 6 km boundary for the pilot. This threshold was later incorporated into the actual agricultural extension program and was implemented regardless of geography or population density. The program was effective in increasing the adoption of modern cultivation techniques and inputs that require minimal upfront monetary investment such as intercropping, crop rotation and the use of manure. This in turn translated into significant improvements in food security (Pan et al., 2018).

3. Data and estimation strategy

3.1. Data

The data used in this paper come from BRAC's agriculture survey conducted in July–December 2011, 3 years after the launch of the extension program. The survey used a two-stage cluster sampling process. First, in each of the counties that received the program, 17 villages were picked from the list of villages in a radius of 9 km around a branch. Next, in each of the selected villages, 25 households were randomly chosen for the survey. Fig. A1 in the online Appendix depicts the survey areas. The household survey collected information on all the usual members of the household including whether the member had suffered from malaria in the preceding six months. Other characteristics used in the analysis are the age and literacy of the household head, and a dummy variable indicating whether any member of the household is a member of a village level (or higher) committee. Finally, we also have the GPS coordinates of households which allows us to calculate the distance of the household and village from the nearest BRAC branch.

While surveyed villages were to lie within a radius of 9 km, in the data we observe a few villages that are further away. This is possibly due to measurement error in sampling or recording the GPS coordinates of the households. For the purpose of this analysis we choose to restrict the sample to villages lying within 10 km of a BRAC branch.[7] The summary statistics, reported in Table 1, show that the sample consists of 7206 households residing in 417 villages. The household head is on average 43.7 years old and 73.2% are literate. At the household level we define household malaria prevalence as the proportion of household members who reported experiencing malaria in the six months preceding the survey. The average household malaria prevalence is 28.7%, and there is considerable heterogeneity at the individual level with malaria prevalence being highest for children aged 5 or below (50%). While malaria prevalence is fairly similar across genders, 40% of pregnant women reported experiencing malaria in the six months preceding the survey.

Admittedly, self-reported prevalence of malaria may suffer from measurement error. Self-reported malaria may be both under-reported or over-reported.[8] While imperfect, Tarozzi et al. (2014) find self-

[4] The 10 districts covered are Kitgum, Agago, Lamwo, Pader, Amuru, Nwoya, Gulu, Oyam, Kole, and Apac. Household coverage is reported to be about 90% in these districts (PMI, 2013).

[5] BRAC branch offices are located closer to towns rather than villages for easy access to communication, transportation and financial services. As the BRAC branch offices were set up well before the design and implementation of the agricultural program, the locations can be considered pre-determined and viewed as exogenous to the potential benefits of the agricultural extension services program.

[6] As the objective was to reach as many households as possible, most of the branches do not overlap their service area and the average distance between branches is 28 km.

[7] While we use data of villages lying within 10 km, all non-parametric results reported in Tables 2–10 are robust to restricting the data to only villages that lie within a radius of 9 km.

[8] For example, under-reporting may occur due to asymptomatic incidence arising from repeated exposure to malaria (Laishram et al., 2012). Similarly, fever episodes maybe misdiagnosed as malaria leading to over-reporting. Still, self-reported prevalence may reflect episodes that were severe enough to be recognized by the household and serve as a valuable indicator of the economic burden of the disease (Tarozzi et al., 2014).

Table 1
Summary statistics.

Variable	Mean	SD
Household level		
HH head age	43.72	14.1
HH head literacy	0.732	0.443
HH size	6.16	2.84
Committee Member	0.330	0.470
Health Program	0.408	0.491
Household level malaria prevalence	0.287	0.320
Bednets owned per capita	0.400	0.462
Individual level malaria prevalence		
Ages 0-5	0.503	0.500
Ages 6-19	0.354	0.478
Ages 20-60	0.295	0.456
Males	0.336	0.472
Females	0.370	0.483
Pregnant	0.399	0.490
All	0.354	0.478
Number of observations		
Number of villages	417	
Number of households	7206	
Number of individuals	36517	

Notes: Household level malaria prevalence is the fraction of household members who reported being infected by malaria in the previous 6 months. Committee Member is a dummy variable that takes the value 1 if at least one household member belongs to a village committee, and 0 otherwise. Health Program is a dummy variable that takes the value of 1 if at least one household in the village reports the availability or utilization of the services of the BRAC health program. The sample is restricted to villages within 10 km of a BRAC branch office.

reported malaria to be strongly correlated with rapid diagnostic blood tests (RDTs) in India. Relating our self-reported measure to two clinically tested malaria prevalence rates available for Uganda during the period of the study gives us reasonable confidence in our measure. First, the self-reported measure relates closely to the clinical malaria prevalence rates estimated among children aged 2–10 years in Uganda during 2011 by the Malaria Atlas Project (Bhatt et al., 2015). For the set of overlapping districts, malaria prevalence is estimated to be 31% by the Malaria Atlas Project while it is 39% in the BRAC survey data for the same age group, and at the district level we find a correlation of 0.66 between the two samples. Second, the Malaria Indicator Survey of 2009 tested children aged 0–59 months for the malaria parasite through two technologies - microscopy testing and RDT. While microscopy testing found 45% of children to be infected, RDTs found prevalence to be 55% (UBOS and ICF Macro, 2010). Although the BRAC survey data are from 2011 and refer to a six month recall period, we also find a high prevalence of malaria among children aged 5 or below (50%).

3.2. Estimation strategy

The nature of the implementation of BRAC's agricultural extension program provides us an opportunity to assess its effect on the prevalence of malaria using a "sharp" regression discontinuity (RD) design. As per BRAC's rules, coverage under the program was limited to villages within the radius of 6 km of each BRAC branch office, making access to the agricultural extension program a discontinuous function of a continuous variable (distance to the nearest BRAC branch office). For every village, we computed each household's distance from the nearest BRAC branch using GPS coordinates and then used the median household's distance as a proxy for the distance of the village from the nearest BRAC branch. The running variable, z_j, is then defined as the distance of the village in kilometers from the cutoff point of 6 km:

$$z_j = d_j - 6 \tag{1}$$

where d_j is the distance of the village j from the BRAC branch. A village should have received the extension program if $z_j \leq 0$. As we restrict the data to villages lying within 10 km of each BRAC branch for the analysis, the running variable lies in the range [-6, 4]. Our RD model can be written as:

$$Y_{ij} = \alpha + \beta T_j + f\left(z_j\right) + \epsilon_{ij} \tag{2}$$

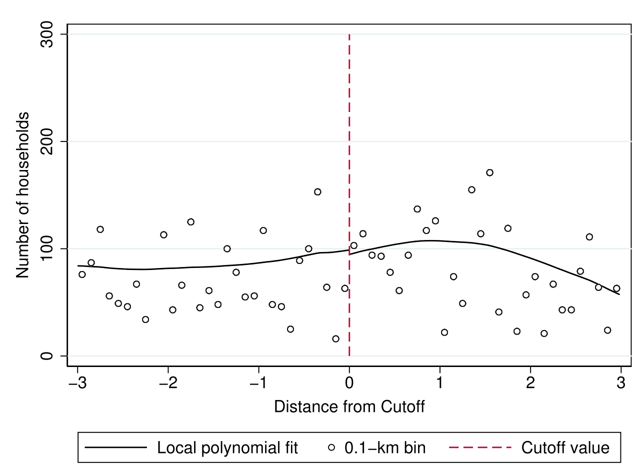
where, Y_{ij} is the outcome of interest for household i in village j. As discussed above, the running variable z_j is the distance of the village from the cutoff point. T_j is a dummy variable that equals one if the household resides in a village below the cutoff point ($z_j \leq 0$) and zero otherwise. We also include a flexible control function of the running variable $f\left(z_j\right)$, that is allowed to differ on either side of the cutoff. The coefficient of interest, β captures the effect of being eligible for the agriculture extension program on the outcome - the "intent-to-treat" (ITT) effect. The analysis includes any spillovers within the village.

Correctly modelling the control function is one of the main issues in RD design. Our primary approach is the non-parametric one suggested by Hahn et al. (2001) and Porter (2003). We use local linear regressions to estimate the left and right limits of the discontinuity at the cutoff of 6 km, and the difference between the two limits indicates the effect of being eligible for the agricultural extension program on the outcome. Thus, the ITT effect is non-parametrically identified as:

$$\beta = lim_{z\uparrow 0}E[Y \mid z_i = z] - lim_{z\downarrow 0}E[Y \mid z_i = z] \tag{3}$$

The choice of the bandwidth can also play an important role in non-parametric estimations. The method suggested by Imbens and Kalyanaraman (2012) gives us an optimal bandwidth of 2.01 km for the primary outcome of interest (household malaria prevalence), whereas the alternative method suggested by Calonico et al. (2014) gives a similar optimal bandwidth of 1.99 km. Further, the optimal bandwidth may vary with the outcome variable and the sample size. As this may lead to unnecessary confusion, we fix our bandwidth to 2 km and then check the sensitivity of our results for the alternative bandwidths of 1.5 km and 3 km. We use a triangular kernel in order to give higher weights to points nearer to the threshold and compute standard errors that are robust to within-cluster correlation at village level (Imbens and Lemieux, 2008; Calonico et al., 2017).

While our primary estimates are based on non-parametric estimations, we also check the sensitivity of our results by modelling the con-



Notes: The figure presents the relationship between the number of households and the distance of the village from the eligibility threshold. The scatter dots represent means of bins with a bandwidth of 0.1 km. The solid lines are local polynomial fits, estimated separately on each side of the cutoff.

Fig. 1. Household density and program eligibility.

Table 2
Discontinuity in controls.

Panel A: Pre-intervention discontinuity using BRAC survey data			
	HH age (1)	HH literacy (2)	Committee Member (3)
Below threshold	0.152	−0.002	0.012
	(0.392)	(0.018)	(0.03)
Control mean	43.86	0.70	0.34
Number of households	3171	3163	3199
Number of villages	173	173	173

Panel B: Pre-intervention discontinuity using UNHS (2005–06)					
	Log agricultural value per capita (1)	HH level malaria prevalence (2)	HH owns at least one bednet = 1 (3)	Log value of bednets per capita (4)	Everyone owns at least 1 pair of shoes = 1 (5)
Below threshold	−0.061	0.045	−0.041	−0.159	−0.032
	(0.209)	(0.031)	(0.071)	(0.549)	(0.131)
Control mean	10.79	0.19	0.24	1.70	0.51
Number of households	564	698	697	697	695
Number of villages	71	71	71	71	71

Notes: The table reports non-parametric reduced form estimates using the bandwidth of 2 km. In Panel B, the reference period for household level malaria prevalence in the UNHS (2005–06) is 30 days preceding the survey. Standard errors calculated clustered at the village level are reported in parentheses. * significant at 10%,** significant at 5%,*** significant at 1%.

trol function as global second-order polynomials.[9] More specifically, we run the following reduced form regressions that allow quadratic trends to differ on either side of the threshold:

$$Y_{ij} = \alpha + \beta T_j + \lambda_1 z_j + \lambda_2 z_j^2 + \lambda_3 T_j z_j + \lambda_4 T_j z_j^2 + \epsilon_{ij} \tag{4}$$

where, once again β captures the effect of eligibility for the program and the error terms are clustered at the village level.

We do not use a "fuzzy" RD design to estimate the local average treatment effects of the program as we have limited information on actual program participation. While the program was launched in 2008, from our survey we can only identify if a household received BRAC's extension services in the six months preceding the survey in 2011 - and not the whole program period. The program was implemented more intensively at the start and therefore, program activities in the six months proceeding the survey would only provide a noisy (and an underestimated) measure of actual program participation. Therefore, throughout the paper we estimate ITT effects using a "sharp" RD design.

Before proceeding to the analysis, we assess the validity of the RD design in the following ways. To begin with, we check for manipulation of the running variable at the point of discontinuity. Fig. 1 shows the number of households in each 0.1 km bin plotted against the distance of the village to the cutoff. If the households selectively moved in order to be eligible for the extension services program then it would lead to bunching just below the threshold. Fig. 1 indicates the absence of such manipulation. More formally, using the McCrary (2008) density test we are unable to reject the null hypothesis of no discontinuity in the density of households at the cutoff.

Next, using both pre-treatment and post-treatment data, we examine if households on either side of the threshold are similar on a variety of characteristics. First, using the BRAC survey we check for discontinuities in covariates such as age and literacy of the household head, and presence of a committee member in the household vary smoothly around the cutoff. In Fig. A2 in the online Appendix, we plot the mean of these covariates in each 0.1 km bin against the distance to the cutoff. Also plotted is the local polynomial fit on each side of the discontinuity separately. A visual inspection does not indicate any substantial discontinuity at the cutoff point. A standard way to formally test for smoothness at the cutoff is to perform 'placebo' tests by estimating the reduced form using the covariates as the outcome variables. As the results presented in Panel A of Table 2 show, we do not observe any discontinuity in these covariates.

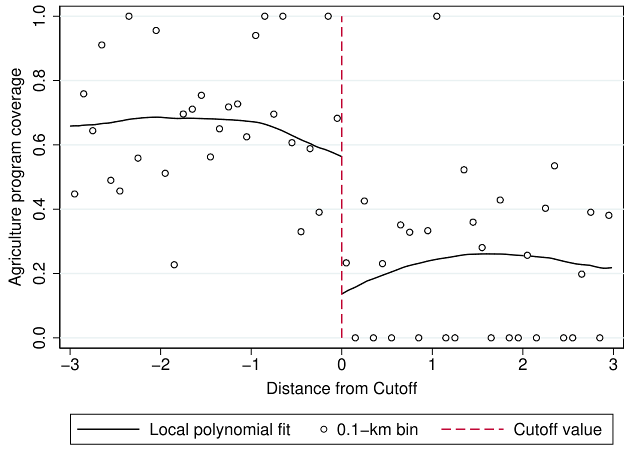
Second, while BRAC did not conduct a baseline survey, we use data from the Uganda National Household Survey 2005–06 (UNHS) to provide further evidence that households on either side of the threshold were similar before the implementation of the BRAC program. Combining GPS coordinates collected under UNHS 2005–06 and those of BRAC branches, we determine distance of households to the nearest BRAC branch and conduct the same 'placebo' tests using a bandwidth of 2 km around the cutoff. These results are presented in Panel B of Table 2. Although the sample is smaller, we find credible evidence that households on either side of the threshold were similar on a variety of key indicators before the introduction of the program. Households do not differ in terms of log value of agricultural production per capita (proxy for household income) and household level malaria prevalence (proportion of household members reporting malaria during 30 days preceding the survey). Households also do not differ significantly in terms of their investments in preventive healthcare in the pre-intervention period, as proxied by the probability that the household owns at least one bednet, the log of value of bednets owned per captia, and a dummy variable that takes the value one if all members of the household own at least one pair of shoes.

Throughout the paper we check if the results are robust to the inclusion of covariates from the BRAC survey. In addition to age and literacy of the household head, and presence of a committee member in the household mentioned above, we also include the household distance to BRAC branch office (as a proxy for market access). We also control for the month of survey as it can affect the prevalence of malaria, and for the gender of the respondent to account for the possibility that women may be more knowledgeable about malaria episodes and bednet usage in the family.[10],[11]

[9] Both the bin regressions method and the Akaike information criteria suggested by Lee and Lemieux (2010) indicate that the optimal order of the polynomial is two. Recent research also recommends limiting RD analysis to quadratic polynomials (Gelman and Imbens, 2018).

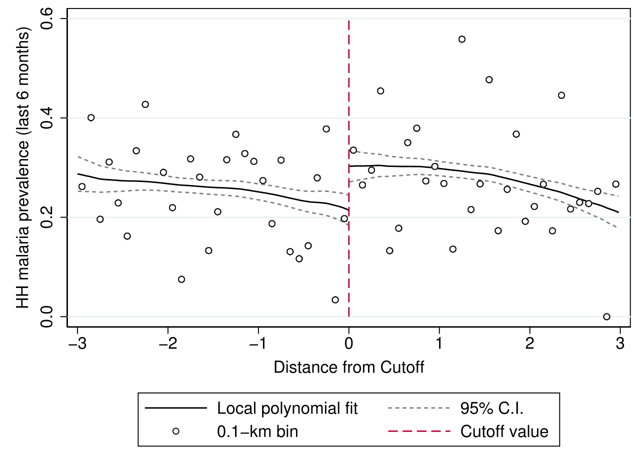
[10] Survey respondents were asked to recall malaria episodes for the last six months and given the typical rainy seasons of March–May and September–December, this implies that in all the survey months (July–December) the malaria recall period overlaped equally with three rainy months. 92% of the survey respondents were males.

[11] The sample size falls with the inclusion of controls. For robustness we check if the estimates are similar when estimating treatment effects without covariates with the sample restricted to households with information on all controls. The results are reported in Tables A5-A9 in the online Appendix.



Notes: The figure presents the relationship between access to the agricultural program in the 6 months preceding the BRAC survey and the distance of the village from the eligibility threshold. The scatter dots represent means of bins with a bandwidth of 0.1 km. The solid lines are local polynomial fits, estimated separately on each side of the cutoff.

Fig. 2. Discontinuity in program activities 6 months preceding the survey.



Notes: The figure presents the relationship between household malaria prevalence in the 6 months preceding the BRAC survey and the distance of the village from the eligibility threshold. The scatter dots represent means of bins with a bandwidth of 0.1 km. The solid lines are local polynomial fits, estimated separately on each side of the cutoff. The dashed lines represent 95% confidence intervals.

Fig. 3. Impact of program eligibility on household malaria prevalence.

We are particularly interested in the effects of the agricultural intervention on the prevalence of malaria for young children and pregnant women, the most vulnerable groups. But it is possible that access to the agricultural extension program could have led to an endogenous change in the fertility decisions of households. However, we do not think this is a concern as we do not find a discontinuity in the probability that at least one member of the household was pregnant at the time of the survey ($\beta = 0.017$, s.e. $= 0.01$). Another assumption for identification is the absence of selective attrition due to death. In our data we find that less than 3% of households reported a death in the six months preceding the survey and all the results on malaria prevalence that follow continue to hold if we exclude households that reported a death.[12] Lastly, while the survey targeted 25 households per village, the average village survey non-participation rate is 30%. However, within our preferred bandwidth of 2 km we do not find the village level survey non-participation rate to be correlated with access to treatment (24% in treatment vs. 26% in control, $p - value = 0.74$), mitigating concerns that survey non-response is biasing the results.

4. Results

We begin with providing evidence of the discontinuity in actual participation in BRAC's agricultural extension program. We then go on to present the primary results on prevalence of malaria at the household level before exploring heterogeneity in the treatment effects by different age groups and gender.

4.1. Discontinuity in actual program participation

First we address the question whether the probability of coverage under BRAC's agricultural extension program was discontinuous at the 6 km cutoff. For this purpose we define a program activity indicator takes the value of 1 if at least one household in the village reports receiving BRAC's agricultural extension program (either training from a model farmer or purchased seeds from a CAP), and 0 otherwise. Program activity is defined at the village level for two reasons: (i) "model farmers" and "CAP" provided extension services to peer farmers residing in their villages; and (ii), as discussed earlier, from our survey we can only identify if a household received BRAC's extension services in the six months preceding the survey, i.e, we do not know if a household was *ever* treated.

The discontinuity in program activities in the six months preceding the survey is graphically shown in Fig. 2 where we plot the proportion of households that receive treatment in each 0.1 km bin against the distance of the village from the cutoff. The figure indicates a clear discontinuity in the coverage of the program - a jump in probability of approximately 40 percentage points at the threshold - indicating that the program had indeed been implemented with a spatial discontinuity.[13]

4.2. Effect on malaria

We begin with a discussion of the results on the prevalence of malaria at the household level. Fig. 3 illustrates the impact of residing within the radius of 6 km from a BRAC branch on prevalence of malaria at the household level, measured as the proportion of members who reported being infected by malaria in the previous six months. This is depicted on the y-axis, while the x-axis measures the distance of the village from the cutoff. Also plotted is the local polynomial fit, estimated separately on each side of the discontinuity. The figure shows a clear jump at the cutoff, indicating that households residing in villages just below the cutoff distance (eligible for the extension program) have a lower proportion of members infected by malaria in the last six months.

Table 3 presents results from the formal evaluation of the agricultural extension program on malaria around the cutoff. Column 1 reports the results from estimating the reduced form relationship between being eligible for the program and the household level prevalence of malaria without controlling for any other covariates. The coefficient in column 1 indicates that, under the preferred bandwidth of 2 km, in households in eligible villages the proportion of members infected by

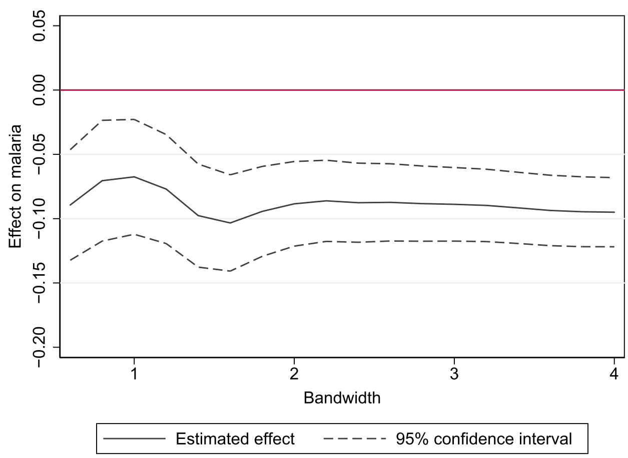
[12] The results are available from the authors.

[13] Formal tests for discontinuity in the availability of the program using local linear regressions are provided in Table A1 in the online Appendix. The results show that households in villages less than 6 km away from a BRAC branch are 43 percentage points more likely to be covered under the agricultural extension program than those in residing in villages further away. This result is also robust to the inclusion of controls discussed in Section 3.2 and changes in bandwidth.

Table 3
Effect on malaria prevalence at household level.

	Malaria prevalence					
	Bandwidth = 2		Bandwidth = 1.5		Bandwidth = 3	
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible	−0.089***	−0.099***	−0.105***	−0.117***	−0.089***	−0.099***
	(0.017)	(0.016)	(0.02)	(0.018)	(0.015)	(0.014)
Covariates	No	Yes	No	Yes	No	Yes
Control mean	0.302	0.305	0.292	0.295	0.286	0.288
Number of households	3240	3085	2489	2363	4475	4272
Number of villages	173	173	131	131	245	245

Notes: This table shows the effect of eligibility for the agricultural extension program on household level malaria prevalence using the non-parametric method described in the text. Controls included are age and literacy of the household head, presence of a committee member in the household, gender of respondent, month of survey, and household distance. Standard errors calculated clustered at the village level are reported in parentheses. * significant at 10%,** significant at 5%,*** significant at 1%.



Notes: The figure presents the intent-to-treat (ITT) effects for bandwidths at every increment of 0.2 km from 0.6 km to 4 km. The dashed lines represent 95% confidence intervals for these bandwidths.

Fig. 4. Effects on household malaria prevalence, by varying bandwidths.

malaria is 8.9 percentage points lower than that for households in ineligible villages. Given a control mean of 0.302, this translates into a 29% reduction.[14]

We perform a variety of tests to check the robustness of this result. First, we check if the baseline results are sensitive to the inclusion of controls discussed in Section 3.2. This check is presented in column 2 of Table 3 and we find that this does not alter the magnitude or significance of the ITT estimates. Second, we show that the result is robust to using alternative bandwidths of 1.5 km and 3 km (columns 3–6 of Table 3). In a finer check of the sensitivity of our result to the choice of bandwidth, we estimate the ITT effects for bandwidths at every increment of 0.2 km from 0.6 km to 4 km. In Fig. 4 we present the estimated effect of the extension services program on malaria prevalence and the 95 percent confidence intervals for these bandwidths. As one would expect, the precision of the estimate increases with bandwidth. Overall, the figure clearly indicates that our primary finding is not sensitive to the choice of bandwidth.

As stated earlier in Section 3.2, parametric regressions may be viewed as a further robustness check to the non-parametric results presented here. We estimate the reduced form using a quadratic polynomial specification where the coefficients of the polynomials are allowed to differ on either side of the cutoff (Eq. (4)). The results are presented in column 1 of Table A2 in the online Appendix and are similar to those obtained for the non-parametric method.

It should be noted that the ITT estimates of the effects on malaria prevalence are a lower bound due to potential spillovers. A reduction in the density of malaria vectors (possibly due to the use of bednets as we discuss later in Section 5.1) can generate positive externalities for residents in the same village and neighboring villages (Hawley et al., 2003; Killeen et al., 2007; Tarozzi et al., 2014). This implies that malaria prevalence in control villages may be lower due to positive spillovers from treatment villages near the threshold. By narrowing the gap in malaria prevalence between treatment and control groups, this could lead to an underestimate of the overall effect of the program.

4.3. Heterogeneity

We now undertake an individual level analysis to investigate the heterogeneity in the effects across different age groups and gender. We particularly care about the effects on children and working age adults because of the implications for current and future labor productivity. A substantial amount of literature finds that exposure to health shocks during pregnancy and early life can adversely affect long-term health and economic wellbeing (Almond and Currie, 2011). In the case of malaria, exposure to anemia - a typical manifestation - when child is in-utero and early-life, reduces the availability of oxygen and nutrients, thereby hampering the development of organs and cognitive capacity. Further, childhood morbidity due to malaria can, in turn increase vulnerability to other diseases. Barreca (2010) estimates that a standard deviation increase in exposure to *in utero* and postnatal malaria reduced educational attainment by 0.23 years. Similarly, a number of studies use exogenous variation in the introduction of malaria eradication programs to identify adverse effects of early life exposure to malaria on future educational attainment and earning capacity (Cutler et al., 2010; Bleakley, 2010; Lucas, 2010; Venkataramani, 2012; Barofsky et al., 2015; Kuecken et al., 2017).

We first focus on children aged 5 or less, who are considered to be the most vulnerable group in terms of exposure to the disease (older cohorts acquire immunity from repeated exposure). As the summary statistics reported in Table 1 show, we find that about 50% of children aged 5 or below are reported to have experienced malaria in the previous six months, which is higher than the infection rates for other age groups. The other age groups we consider are children aged 6–19 and the working-age adults in the age group of 20–60.

The first column in Panel A of Table 4 reports the effects on children aged 5 or less. We find that children residing in villages eligible for the agricultural extension program are 11.2 percentage points (or 22%) less likely to report having experienced malaria in the preceding six months.

[14] Alternatively, one could measure the prevalence of malaria at the household level with a dummy variable that takes the value of 1 if at least one member of the household reports being infected by malaria and 0 otherwise. Our result is robust to this measure: the ITT estimate (s.e.) is −0.111 (0.029).

Table 4
Effects on malaria: Individual level.

	By age groups			By Gender		
	Ages 0-5 (1)	Ages 6-19 (2)	Ages 20-60 (3)	Males (4)	Females (5)	Pregnant (6)
Panel A: Without covariates						
Eligible	−0.112***	−0.058**	−0.062***	−0.084***	−0.074**	−0.225***
	(0.041)	(0.024)	(0.023)	(0.024)	(0.033)	(0.067)
Control mean	0.501	0.381	0.319	0.362	0.387	0.396
Observations	2319	7090	6329	7810	8276	366
Number of villages	171	172	173	172	173	124
Panel B: With covariates						
Eligible	−0.129***	−0.067*	−0.074**	−0.093***	−0.083***	−0.191***
	(0.043)	(0.037)	(0.031)	(0.024)	(0.032)	(0.072)
Control mean	0.502	0.380	0.316	0.360	0.387	0.398
Observations	2224	6782	6100	7544	8017	356
Number of villages	171	172	173	172	173	123

Notes: The table reports non-parametric estimates of the impact of program eligibility using the bandwidth of 2 km. Controls included are age and literacy of the household head, presence of a committee member in the household, gender of respondent, month of survey, and household distance. Standard errors calculated clustered at the village level are reported in parentheses. * significant at 10%,** significant at 5%,*** significant at 1%.

Columns 2 and 3 of Panel A of Table 4 show that there are significant reductions for individuals in the age groups of 6–19 and 20–60 as well. While the point estimates for these groups are smaller, we find that the effects are not statistically different across the age groups.

On analyzing the data for males and females separately, we find significant reductions in malaria for both groups. These results are presented in columns 4 and 5 of Panel A of Table 4. The point estimates are similar and not statistically different from each other ($p - value = 0.81$), indicating minimal difference by gender. We also examine the impact of the agricultural extension program on the malaria rates for pregnant women. While the sample size is smaller, the results presented in the last column in Panel A of Table 4 show large, significant reductions in the prevalence of malaria for pregnant women. These results are robust to the inclusion of controls (Panel B of Table 4); varying the bandwidth (Table A3 in the online Appendix); and parametric estimation (columns 2–7 of Table A2 in the online Appendix). In light of the decline in mortality and morbidity associated with reduction *in utero* and postnatal exposure to malaria discussed earlier, our results imply that the agricultural extension services program could translate into significant benefits in terms of saving lives and boosting health and incomes in the long-run.

Lastly, it is possible that there are gender differences within each age group as resource-constrained households often make a distinction between males and females when allocating resources. For example, in Uganda, Björkman-Nyqvist (2013) finds that negative income shocks (proxied by rainfall shocks) had an adverse effect on girls' school enrollment but not on that of boys. We examine this possibility by estimating the effects for males and females separately within each age group. In results reported in Table A4 of the online Appendix, we find that in the 0–5 year age group, females experienced a significant decline in malaria prevalence while males did not. However, we are unable to reject the null of equality of coefficients between males and females ($p - value = 0.37$). It is possible that these effects are imprecisely estimated due to modest sample sizes. Similarly, there are no significant gender differences in the age groups of 6–19 and 20–60.

5. Mechanisms

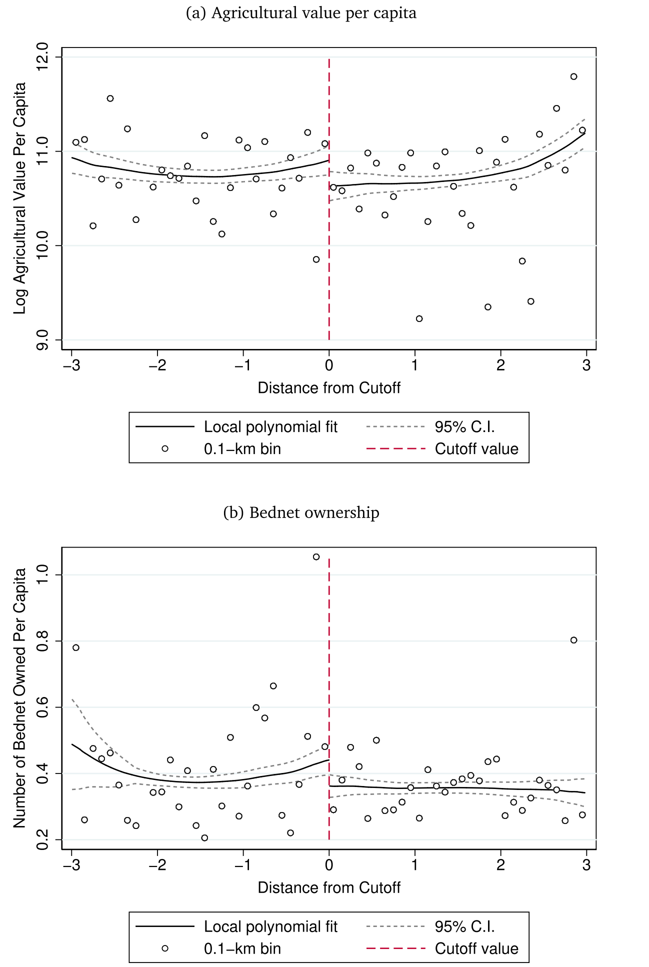
5.1. Income

As discussed in Section 1, a number of papers have pointed to the possibility that improvements in the income generating capacity of agriculture can reduce malaria. Recall that using the UNHS 2005–06 pre-treatment data we did not find any difference in the value of agricultural production (proxy for household income) between treated and control households at the threshold (Section 3.2 and column 1 of Panel B in Table 2). However, using the BRAC data and the same RD design, we find that the agricultural extension program increased value of agricultural production, as illustrated in Fig. 5a.[15] More formally, results presented in Table 5 show that three years after the start of the intervention, the value of agricultural production per capita was approximately 27.6% higher in villages with access to the program (columns 1–2 of Table 5). While the effects are large, it must be noted that these are measured three years after the start of the intervention and that the primary beneficiaries were marginalized female farmers (who generally have lesser income to begin with). This improved economic status of the household could have reduced the prevalence of malaria in two important ways - (i) by increasing the capacity to buy bednets and; (ii) by improving nutritional status and immunity. We explore these channels in turn below.

First, the prevalence of malaria could decrease if the increased income resulting from the intervention was invested in malaria prevention technologies such as bednets. Previous surveys suggest that household income plays a role in constraining access to bednets. As discussed earlier in Section 2.1, while the NMCP does support the distribution of some free and subsidized bednets, the most common way of obtaining a bednet is through the open market. The positive association between income and bednet ownership is borne out in several surveys. For example, the Uganda DHS, 2011 finds that while 67.2% of households in the lowest wealth quintile owned at least one bednet, 84.2% did so in the highest wealth quintile (UBOS and ICF, 2012). BRAC's survey collected information on the ownership of bednets at the household level and whether each usual member of the household had slept under a bednet the night previous to the survey.[16] Note that while we have information regarding ownership at the household level, we have information only on usage at the individual level. Using this information and the same estimation strategy we investigate the plausibility of this channel.

[15] Value of agricultural production is a suitable proxy for household income as a majority of household report agriculture to be the main source of income. Engagement in formal wage employment is low (approximately 5% in the sample) and does not differ significantly at the cutoff ($\beta = 0.016$, s.e. $= 0.01$).

[16] Bednets can be broadly classified into three types: untreated bednet, insecticide-treated bednets (ITNs) and Long-lasting insecticidal bednets (LLINs). Our data do not distinguish the type of bednet used.



Notes: The figure presents the relationship between the distance of the village from the eligibility threshold and (a) household's value of agricultural production per capita, and (b) household's bednet ownership per capita. The scatter dots represent means of bins with a bandwidth of 0.1 km. The solid lines are local polynomial fits, estimated separately on each side of the cutoff. The dashed lines represent 95% confidence intervals.

Fig. 5. Impact of program eligibility on agricultural value per capita and bednet ownership per capita.

The ITT estimates are shown in columns 3–4 of Table 5 for the preferred bandwidth of 2 km. At the household level we find that bednets owned per capita are higher by 0.08 in households residing in villages eligible for the extension program, which given a control mean of 0.363 translates into a 22% increase. The corresponding Fig. 5b shows the effect graphically. This strongly indicates that low income is a barrier to investment in preventive health technologies.[17]

We now turn to the use of bednets at individual level in Table 6. The results show that children and adolescents (age groups 0–5 and 6–19) in villages eligible for the extension program are 14.4 and 9.7 percentage points more likely to sleep under bednets, respectively (Panel A, columns 1 and 2). This is in line with the sharp reduction in the prevalence of malaria found for these age groups in Section 4.3. The positive effect on the use of bednets is smaller and not statistically significant for adults in the age group of 20–60. Furthermore, on comparing the effects across age groups we find that the increase in bednet usage among children aged 0–5 is significantly greater than the effects on adults ($p - value = 0.01$). Columns 4 and 5 of Panel A show that the increase in the use of bednets for males and females is similar (7 and 5.9 percentage points, respectively) and not statistically different from each other ($p - value = 0.79$). Finally, we do not find any significant effects on the use of bednets by pregnant women (column 6). Panel B of Table 6 shows that these results are robust to the inclusion of controls discussed in Section 3.2.

These results are important as they shed light on the intra-household allocation of health resources when households are faced with financial constraints. Previous studies find that in Ugandan households with limited income, children - the more biologically vulnerable group - may not receive priority in the allocation of bednets (Lam et al., 2014; Hoffmann, 2009). Our results are consistent with this finding as they indicate that an easing of the income constraint led to greater usage of bednets, and a corresponding decline in malaria, especially among children.

While an increase in the use of bednets could explain the decline in malaria prevalence for those under the age of 20, it does not explain the significant decline noted for pregnant women. Our hypothesis is that pregnant women in villages below the threshold benefited from positive intra-household spillovers (Killeen et al., 2007). In particular pregnant women are more likely to spend time in the house with young children and, given the noted decline in malaria prevalence for children, they would therefore be less exposed to infectious mosquito bites.[18] We find suggestive evidence in favor of this hypothesis. On splitting the sample of pregnant women into those residing in households with no children aged 5 or below and those residing in households with at least one child aged 5 or below, we find that there is a significant reduction in the prevalence of malaria for pregnant women only in the latter case (Table 7). Further giving credence to this finding, we can also rule out the role of intermittent preventive treatment during pregnancy (IPTp). Supported by the NMCP, IPTp is provided through antenatal clinics (ANC), involves taking at least two doses of sulfadoxine-pyrimethamine (SP/Fansidar) during pregnancy, and has been shown to reduce maternal malaria episodes, severe maternal anaemia, and low birth weight (see WHO, 2015).[19] We find that women who were pregnant during the survey or gave birth in the year preceding the survey were *less* likely to have visited an ANC during pregnancy ($\beta = -0.076$, s.e. = 0.034).

Second, improved nutritional status of household members could reduce the prevalence of malaria through a reduction of infections and faster recovery. The potential pathways are deficiencies in micronutrients such as zinc and vitamin A that reduce the ability of the immune system (Shankar, 2000). While we do not have information on nutritional intake, Pan et al. (2018) find that coverage under the BRAC program not only increased per capita consumption but also increased the variety of food consumed and reduced scarcity of food (especially in the pre-harvest periods).

It is also possible that an increase in household income resulted in the use of other preventive technologies such as indoor residual spraying (IRS). While we are unable to observe the use of IRS, as discussed in Section 2.1, only around 7% of the households in the Uganda DHS reported using IRS in 2011. Further, bednets continue to be the dominant technology promoted by the Government of Uganda, NGOs and other international organizations. Similarly, we are also unable to observe modifications made to the dwelling - closing openings in ceilings, doors, windows and eaves with screens or other methods - in order to reduce the indoor density of mosquitoes.

[17] Congruently, this result also indicates that with an increase in the opportunity cost of falling sick (due to the income effect of the agricultural intervention), households are more willing to buy bednets in order to avoid being sick. We discuss this further in Section 5.2.

[18] We thank Pascaline Dupas for pointing this out.

[19] According to the Uganda DHS in 2011, 27% of women reported using IPTp during their last pregnancy (UBOS and ICF, 2012).

Table 5
Effects on household agricultural income and bednet ownership.

	Log agricultural value per capita		Bednet ownership per capita	
	(1)	(2)	(3)	(4)
Eligible	0.276***	0.29***	0.079***	0.052*
	(0.097)	(0.10)	(0.029)	(0.030)
Covariates	No	Yes	No	Yes
Control mean	10.65	10.66	0.363	0.364
Number of households	2775	2655	3186	3051
Number of villages	173	173	173	173

Notes: The table reports non-parametric estimates of the impact of program eligibility using the bandwidth of 2 km. Controls included are age and literacy of the household head, presence of a committee member in the household, gender of respondent, month of survey, and household distance. Standard errors calculated clustered at the village level are reported in parentheses. * significant at 10%,** significant at 5%,*** significant at 1%.

Table 6
Effects on bednet usage.

	By age groups			By Gender		
	Ages 0-5	Ages 6-19	Ages 20-60	Males	Females	Pregnant
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Without covariates						
Eligible	0.144***	0.097***	0.032	0.070**	0.059**	−0.027
	(0.032)	(0.034)	(0.03)	(0.033)	(0.03)	(0.082)
Control mean	0.595	0.444	0.549	0.505	0.517	0.631
Observations	2333	7127	6366	7857	8319	366
Number of villages	171	172	173	172	173	124
Panel B: With covariates						
Eligible	0.129***	0.083**	0.001	0.054*	0.045	−0.058
	(0.029)	(0.033)	(0.03)	(0.031)	(0.029)	(0.092)
Control mean	0.597	0.444	0.548	0.503	0.514	0.625
Observations	2236	6815	6136	7588	8056	356
Number of villages	171	172	173	172	173	123

Notes: The table reports non-parametric estimates of the impact of program eligibility using the bandwidth of 2 km. Controls included are age and literacy of the household head, presence of a committee member in the household, gender of respondent, month of survey, and household distance. Standard errors calculated clustered at the village level are reported in parentheses. * significant at 10%,** significant at 5%,*** significant at 1%.

Table 7
Effects on malaria: Intra-household spillovers on pregnant women.

	No child aged ≤ 5 in HH		At least 1 Child aged ≤ 5 in HH	
	(1)	(2)	(3)	(4)
Eligible	0.063	0.117	−0.393***	−0.325***
	(0.116)	(0.111)	(0.098)	(0.099)
Covariates	No	Yes	No	Yes
Control Mean	0.293	0.278	0.449	0.458
Observations	135	131	231	225
Number of villages	72	70	110	109

Notes: The table reports non-parametric estimates of the impact of program eligibility on malaria incidence among pregnant women using the bandwidth of 2 km. Controls included are age and literacy of the household head, presence of a committee member in the household, gender of respondent, month of survey, and household distance. Standard errors calculated clustered at the village level are reported in parentheses. * significant at 10%,** significant at 5%,*** significant at 1%.

Finally, in order to further underscore the role of income in malaria reduction we use the UNHS 2005-06 data to check heterogeneity based on pre-treatment income levels. We combined GPS coordinates of BRAC branches and those of households surveyed under UNHS 2005–06 to determine the average value of agricultural production in the neighborhood of each BRAC branch.[20] Sorting branches by the value of agricultural production per capita, we then classified those above median as "richer branches" and those below as "poorer branches". Table 8 reports the effects on household level malaria prevalence and bednet ownership based on this classification of pre-treatment branch income level. We find that the effects on malaria reduction and increase in bednets is driven by program effects in poorer branches, further highlighting the importance of income in malaria reduction.

[20] In order to compute average income level near a BRAC branch we use households in a 20 km radius.

Table 8
Effects on malaria and bednets: differences by pre-treatment income level.

	HH level malaria prevalence		Bednet ownership per capita	
	Poorer branches (1)	Richer branches (2)	Poorer branches (3)	Richer branches (4)
Eligible	−0.152***	−0.012	0.163***	0.002
	(0.012)	(0.03)	(0.021)	(0.047)
Control Mean	0.407	0.193	0.358	0.369
Number of households	1501	1739	1479	1707
Number of villages	72	101	72	101

Notes: The table reports non-parametric estimates of the impact of program eligibility on household level malaria prevalence and bednet ownership using the bandwidth of 2 km. Branches are classified as rich/poor based on value of agricultural production per capita using the UNHS 2005-06 data. Standard errors calculated clustered at the village level are reported in parentheses. * significant at 10%,** significant at 5%,*** significant at 1%.

5.2. Other possible channels

We have argued that BRAC's agricultural extension program in Uganda led to a decline in the prevalence of malaria, likely through an increase in income and the capacity to buy bednets. In this section, we discuss a variety of alternative pathways and explanations.

The most plausible alternative channel explaining our results could be that households situated closer to BRAC branch offices have better access to health infrastructure. While this is indeed a possibility, there is no reason to expect a discontinuity in the access to health facilities at the arbitrary cutoff of 6 km. Nonetheless, we address this concern in a variety of ways. Individuals who reported being sick in the 30 days preceding the BRAC survey and sought treatment, were asked the distance of the health facility where treatment was sought. Using the same RD design we do not find the distance to the health facility to differ at the cutoff ($\beta = 1.02$, s.e. = 1.27). Still, as a part of our robustness checks we included the household distance to BRAC branch office (as a proxy for market access) as one of the control variables throughout the analysis.

BRAC also operated a primary health care program in which it trained local community health promoters to conduct home visits, provide basic medical advice and diagnoses, pre-natal and post-natal care to pregnant women, and sell preventive and curative health products. The community health promoters program was run separately and there was no institutionally mandated discontinuity in the implementation of this program. The program was rolled out in a randomized manner across some clusters, but not for the country as a whole (see Björkman-Nyqvist et al., 2018 for a randomized impact evaluation). Nonetheless, we define village-level coverage under BRAC's health program by a dummy variable that takes the value of 1 if at least one household in the village reports the availability or utilization of the services of a BRAC health promoter, and test if this program could have affected our results in the following ways. First, while there was no mandated discontinuity in the health program, the actual implementation of the program might have been discontinuous at the cutoff of 6 km. Using the same RD design we find that access to BRAC's health program was not discontinuous at the threshold ($\beta = 0.034$, s.e. = 0.049).[21] Second, we find that controlling for access to the health program does not change the estimated effect on household malaria prevalence ($\beta = -0.096$, s.e. = 0.015). Third, while BRAC's health program was not discontinuous at the 6 km cutoff, there might be interaction effects between the health program and the agricultural program in the sense that the agricultural intervention had a differential impact on malaria in the presence of the health program. In order to check for this we included an interaction of the indicator variable for the health program and T_j in Equation (4). We find the coefficient on the interaction term to be statistically insignificant indicating that access to the health program did not differentially affect malaria prevalence ($\beta = 0.047$, s.e. = 0.045). Admittedly, while these checks suggests that the health program may not be driving the effects on malaria, access to the program may have varied in unobserved dimensions (such as intensity of activities) and we can not rule it out with certainty.

A related concern could be that the treatment villages had better access to bednets through various government programs. We can rule this out in the case of two key programs that were in place during the period of this study. The NMCP has long pursued free distribution of bednets to pregnant women during ANC visits. However, as we discussed in Section 5.1, we find that women in treatment villages who were pregnant at the time of the survey or gave birth in the year preceding the survey were less likely to have visited an ANC. Further, in order to boost bednet coverage in the country the NMCP also started a mass LLIN distribution campaign in 2010, beginning with households with a pregnant woman or child under five in 13 districts in the Central region of Uganda (Wanzira et al., 2014). Targeting was imperfect as substantial number of ineligible households received the campaign bednets as well. We drop all households residing in the overlapping districts in the Central region and find that the effects on household level malaria prevalence continue to hold ($\beta = -0.08$, s.e. = 0.013).

Overall, while we do not find the supply of healthcare to be discontinuous at the threshold, it is possible that the demand for healthcare increased. For example, by increasing the economic value of labor, the agricultural program could have changed household preferences such that households become more willing to invest in preventive healthcare (irrespective of a relaxation of the household budget constraint). Households might have been more willing to purchase bednets, anti-malarial drugs, or seek medical advice that reduced the frequency and/or the length of malaria episodes. While we do not have information on the demand for preventive healthcare, we find that demand for curative care increased - conditional of being sick, the probability of visiting a doctor is higher in the treatment villages ($\beta = 0.026$, s.e. = 0.01).

Lack of information on malaria transmission and its effects may dampen investment in malaria prevention. The agricultural extension program studied here did not include any health component. Still, it is possible that by facilitating more social interaction, the agricultural extension program resulted in the exchange of some health related information as well. The BRAC questionnaire asked respondents if they were related to or friends with other surveyed respondents in the village, providing partial information on the social network of the household. On restricting our sample to households with at least a friend or a relative covered in the survey, we find that the likelihood of turning to their social network for health related information does not change significantly at the cutoff ($\beta = -0.063$, s.e. = 0.051). Note that this only partially rules out the information channel as playing a role in the reduction of malaria. For example, it is possible that the respondents sought health related information from BRAC's extension services

[21] Fig. A2d in the online Appendix shows this graphically. Alternatively, we could define village-level coverage as the proportion of households in a village that report access to BRAC's health program. We do not find this measure to differ at the threshold as well (β = 0.006, s.e. = 0.021).

Table 9
Effects on the probability of being sick during the last 30 days.

	Ages 0-5 (1)	Ages 6-19 (2)	Ages 20-60 (3)
Panel A: Pooled sample			
Eligible	0.011	0.016	−0.001
	(0.043)	(0.021)	(0.017)
Control mean	0.265	0.155	0.145
Observations	2321	7165	6416
Number of villages	171	172	173
Panel B: Recall period overlaps with dry season			
Eligible	−0.019	−0.018	0.005
	(0.061)	(0.027)	(0.022)
Control mean	0.279	0.173	0.153
Observations	1634	5212	4588
Number of villages	129	136	137
Panel C: Recall period overlaps with rainy season			
Eligible	−0.089*	−0.007	−0.067***
	(0.046)	(0.025)	(0.02)
Control mean	0.236	0.104	0.124
Observations	680	1927	1805
Number of villages	59	63	64

Notes: The table reports non-parametric estimates of the impact of program eligibility on the likelihood of being sick during the last 30 days using the bandwidth of 2 km. Panel B includes survey months July–September and Panel C includes survey months October–December. Standard errors calculated clustered at the village level are reported in parentheses. * significant at 10%,** significant at 5%,*** significant at 1%.

agents (who are typically more educated) or parts of their social network that were not surveyed.

The extension services program, by altering agricultural practices, could have also directly affected the prevalence of malaria in two crucial ways - the use of irrigation and pesticides. It is possible that the use of agricultural inputs such as irrigation, particularly in the case of rice production, may be conducive for malaria vectors (Ghebreyesus et al., 1999).[22] Majority of cultivation in Uganda is rain-fed, with only 2.51% of farmers reporting using irrigation in our sample. Nonetheless, an increase in irrigation can result in the occurrence of small stagnant water bodies and an increase in the moisture content of soil. However, Pan et al. (2018) find that the agricultural extension program *increased* the probability that a household used irrigation, leading us to believe that this channel is not driving our results.[23] Similarly, an increase in the use of pesticides could result in the decline of malaria vectors and the prevalence of malaria, but Pan et al. (2018) do not find a significant increase in the use of pesticides.

Finally, it is worth noting that the reduction in malaria is associated with an agricultural extension program that specifically targeted female farmers. Some of the existing literature finds that women are more likely to invest in health than men (see Strauss et al., 2000). Then the increase in the household ownership of bednets may not only be due to an increase in income but also due to a resulting increase in the bargaining power of women. We use the proportion of household consumption expenditure spent on tobacco and alcohol as a proxy for the bargaining power of women (Hoddinott and Haddad, 1995) to check if the program increased the bargaining power of women. Using the same RD design we do not find a reduction in the share of household expenditure on these items at the cutoff point ($\beta = 0.001$, s.e. $= 0.002$). Nonetheless, while this result indicates that an increase in the bargaining power of women may not be primary force driving the reduction in malaria, we do not rule out the possibility that it might have played a role in it.

5.3. Other outcomes

In order to further emphasize the impact of the intervention on the disease environment and welfare, we report effects on related health outcomes and the education of children.

First from the BRAC survey we also have information on whether an individual had been sick in the 30 days preceding the survey. As shown in Panel A of Table 9, we find that access to the program did not have any effect on the likelihood of being sick for any of the three age groups. While this outcome variable is more general than malaria, the lack of effects here are somewhat puzzling, but it is possible that the month of survey plays a role. Individuals are more vulnerable to sickness during the rainy season (diseases such as malaria, dengue, diarrhea, etc. are more common), and it is possible that the intervention had an effect during this time rather than during the dry season. The survey was conducted over the months of July–December 2011. As the rainy seasons in Uganda are March–May and September–December, the recall period for the first half of the survey (survey months July–September) overlapped with the dry season, and during the rest of the survey (survey months October–December), it overlapped with the rainy season. We investigate this possibility by splitting the sample by whether the recall period for the household overlapped with the rainy season or not in Panels B and C of Table 9. In Panel B we find that the program had no effect on illness during the dry season. However, results in Panel C show that the likelihood of illness reduced significantly during the rainy season for children aged 0–5 years and adults aged 20–60 years. These results further substantiate the finding that the intervention positively affected health.

Second, we also checked if the household made other related health investments. Footwear can be considered a health-related product as by limiting skin contact with fecal matter it reduces the chances of intestinal worm infections (hookworm, roundworm). The first column

[22] However, recent medical literature from Africa suggests that even though irrigation increases the density of malaria vectors, this may not necessarily translate into a higher prevalence of malaria - resulting in what is termed as the “paddies paradox.” A possible explanation is that the mosquito An. arabiensis Patton, with lesser malaria carrying capacity, multiplies faster in rice fields and may displace An. Gambiae Giles, the most effective malaria vector (Ijumba and Lindsay, 2001).

[23] Furthermore, observations from the field do not indicate construction of new dams or other major irrigation systems. Unfortunately, we do not observe the method of irrigation in the data.

Table 10
Effects on other related outcomes.

	Everyone owns at least 1 pair of shoes in HH (1)	Children aged 7-13	
		Attending school (2)	Overage (3)
Eligible	0.139***	0.043***	−0.118***
	(0.011)	(0.008)	(0.024)
Control mean	0.768	0.934	0.475
Observations	3187	3797	3650
Number of villages	173	172	172

Notes: The table reports non-parametric estimates of the impact of program eligibility using the bandwidth of 2 km. Standard errors calculated clustered at the village level are reported in parentheses. * significant at 10%,** significant at 5%,*** significant at 1%.

of Table 10, reports that the probability that every household member owns at least one pair of shoes is significantly higher for households residing below the cutoff. Once again this is a striking result given that we do not find households to differ on this measure when using the pre-treatment UNHS 2005-06 data (column 5, Panel B in Table 2).

Lastly, there is now a robust empirical literature that causally links children's health to educational outcomes. As discussed earlier in Section 4.3, studies have found malaria eradication programs in the 20th century to have improved literacy, educational attainment and cognition in the long-run (Lucas, 2010; Venkataramani, 2012; Barreca, 2010). In a similar vein, Miguel and Kremer (2004) find that a school-based deworming program in Kenya increased school attendance. Results in columns 2 and 3 of Table 10 show that the effects of the agricultural extension program correspond to the existing literature. We restrict the sample to children aged 7–13 years old as children normally start primary school at age 6, and 7 years of primary education is compulsory. In column 2 we find that children in eligible villages are 4.3 percentage points more likely to be attending school. Further, in order to capture educational attainment we use an indicator for age-grade distortion, where the outcome variable takes the value 1 if the child is overage relative to her grade.[24] The result in column 3 shows that in villages with access to the program, children are also less likely to be overage for their grade. Taken together, these results suggest that the intervention had a meaningful effect on household welfare.

6. Conclusion

Despite a recent declining trend, malaria continues to be a significant cause of global morbidity and mortality. In this paper, we find that access to an agricultural intervention program in Uganda led to substantial reductions in the prevalence of malaria. The effects were substantial for the most vulnerable groups - children under the age of 5 and pregnant women. As exposure to health shocks in early life can translate into considerable economic losses over time, our results imply that the agricultural extension services program could have substantial long-term benefits that are not accounted for in standard cost-benefit analyses.

One of the explanations, consistent with our data, seems to be that the reduction in malaria was driven by an increase in incomes. Overall, we estimate that agricultural extension program increased household income by 27.5% while it reduced household malaria prevalence by 8.9 percentage points, thereby highlighting the role of financial constraints in limiting investments in malaria control. Moreover, the finding that an easing of the income constraint led to greater bednet use and reduction in malaria among children indicates that the interplay between the lack of income and preferences for intra-household allocation of health resources may have severe implications for the most vulnerable household members. Further research into the preferences for intra-household allocation of health resources other than bednets, such as nutritional intake and preventive medicine consumption could inform policies that seek to promote health status of the most vulnerable members.

From a policy perspective, while there is consensus on the need for subsidization to boost uptake of bednets, there is debate on the amount of subsidization as governments typically face a trade-off between the cost of subsidization and the possibility of excluding the poorest households that lack the cash to even pay reduced prices. Our results indicate that alleviating the income constraint of households through other policy instruments can go some way in improving health outcomes and easing this trade-off.

Finally, while the results are specific to the ecology of the area under study, they do indicate that higher incomes associated with increased agricultural productivity can perhaps improve health outcomes in other similar contexts as well. The relationship between agriculture and health is complex, but continued assessment of the health impacts of agricultural interventions can further our understanding. To that end, we also recommend including questions regarding health and preventive health investments in agricultural survey instruments.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jdeveco.2019.03.006.

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[24] More precisely, we calculate age-grade distortion as AGD = grade-(age-7). The overage indicator takes the value of 1 if AGD < =0. As children usually start school at the age of 6 years, if they are older than 7 after having completed the first year of primary school then they are over-age for their grade level.

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