Program Evaluation of BRAC Uganda's Community Health Sensitization Program: Quantitative analysis of malaria prevention behaviors in a randomized control trial

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Executive Summary[1]

Program Overview

BRAC Uganda provided mentors in 12 adolescent groups with megaphones and short health sensitization announcements as part of a community health sensitization program. BRAC instructed mentors to make daily announcements based on the script in their local language and during the afternoon or evening while walking around their village. All the participating villages are in Eastern Uganda, spread across 6 BRAC administrative units.

Evaluation Methods and Data

In order to facilitate evaluation of the program, BRAC randomized participation, assigning 24 villages to treatment or control status. Two rounds of surveys were conducted to measure program exposure, household characteristics, household knowledge of malaria prevention activities, and household bednet use and ownership. The response rate for the follow-up survey was 86%; much of the attrition was driven by a large building project in one of the treatment villages that displaced a number of survey respondents. While the treatment and control groups were well-balanced, the overall sample seems to be slightly wealthier than most inhabitants of Eastern Uganda. There was some contamination of control village respondents who lived near treatment villages and reported hearing the announcements.

Findings

This study measures the program's effect on a variety of malaria prevention activities. While there was little evidence that the program caused wide-spread changes in household knowledge or practices, there were a few notable outcomes. The BRAC program shifted the distribution of nets owned, with households in treatment villages more likely to own two nets than one net. Though it was not possible to determine how these extra nets were acquired, findings suggest that the households in treatment villages may be slightly more likely to purchase nets than were households in control villages, though findings are not statistically significant. Households in treatment villages also seem more likely to report discussing bednets with their neighbors more often than households in control villages, though these finding are not statistically significant. Despite the changes in net ownership, however, there was no significant effect of the program on actual use of nets, even in high-risk populations. Households in treatment villages also did not show any greater knowledge of malaria prevention methods or use of other malaria prevention methods.

Conclusion

BRAC's health sensitization program had limited success in changing household malaria prevention behaviors, though not the extent that was intended. If BRAC decides to continue with the program, staff should pay special attention to improving implementation through:

1. Greater involvement by ELA program staff at the branch and village level
2. Wider participation in the program
3. Improved training of survey staff

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[1] *This student paper was prepared in 2012 as a requirement for the Masters of Public Policy Program at the Sanford School of Public Policy at Duke University. The research, analysis, and recommendations contained in this paper are the work of the student who authored the document, and do not represent the official or unofficial views of the Sanford School of Public Policy or of Duke University. Without the specific permission of its author, this paper may not be used or cited for any purpose other than to inform the client organization about the subject matter. The author relied in many instances on data provided by the client and makes no independent representations as to the accuracy of the data.*

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Policy Question

Was BRAC's community health sensitization program effective in its strategy for increasing malaria prevention behaviors?

Problem Statement

In December 2010 BRAC Uganda began a community health sensitization program, in which they provided mentors in their adolescent groups (ELA) with megaphones and short health sensitization scripts. BRAC staff instructed the girls to make health announcements on a daily basis while walking through their villages. In order to facilitate evaluation, BRAC Uganda implemented this program as a randomized control trial. The Research and Evaluation Unit would like to study the effects of the program in order to determine future use. This project is a quantitative evaluation of the program's effect on use of malaria prevention behaviors.

Literature Review

Malaria in Uganda

Malaria is a preventable disease that affects over 200 million people worldwide. The World Health Organization (WHO) estimates that there are 225 million global malaria cases and the number of deaths from malaria in 2009 are at least 781,000, of which 91% were in Africa. Costs of malaria are very high in developing countries, especially given the low income levels and multiple cases of malaria that characterize these households. Studies give a variety of high cost estimates for dealing with the disease, but all agree that the burden is extreme (Deressa 2007, Ettling 1991, Guiguemdé 1997, Konradsen 1997, Russell 2004, and Shepard 1991).

In 2006 the World Health Organization estimated there were 10.6 million cases throughout Uganda, with 94% of the country's 29.9 million residents living in areas of high transmission (WHO 2008, 120). Though the majority of cases reported are not laboratory confirmed, the Ugandan Bureau of Statistics (BOS) estimates that at least 60% of cases are likely treated at home, meaning that numbers cited may actually greatly understate the number of cases in the country (Uganda BOS 2010, 4). BRAC's sensitization program took place in 12 villages in Eastern Uganda, all of which are located in regions categorized as having medium-high or very high malaria transmission levels (Uganda BOS 2010, 2).

Malaria prevention with bednets

As part of its efforts to combat malaria, the WHO encouraged governments to aim for full coverage of vulnerable population with long-lasting insecticidal nets (LLINs[2]), citing extensive public health benefits (WHO Global Malaria Program 2007). In a literature review of the health effects of bednet use, Lengler found that insecticide treated net (ITN) use was able to halve the number of malaria cases in most areas and estimated that "full ITN coverage could prevent 370,000 child deaths per year" (Lengler 2006). Research suggests that the coverage rate of a village can decrease malaria morbidity and mortality when coverage rates exceed 50% (Hawley 2003), though reaching the full potential of ITN's community protection likely requires coverage rates much higher than the Abuja Summit's goal of 60% (Curtis 2003). As a result of the "herd immunity" effects of LLIN use, there are public, as well as private, benefits to wide, proper use of LLINs. Therefore, provision of LLINs has been a key piece of many malaria prevention programs, including the Global Malaria Action Plan and the Roll Back Malaria Partnership (Vanden Eng 2010).

[2] ITN refers to nets that have been treated with insecticide and generally require retreatment every six months. LLINs are a subset of treated nets that release insecticide over five years, and do not generally require retreatment.

There are two important indicators that have to be considered when discussing bednets as part of any public health policy: net ownership and net use. Ownership of a net is obviously a necessary step in any policy that relies on use of bednets to combat malaria. Many studies have examined the effects of providing nets to households and policy options for doing so (See Lengler 2006, Howard 2000, Ahmed 2010, and Dupas 2010, among others). Recent studies of net ownership conclude that improving bednet ownership among the poorest households required a concerted effort by service providers to make nets available at no or very-low cash cost (Cohen 2010 and Ahmed 2010). But getting nets into households is only half the battle.

Use of bednets

Previous studies by BRAC Uganda (Ahmed 2010), as well as other organizations and health scholars (Iwashita 2010, Baume 2007, Pulford 2011, Alaii 2003, Toe 2009, among others), have noted that households which own nets often use them at rates much lower than 100%. Though access to affordable nets is still a difficult hurdle for many of the poor in Uganda, these studies suggest that there are households whose nets go unused on a regular basis.

Surveys investigating reasons for non-use, conducted in a variety of settings, have highlighted a number of technical, environmental, and social/cultural factors that keep net usage rates from reaching 100% levels. Technical issues generally relate to difficulties hanging the net or highlight sleeping arrangements and housing types that are incompatible with net use (Iwashita 2010). Environmentally, net use tends to vary with the seasons, decreasing when the perceived risks of infection are lower or when hot weather makes sleeping under a net uncomfortable (Binka 1997 and Batega 2004). Social and cultural issues for non-use include a lack of awareness of the benefits of regular mosquito net use and, more generally, non-habituation(Baume 2009 and Vanden Eng 2010).

While technical issues for net non-use will require the development of nets better adapted to close living quarters, combined sleeping and living spaces, and outdoor sleeping habits in order to overcome the hurdle to greater net use (Baume 2009 and Iwashita 2010), environmental and social/cultural barriers may be overcome by improved community education and sensitization campaigns (Ndjinga 2010 and Batega 2004).

Encouraging more regular net use by those people who already own nets could serve to increase net use without relying on costly distribution schemes, but the question is how best to encourage unconvinced people to use the nets. A community program encouraging water chlorination suggest that use of local spokespersons had a greater effect on water cleanliness than door-to-door social marketing by non-community members (IPA 2011). These preliminary results suggest that involving local promoters could result in more effective interventions than a traditional education campaign by outside organizations.

Researchers in a community-based education program found that increased knowledge of mosquito breeding habits and methods of vector control in the community was directly related to reductions in mosquito populations (Yasuoka 2006). This finding contrasted with results of previous vector reduction programs; the authors hypothesized that the critical difference was the focus on community education. Elementary school-based education programs in Puerto Rico were found to not only increase knowledge at a household level, but also to reduce the number of mosquito larvae found in the students' homes (Wench 2002). Such studies suggest a clear link between education programs, increased knowledge, and actual changes in vector indicators when education is conducted with a focus on community actors and local contexts.

Program Background

In an attempt to boost net usage rates, a community health sensitization, or micking, program developed and implemented by BRAC Uganda provided leaders of BRAC Empowerment and Livelihood for Adolescents (ELA) groups with a megaphone in 12 randomly-selected villages of Eastern Uganda. Program staff instructed these leaders, called mentors, to make daily health announcements that encouraged mosquito net use and other malaria prevention behaviors. (See Appendix A: for more information on ELA and ELA mentors.) As a result of previous net ownership campaigns by public and NGO actors in the region, 60% of households surveyed for this project already owned nets, which suggests a general familiarity with nets and their use, even for non-owners.

Data

Sampling strategy and data collection

In September 2010 BRAC Uganda conducted a baseline survey in 24 villages within 6 BRAC branches in Eastern Uganda (see Figure 1 for timeline). The six branches that took part in the survey were non-randomly chosen by BRAC Country Office staff for facilitation of the project and similarities of the branch areas in terms of population demographics and environment. ELA program staff within each chosen branch identified four clubs to take part in the project from the 12-15 they managed at the branch. Program staff were instructed to pick clubs that were roughly comparable to one another and had mentors who were willing and able to conduct the program. Once the 24 clubs were identified, two from each branch were randomly assigned to the treatment group with the remaining two assigned to the control group.

Figure 1: Evaluation Timeline

The short surveys were conducted with 50 households in each village during the rainy season in September 2010. The households were chosen non-randomly based on location near the ELA club house before staff assigned the club to treatment or control status. Members of ELA clubs conducted the surveys; BRAC staff trained and supervised the enumerators. The survey included a brief household roster with basic demographic variables and malaria indicators for each household member, as well as household level poverty characteristics and health knowledge indicators.

In November and December of 2010, ELA program staff provided megaphones and instructions to the leaders of the 12 ELA clubs assigned to the treatment group. Mentors were instructed to make the daily announcements in their local language, and, in most cases, to make the announcements in the afternoon or evening while walking around their village.[3]

After the program had been in place for about 6 months, and at the end of the first rainy season, BRAC survey enumerators conducted a follow-up survey. These enumerators were BRAC employees, unconnected to the ELA program, whom BRAC had hired and trained to conduct the survey in the local languages. This survey took place in the first three weeks of July 2011 and included 1,003 of the original households (See "Data Limitations" below for more information on attrition in the follow-up survey). This survey included

[3] In a few cases the girls were told to make announcements only three times per week. However, given the decrease in activity after the first few months of the program, most mentors reported very similar uses of the megaphone.

the data collected at baseline in addition to information regarding exposure to the sensitization program, more detailed health and knowledge indicators, relationship with the ELA mentor, social network within the village, and a roster of all household nets, including brand, type, age, and other characteristics.

Effectiveness of randomization

Full results for the tests of balance between treatment and control groups can be found in Appendix B: . Tests of balance were conducted with proxy regressions, which clustered standard errors at the village level. The treatment and control groups are well balanced, showing significant differences in only two of 101 baseline indicators. Among households that completed the follow-up survey, there is similar balance, with only one of the indicators showing any significant difference.

For households that attrited from the survey, there are some significant differences in indicators of net ownership and self-perceptions of poverty levels. These findings suggest that treatment village households that did not complete follow-up surveys were more likely to describe themselves as "very poorer" or "better" than their neighbors, whereas control group households were more likely to characterize themselves only as "poorer." Also, attrited households in treatment villages were more likely to own three or more nets than attrited households in control villages. It is important to remember though, that these findings are based only on 164 households (79 control, 85 treatment), of which nearly half (83) come from just six of the 24 villages. Given these facts, and the fact that p-values of less than 10% are only present in 6% of our tests, I am confident that the treatment and control groups were well randomized at baseline, and that attrition was random enough as to not invalidate the results below.

Limitations of the data

Sample representativeness

While the sampling design was randomized at the village-level, households within each village were not chosen randomly. Therefore, I used data from the 2009 Uganda Malaria Indicator Survey to investigate representativeness of the household survey data. BRAC respondents were compared to MIS respondents in their region (Busia, Mukono, or East Central). Analysis shows differences in house construction and fuel type (common poverty indicators), as well as the number of children and net ownership and use statistics. On the whole, BRAC respondents seem to be slightly wealthier with higher levels of net use and ownership than others in their region. Findings from tests of representativeness are in Appendix E: .

The non-representativeness of the sample has implications for the generalizability of findings from this study to future programs. Since survey respondents are not representative of their regions, one may question whether the findings here can be expected to hold for poorer households in the same region. These findings may provide a view of the program effects that do not hold for other sub-populations in the region. On a wider scale, it is difficult to say if the context affecting this study's results is different than other regions of Uganda, which could have implications for any further scale up of the program.

Contamination

While randomization seems to have been carried out fairly effectively, there are indications that there was contamination of some of the control villages. In one control village the mentor made malaria announcements independently of the BRAC program after hearing about the program from a fellow mentor in a treatment village. Another control village was located close to a treatment village, and some residents of the control village heard announcements in the treatment village on market days.

Study attrition and data quality

Follow-up took place after less than a year, yet the response rate is only 86% for properly conducted (see discussion of duplicated surveys below) baseline surveys. Part of this attrition is due to a major building project in one village that forced many respondents to relocate, some of whom moved to villages outside of the survey area or were otherwise unreachable. In the case that enumerators could not locate baseline respondents, the households were characterized as unable to reach for follow-up. Those households for whom survey enumerators were able to find community members who knew a missing survey respondent are listed in Table 1 below as "respondent known, not found." There were 77 households for which survey enumerators could not find any community members who knew listed members of the household ("respondent unknown" in Table 1).

A number of baseline surveys had to be dropped from the project, since we found on follow-up that multiple people from the same household had been interviewed. These surveys were characterized as duplicate and dropped from the study. They are therefore not included in any testing for balance in baseline data. Enumerators only classified households as duplicates if they were able to provide an ID number for the duplicate survey respondent's actual household. 34 (2.8%) of the original 1,201 surveys were found to be duplicates. It is possible that the issue of falsified data could be the reason for some of the households that enumerators were unable to find.

Table 1: Follow-up survey outcomes (for unduplicated baseline surveys)

Completed follow-up	Did not complete follow-up			**Total**
	Respondent known, not found	Respondent unknown	Survey lost	
1,003	83	77	4	**1,167**
86.0%	7.1%	6.6%	0.3%	**100%**

Methods

Creation of poverty and knowledge indices

Both malaria prevention knowledge and poverty are measured by multiple questions in the baseline and follow-up surveys. The follow-up survey also includes additional knowledge questions. In addition to using the individual indicators as explanatory variables, I used indices to make categorization and description of respondents simpler and used both measurements to test the robustness of my findings.

Poverty

A first poverty index is constructed based on other poverty research by BRAC. The full scoring and weights are included in Appendix C: . The index consists of scores applied to each possible answer, where higher scores indicate an answer correlated with lower poverty status. These raw scores are then added, and the sums are reported as the poverty index score.

Knowledge

The basic knowledge index is constructed in a similar fashion to the poverty index, with varying scores assigned to each answer. The final score is a sum of the three bednet use knowledge indicator raw scores. The scores used are detailed in Appendix D: . Additional questions were asked regarding malaria prevention knowledge in the follow-up survey. The detailed knowledge index is created by adding raw scores for each of the additional indicators to the basic knowledge score. These scores are also detailed in Appendix D: .

Measurement of treatment effects

To evaluate the outcome differences between control and treatment groups, I ran ordinary least-squares (OLS) regression on each outcome variable, using treatment assignment as the main explanatory variable. Though the two groups are roughly balanced, I also ran models including additional explanatory variables for increased precision. The inclusion of various sets of covariates did not have much effect on the coefficients for treatment effect or the associated standard errors. The basic regression took this following form:

$$[1]\ Y = \alpha + \beta_1(Treatment) + \beta_{2i}X_i + \varepsilon$$

The outcomes to be examined (Y) are various measures of malaria prevention behaviors, including net use and ownership, net acquisition, knowledge of prevention techniques, and bednet discussions held with others. In this equation α is the constant, representing the outcome for respondents living in control villages. β_1 is the estimate of the treatment effect. X_i represents a number of explanatory factors for which the equation controls, while β_{2i} estimates the effects of each X_i on the outcome variable of interest. Finally, ε is the measure of random error. For all of the regressions, I clustered all standard errors at the village level.

For outcome variables that were also collected at baseline, I performed difference in differences regressions, in order to ensure that findings from equation [1] are actually due to the treatment and not pre-existing differences between the groups. Though I will report some difference-in-difference results below, in most cases these mirrored the results of the basic regression. In this case results from equation [1] are preferred for the reduced noise in the estimations. The difference-in-difference equation takes the following form:

$$[2]\ Y = \alpha + \beta_1(Treatment) + \beta_2(After) + \beta_3(Treatment * After) + \beta_{4i}X_i + \varepsilon$$

In this case β_1 is an estimate of the effect of living in a treatment village on the outcome in either time period, β_2 is the effect of being in the secon time period for the control group on the outcome, and β_3 is the treatment effect in the second time-period, our coefficient of interest. As in Equation [1], β_{4i} is a vector of coefficients corresponding to various control variables.

For all of the regressions reported in this paper I ran simple models, as well as difference-in-difference models, including various sets of explanatory variables. Generally I have only reported the results from simple models with a basic set of control variables, since all estimates were fairly stable across the various model specifications. In the regression tables that follow, I have only included a subset of the control variables; not all coefficients are reported.

Controlling for exposure

As described in the contamination section on page 4, some control village respondents were exposed to the program. Additionally, the follow-up survey even showed differential rates of treatment households reporting having heard the announcements (See Table 2). To investigate the possible additional effect of hearing the announcements above treatment status of a village, one model of equation [1] will include an indicator variable, "heard" for any household that was exposed to the announcements, and interact the indicator with the treatment variable (Treatment*heard), yielding Equation 3:

$$[3]\ Y = \alpha + \beta_1(Treatment) + \beta_2(heard) + \beta_3(Treatment * heard) + \beta_{4i}X_i + \varepsilon$$

In the above equation, $\beta_1 + \beta_3$ will provide the overall effect for those in the treatment villages, while β_2 measures any spillover effect of hearing the treatment on control village households. Since there were no

cases in which controlling for exposure provided any different treatment effect estimates, I have not included the results from these regressions below.

Table 2: Respondent reports of program exposure

	Did not hear	Did hear	**Total**
Control	551	52	**603**
Treatment	450	148	**598**
Total	**1,001**	**200**	**1,201**

Seemingly unrelated regressions

Net ownership can be looked at as a collection of dichotomous variables, in order to investigate changes in the distributions, rather than just changes in means. For example, the number of nets a house owns can be a continuous variable, looking at the effect of treatment status on total net ownership. Another method, however, is to look at the effect of treatment status on the likelihood of owning no nets, owning one net, owning two nets, etc. These effects are all calculated in separate regressions, but simultaneously, in order to allow for joint tests of significance between the treatment effects on the related outcome variables.[4] Use of the seemingly unrelated regression method allows us to perform statistical tests of significance of differences across groups of regressions.

Imputation of data

Eight households' surveys did not indicate whether a member of the household was a BRAC microfinance member. Since these households did not have members involved in other BRAC programs, and only 20% of all survey respondents had microfinance members in the household, I categorized these households as not being microfinance members. For those households missing a poverty indicator (51 households), their poverty scores were computed with the mean poverty indicator scores for any missing indicator. When I included an indicator for use of imputed values in the vector of covariates, the coefficient was not statistically different from zero, showing no effect of use of imputed values on the outcome.

Results

Net ownership

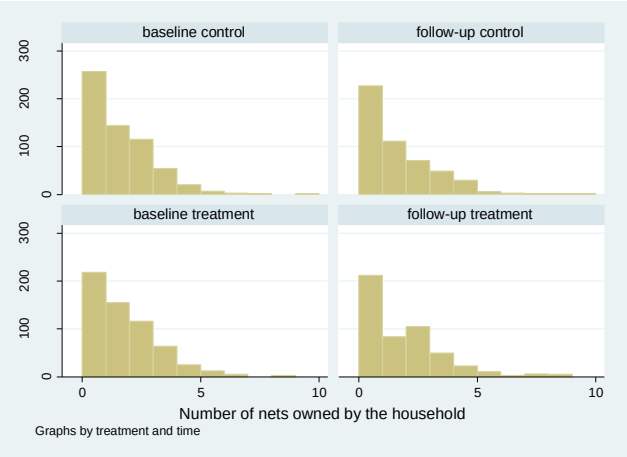
After clustering standard errors at the village level, treatment households have a higher, but not significantly higher, number of nets at baseline than the control group. Comparing averages of net ownership in the follow-up period shows no significant difference between the treatment and control group. Even after controlling for various baseline characteristics and exposure to the program, analysis showed no significant treatment effect on number of bednets owned, even when control variables and variables controlling for exposure to the program are included in the regression (not shown). Unfortunately, I cannot find evidence to support any claims that the program affected average net ownership in these villages.

While there was no effect on average net ownership, averages are not necessarily the only way to illustrate changes in ownership, since they are mainly driven by the many households in the sample that own no or many nets. Consider a situation in which there are 100 households, 50 of which own no nets and 10 of which own 1, 2, 3, 4, or 5 nets. The average net ownership in this sample is 1.5 nets per household. Now suppose that a public announcement program was put into place, encouraging net use. All of the households owning only one net realized it was not enough to properly cover their families and purchased an additional net.

[4] See Zellner, 1962 for more discussion of this technique.

Assume that the program had no effect for the rest of the population, though 3 of the five-net households each discarded one old net. As a result of these shifts, the average net ownership will only change slightly, to 1.57. This change is not statistically significant, but it is obvious from the story that there is a transition happening here that is not accurately captured through discussion of averages. Figure 2 clearly shows a shift in net ownership distribution for those in the treatment and control groups at both baseline and follow-up similar to that described in the hypothetical situation above.

Figure 2: Distribution of net ownership over time, by treatment status



In follow-up, treatment villages have fewer households owning only one net, but a higher number of households owning two nets. This difference is especially noticeable in the bottom-right histogram showing net ownership by treatment groups in the follow-up period. While the other three graphs show a continuous decline in the number of nets owned by households, the fourth graph shows an obvious dip in the number of households with one net, and a larger number of households with two nets. This change is not seen in the control group, even though (as the two left-hand graphs show) net ownership was very similar between the two groups at baseline. Measuring net ownership as a collection of dichotomous variables allows for a richer understanding of household habits than would a simple count.

To statistically test for shifts in this distribution, I created a series of variables indicating whether a household owned no nets, one net only, two nets only, three nets only, or four or more nets. I ran separate regression with each indicator variable as the outcome variable of interest. Running these five regressions together allows me to perform post-estimation tests of joint significance. The regression results from this process are in Table 3. Similar results were obtained when branch fixed effects and exposure to the program were taken into account, as well as when a difference-in-difference model was used (not shown).

Table 3: Program effects on the distribution of net ownership

VARIABLES	(11) Own 0 nets	(13) Own 1 net	(15) Own 2 nets	(17) Own 3 nets	(19) Own 4+ nets
treatment	-0.025	-0.054*	0.075*	0.006	-0.002
	(0.077)	(0.032)	(0.039)	(0.025)	(0.024)
% of hh members using net at BL	-0.023	0.037	-0.063	0.045	0.005
	(0.090)	(0.064)	(0.075)	(0.053)	(0.051)
HH size at BL	-0.003	-0.018*	-0.025***	0.016	0.030***
	(0.012)	(0.010)	(0.009)	(0.010)	(0.008)
# of girls (not head) in hh at BL	0.015	-0.005	0.009	-0.003	-0.016*
	(0.014)	(0.009)	(0.010)	(0.011)	(0.008)
Female hh head at BL	0.084**	-0.011	-0.105***	0.033	0.001
	(0.042)	(0.037)	(0.024)	(0.027)	(0.017)
BL poverty score	0.001	0.001	-0.001	-7.01e-05	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
BL malaria knowledge score	-0.011	-0.001	0.008	0.015*	-0.011
	(0.013)	(0.008)	(0.008)	(0.008)	(0.007)
Constant	0.479***	0.160	0.205***	0.058	0.099
	(0.170)	(0.158)	(0.075)	(0.089)	(0.099)
Observations	1,001	1,001	1,001	1,001	1,001

Robust standard errors in parentheses; standard errors are clustered at the village level
*** p<0.01, ** p<0.05, * p<0.1

The first thing to notice above is that the treatment effects all add to zero. Every household in the study belongs to one of these 5 groups, and any decrease in one category must be matched by an increase in another category. Table 3 shows a decrease in the number of treatment village households owning one net only and an increase in the number house households owning two nets. These findings are significant at the 10% level. While there was no significant change to the average number of nets owned in a village, treatment villages have about a 5% decrease in the number of households owning one net and 7% increase in the number of households owning two nets. A joint chi-squared test of these results is 7.41 (p-value=0.007), showing that this shift, visible in the data, is statistically significant. This result shows that the program does seem to have an effect on net ownership. I cannot say from the data exactly how or why these changes occurred, though it seems plausible that the announcements made households aware their ownership of only one net was not optimal for their needs.

Net acquisition during the program

The results above indicate that there was a change in net acquisition, but does not explain how the change happened. There are two ways of measuring acquisition of nets in this program. The first is a simple comparison of the number of nets owned in both surveys (column 1 in Table 4). Any household that shows more nets in the follow-up survey than at baseline is categorized as having more nets. Since the baseline survey was held many months before the micking program started, this measurement of net acquisition will also pick up any nets purchased in the four to five months between the baseline survey and the beginning of the program. This measure only works if one assumes that acquisition habits during this time period remained balanced between treatment and control villages. This measure may also underestimate the number of nets acquired during the period if a household discarded more nets than it acquired. In such a case, a household that acquired new nets would not be characterized as such since their total number of nets owned actually decreased. Using differences in net count between the two surveys may cause us to miss a story in which households are getting rid of old, unusable nets and replacing them with (fewer) new nets.

Another option is to use the number of nets households acquired during the program time (column 2 in Table 4). Luckily, there are only 15 households (3% of net owning households) that reported owning nets but refused to provide the net characteristic information requested in the roster. Using this measure will not, however, take account of the fact that these households, while gaining nets in the relevant time period, may have discarded nets. In this case the household could actually own fewer nets after the program than before, but we would still consider them a net-acquiring household. In fact, there are 29 households that reported acquiring a net in the six months before the survey but did not show a gain over time in the number of nets owned. This is nearly 47% of all households reporting recent acquisition of a net.

Since both the variables discussed above reflect various facets of recent net acquisition, I will use both in my analysis of responses to the micking program. Unfortunately, as seen in Table 4 below, there seems to be no significant difference in net acquisition between the treatment and control groups. These findings do not vary across model specifications. Therefore, there is no evidence in the data that BRAC's community sensitization program had any effect on overall net acquisition in the treatment villages. These findings hold even if I restrict the sample to sub-populations defined by number of nets acquired or number owned at baseline (results not shown).

Table 4: Program effect on net acquisition during program

VARIABLES	(1) Increased ownership	(2) Program acquisition
treatment	0.035	0.002
	(0.07)	(0.03)
% of hh members using net at BL	0.043	-0.221**
	(0.09)	(0.07)
HH size at BL	0.014	-0.010
	(0.01)	(0.01)
# of girls (not head) in hh at BL	-0.008	0.001
	(0.01)	(0.01)
Female hh head at BL	-0.006	-0.052
	(0.03)	(0.03)
BL poverty score	0.001	0.001
	(0.00)	(0.00)
BL malaria knowledge score	-0.002	0.043***
	(0.01)	(0.01)
Constant	0.284	0.164
	(0.17)	(0.35)
Observations	1,002	548
R-squared	0.122	0.077

Robust standard errors in parentheses; standard errors are clustered at the village level
*** p<0.01, ** p<0.05, * p<0.1

Though there was a shift in the distribution of nets owned in treatment villages, there is no evidence in the data that this shift was a result of increased acquisition during the treatment period, no matter the measurement used for "acquisition." It could be the case that the shift highlighted above from owning only one net to owning two could result from a mixture of net acquisition and a higher propensity to keep nets, instead of discarding old ones. However, this is only speculation and cannot be proved with the data. Another possible culprit for the lack of statistically significant results is the fact that the study was not powered to detect the small changes in acquisition patterns that would produce the shift from households

owning one to two nets. Further study is needed to understand how net acquisition may be driving this change in distribution of net ownership.

Methods of net acquisition

There are two ways for a household to acquire a net: households can receive a net for free or purchase it. Table 5 shows the effect of treatment on the two methods of net acquisition. Note that because households often owned more than one net there can be overlap in the characterization of households: 281 households received at least one net for free, 318 purchased at least one net, and 56 reported both receiving at least one net *and* purchasing at least one net. Therefore, a positive effect on one variable would not necessarily mean a negative effect on the other. The effect of treatment on whether households receive free nets is not statistically different than zero[5]. Considering that acquisition of a free net is generally a result of decisions made by the government or NGOs running a net distribution program, rather than individuals on the ground, this result is not entirely a surprise.

Table 5: Program effect on nets acquisition methods

VARIABLES	(1) Received at least one net for free at any time	(2) Purchased at least one net at any time
Treatment	-0.056	0.089
	(0.10)	(0.09)
% of hh members using net at BL	-0.043	-0.028
	(0.12)	(0.11)
HH size at BL	0.038*	-0.004
	(0.02)	(0.01)
# of girls (not head) in hh at BL	-0.033	0.017
	(0.02)	(0.02)
Female hh head at BL	0.010	0.025
	(0.07)	(0.06)
BL poverty score	-0.004*	0.003
	(0.00)	(0.00)
BL malaria knowledge score	0.011	0.006
	(0.02)	(0.02)
Constant	0.539	0.670
	(0.37)	(0.33)
Observations	548	548
R-squared	0.092	0.060

Robust standard errors in parentheses; standard errors are clustered at the village level
*** p<0.01, ** p<0.05, * p<0.1

The BRAC micking program shows a slight positive effect on nets purchased. While the results are not statistically different than zero, the small standard error (in comparison with the coefficient) suggests that these findings may be indicative of an underlying pattern in the data. The finding is an indication that the shift in the distribution of net ownership described above could be the result of households in treatment villages increasing their purchases of nets. However, post-estimation comparisons of the two findings do not provide any evidence that the treatment coefficients are statistically significantly different from one another

[5] This sample is not restricted to any sub-population, but includes all households who owned a net at follow-up. Therefore, it does not specify when nets were acquired. With the assumption that groups were evenly balanced at baseline (see Appendix B:), including how they acquired nets, any results should be due to nets acquired between the two surveys. There is little reason to think that there would be any differences between the time of the survey and the beginning of the program, so I will discuss the results in Table 5 as the effect of the treatment on acquisition habits. It should be clear, however, that as a result of imprecise measurement and recall this may not be the case.

(p=0.43). While these findings merit more investigation, it is difficult to conclude that there was any effect of treatment on the methods by which households acquired nets.

Net use

The findings from regressions on individual net use generally suggest a positive effect of the treatment, but the effects are very small and not statistically significant. Use of nonlinear models does not show significantly different outcomes, so for ease of interpretation I have only reported results from linear probability models. As with the reports above for net ownership, there is little effect of varying model specifications on the findings. Table 6 shows the effects of treatment on net use and includes interaction terms between the treatment effect and whether an individual is a child or a pregnant/nursing woman. There is no significant treatment effect on pregnant women, and there is even a statistically significant negative coefficient on the treatment interaction for children. Both treatment interactions are negative when the subsample is restricted to females, though the coefficients are not statistically significant.

Table 6: Program effect on net use by household members

VARIABLES	(1) All surveyed hh members	(4) Females only
Treatment	0.081	0.072
	(0.07)	(0.06)
Individual is a child	0.196***	0.211**
	(0.03)	(0.06)
Individual is pregnant or nursing	0.178**	0.183*
	(0.06)	(0.08)
Individual is female	0.031**	
	(0.01)	
Interaction between pregnant and treatment	-0.130	-0.118
	(0.07)	(0.09)
Interaction between child and treatment	-0.107*	-0.118
	(0.04)	(0.07)
Age	0.002*	0.002*
	(0.00)	(0.00)
% of hh members using net at BL	0.141*	0.177**
	(0.06)	(0.06)
HH size at BL	-0.006	-0.008
	(0.01)	(0.01)
BL poverty score	-0.001	-0.001
	(0.00)	(0.00)
BL malaria knowledge score	-0.011	-0.021
	(0.01)	(0.01)
# of girls (not head) in hh at BL	-0.012	-0.012
	(0.01)	(0.01)
Female hh head at BL	-0.088*	-0.081*
	(0.04)	(0.04)
Age of hh head at BL	-0.005**	-0.004*
	(0.00)	(0.00)
Constant	0.687**	0.703**
	(0.21)	(0.21)
Observations	5,102	2,830
R-squared	0.113	0.131

Robust standard errors in parentheses; standard errors are clustered at the village level
*** p<0.01, ** p<0.05, * p<0.1

Malaria prevention behaviors

The announcements made by girls in the adolescent clubs encouraged a variety of malaria prevention behaviors besides net use. Table 8 shows the effect of treatment on whether respondents identified three malaria prevention behaviors that were mentioned in the announcements, in response to the question "What can people do to prevent malaria?" without any prompting from the survey enumerator.

Table 7: Recall of preventative behaviors

Preventative measures	% of respondents recall
Clean bushes near the house	43.4%
Clear standing water	27.2%
Anti-malarials for pregnant women	20.5%

Table 7 shows that recall of all behaviors were relatively low (below 50% recall by households), so there should have been room for treatment to improve these measures. It is important to note that these variables merely measure recall of the measure, and not actual use of the prevention behavior. In Table 8 below, I have also included an indicator of whether respondents mentioned use of mosquito coils, a measure that was not included in the announcements made by the ELA club members. While there is no treatment effect for mention of mosquito coils (to be expected since it did not form part of the treatment program), there is also no significant effect of treatment on any of the other indicators.

Table 8: Program effect on recall of non-net malaria prevention behaviors

VARIABLES	(1) Use of mosquito coils	(2) Anti-malarials for pregnant women	(3) Clear standing water	(4) Clean bushes near the house
treatment	0.029	0.034	0.025	0.078
	(0.05)	(0.11)	(0.07)	(0.10)
% of hh members using net at BL	-0.096	0.155*	0.039	0.170
	(0.06)	(0.07)	(0.08)	(0.09)
HH size at BL	0.006	0.019*	0.010	0.003
	(0.01)	(0.01)	(0.01)	(0.01)
# of girls (not head) in hh at BL	-0.014	-0.009	-0.002	0.034*
	(0.01)	(0.01)	(0.01)	(0.01)
Female hh head at BL	0.020	-0.102*	0.045	-0.070
	(0.03)	(0.04)	(0.03)	(0.04)
Age of hh head at BL	0.000	-0.002	-0.000	-0.001
	(0.00)	(0.00)	(0.00)	(0.00)
BL poverty score	0.000	0.003*	0.003*	0.003
	(0.00)	(0.00)	(0.00)	(0.00)
BL malaria knowledge score	0.032**	-0.017	-0.012	0.006
	(0.01)	(0.01)	(0.01)	(0.01)
Constant	0.150	0.242	-0.115	0.335
	(0.16)	(0.15)	(0.17)	(0.18)
Observations	1,002	1,002	1,002	1,002
R-squared	0.061	0.071	0.047	0.079

Robust standard errors in parentheses; standard errors are clustered at the village level

*** p<0.001, ** p<0.01, * p<0.05

When I rerun the regressions using reported use of anti-malarials by pregnant women, there is no significant effect of treatment, though the coefficient is slightly higher than the coefficient on recall of the method (Table 9). Cleared water (column 2) and cleaned bushes (column 3) are reported by enumerators, answering the questions "Are there bushes close to the respondent's house that need to be cleaned?" and "Is there standing water around the respondent's house?" The treatment effect on a household having not cleaned bushes is positive, but not statistically significant. The effect on cleared standing water is also positive, and in this case the effect is statistically significant. These findings suggest that households in treatment villages are more likely to have standing water than those in control villages, even when controlling for the fixed effects of different enumerators.

Table 9: Program effect on use of non-net malaria prevention behaviors

VARIABLES	(1) Use of anti-malarials by pregnant women	(2) Enumerators report bushes not cleaned†	(3) Enumerators report water not cleared†
treatment	0.059	0.046	0.058**
	(0.07)	(0.04)	(0.02)
age	-0.005		
	(0.00)		
% of hh members using net at BL	-0.351**	0.144**	0.083
	(0.10)	(0.05)	(0.04)
HH size at BL	-0.024	0.012	-0.000
	(0.03)	(0.01)	(0.01)
BL poverty score	-0.001	-0.001	-0.001
	(0.00)	(0.00)	(0.00)
BL malaria knowledge score	0.029	-0.002	-0.009
	(0.02)	(0.01)	(0.01)
# of girls (not head) in hh at BL	-0.006	-0.004	0.002
	(0.02)	(0.01)	(0.01)
Female hh head at BL	-0.054	0.034	0.025
	(0.07)	(0.02)	(0.02)
Age of hh head at BL	0.003	0.001	-0.000
	(0.00)	(0.00)	(0.00)
Constant	0.778*	-0.172	0.031
	(0.31)	(0.14)	(0.07)
Observations	234	995	994
R-squared	0.148	0.266	0.072

Robust standard errors in parentheses; standard errors are clustered at the village level
*** p<0.001, ** p<0.01, * p<0.05
†These results also control for enumerator fixed effects, the coefficients of which are not included in the table.

It is important to note that variables indicating which enumerator carried out a survey were actually statistically significant in these regressions. A higher likelihood for some enumerators to report that a household needed to clear water or clean bushes, suggests bias in the response to these questions. While enumerators were given general guidelines on how to answer this question, it is still a very subjective question and dependent on the enumerators' personal perceptions. Responses may also be framed by what was learned in the survey, since the questions were included at the end of the survey form and generally filled out

after the survey was completed. Therefore, while these are significant findings, it is likely that they reflect enumerator biases rather than an actual effect of the program on household cleanliness.

Malaria knowledge scores

There are no statistically significant results of the program on respondents' knowledge scores (column 1, Table 10), which are a composite of stated knowledge about net use within the household and recall of rules for net use. Nor is there any significant effect on the augmented knowledge index that includes recall of prevention behaviors (column 2; includes recall investigated in Table 8 and Table 9, above). Households had a mean basic knowledge score of 3.18 out of a possible 12 points, while the average augmented knowledge scores were 5.12 out of a possible 17 points. These low scores suggest that there should have been room for treatment to improve these measures.

Table 10: Program effect on knowledge scores of survey respondents

VARIABLES	(1) Basic Knowledge Score	(2) Augmented Knowledge Score
treatment	0.136	0.347
	(0.38)	(0.35)
% of hh members using net at BL	0.267	0.412
	(0.60)	(0.55)
HH size at BL	-0.162*	-0.118
	(0.08)	(0.08)
# of girls (not head) in hh at BL	-0.022	-0.030
	(0.09)	(0.11)
Female hh head at BL	-0.142	-0.223
	(0.22)	(0.23)
Age of hh head at BL	-0.009	-0.013
	(0.01)	(0.01)
BL poverty score	0.001	0.012
	(0.01)	(0.01)
BL malaria knowledge score	-0.078	-0.055
	(0.10)	(0.09)
Constant	4.634***	6.204***
	(0.95)	(1.12)
Observations	1,001	1,001
R-squared	0.073	0.093

Robust standard errors in parentheses; standard errors are clustered at the village level
*** p<0.001, ** p<0.01, * p<0.05

Discussion of bednets with others

Respondents were asked who, if anyone, they had spoken with about bednets in the two months before the survey was carried out. They could provide multiple answers, and the possible choices were family members, neighbors, or other community members. This question was not meant to measure discussions with out-of-community people, such as NGO workers who discussed use of bednets within the community. While The survey enumerators did not define the term "neighbors" for the respondents, so it is possible that increased discussion with neighbors could have led to spillover effects to people in nearby villages, some of whom may have been respondents themselves in this study. One significant relationship noted in the table below is that higher knowledge scores were closely correlated with discussion about malaria, which suggests that people may be getting high quality information about malaria prevention through this informal communication.

Table 11 shows no statistically significant results at the 10% level, there does seem to be a positive effect on the likelihood that a household reports talking with neighbors (column 2) and a decrease in households who report that spoke with no one about bednets during the two month period (column 4). The survey enumerators did not define the term "neighbors" for the respondents, so it is possible that increased discussion with neighbors could have led to spillover effects to people in nearby villages, some of whom may have been respondents themselves in this study. One significant relationship noted in the table below is that higher knowledge scores were closely correlated with discussion about malaria, which suggests that people may be getting high quality information about malaria prevention through this informal communication.

Table 11: Program effect on respondents' discussion habits

	(1)	(2)	(3)	(4)
VARIABLES	Talks to family	Talks to neighbors	Talks to other community members	Talks to no one
treatment	0.024	0.122	0.022	-0.082
	(0.05)	(0.06)	(0.04)	(0.06)
% of hh members using net at BL	-0.121	-0.108	0.001	0.049
	(0.08)	(0.06)	(0.05)	(0.08)
HH size at BL	-0.003	-0.019	-0.012	0.010
	(0.01)	(0.01)	(0.01)	(0.01)
# of girls (not head) in hh at BL	0.013	0.007	0.008	-0.002
	(0.01)	(0.01)	(0.01)	(0.01)
Female hh head at BL	0.009	0.042	-0.023	-0.025
	(0.04)	(0.03)	(0.03)	(0.04)
Age of hh head at BL	0.000	-0.001	-0.000	0.000
	(0.00)	(0.00)	(0.00)	(0.00)
BL poverty score	-0.001	-0.002	0.000	0.001
	(0.00)	(0.00)	(0.00)	(0.00)
BL malaria knowledge score	0.024*	0.038***	0.012	-0.030*
	(0.01)	(0.01)	(0.01)	(0.01)
Constant	0.226***	0.373*	0.158	0.566**
	(0.04)	(0.14)	(0.17)	(0.17)
Observations	1,003	1,002	1,002	1,002
R-squared	0.002	0.139	0.037	0.101

Robust standard errors in parentheses; standard errors are clustered at the village level
*** p<0.001, ** p<0.01, * p<0.05

Discussion

There is no evidence that BRAC Uganda's micking program had the intended effect of encouraging greater use of nets to which households already have access. Very few households reported owning nets that they did not use, even though survey enumerators were prompted to ask people if they had any non-used nets that were not reported in the net roster. Out of the 1,244 nets for which data were collected, only 48 (4%) were listed as unused the night before. Table 12 shows no significant difference between nets reported as unused in treatment versus control groups at follow-up. It seems that there is not the significant ownership but non-use of nets for this sample that BRAC's previous research suggests. Whether this is due to underlying population differences or incorrect measurement of non-use is hard to determine.

Table 12: Reported net non-use (by treatment status)

	Nets reported as unused
Control HH mean	0.021
Treatment HH difference	-0.008
p-value of difference	0.46

Note: standard errors clustered at the village level

There do seem to be some differences in net acquisition across the treatment groups, indicating a shift away from ownership of only one net to ownership of two nets in the treatment villages. There is little in the data, though to help us understand how these nets are being acquired, since nets in treatment villages are not significantly more likely to have been acquired during the program than nets owned in control villages (Table 4). Nor are do investigations of the method of acquisition yield any statistically significant results, though, the general trends seem to suggest there may be an increase in nets purchased during the program. While there is a shift in treatment villages from owning one net to owning two nets, it is not possible to say how this shift is happening with any statistical certainty.

Another interesting outcome highlighted in the results above is the greater propensity for households to discuss bednets with their neighbors. In some cases respondents could be reporting discussions about the program itself, rather than any specific discussion of how to use bednets or encouragement for neighbors to use bednets. This may explain the apparent disconnect between higher levels of discussion but unchanging levels of net use. While it is difficult to say what the long-term effects could be of greater bednet discussion within communities, this is an outcome that merits some attention.

Recommendations

The health sensitization program, as it was implemented, does not seem to have had the intended effect of improving rates of bednet use in treatment villages. However, there is evidence that the program affected acquisition of nets in the treatment villages, with decreases in households reporting only owning one net and increases in households reporting owning two nets. The data also suggest that the program increases discussion of issues relating to bednets, though this should not come as a surprise given the highly public nature of the program. These findings suggest that the program low-touch health sensitization through public announcements was able to have an effect on community behaviors. It is not clear, however, from this evaluation whether the effects of the program are worth the costs to BRAC and the girls in the adolescent group.

If BRAC decides that the benefits of the micking program are worth the effort, it is essential that they address the low numbers of respondents who reported hearing the announcements. Unfortunately for this study, actual implementation of the program was very low. Only 37% of respondent in treatment villages report having heard the announcements. (See Table 2 on page 7.) Of those respondents who reported hearing the announcements, only 57% report having heard the announcement in the month before the survey. It is hard to say what the effects would have been if the program had been more rigorously implemented, but the results of this study seem to suggest that even at this very low level of implementation there were some slight effects on behavior in treatment villages. There are two possible reasons for the low reports of announcements heard. The first, and most obvious, is that the announcements were taking place very infrequently. In this case improved rates of announcement recall would require closer monitoring of mentor activity, different incentives, or some combination of these and other methods for increased effort on the part of mentors. Another possibility is that village members are not hearing the announcements, even though the announcements are being made.

Community members might be ignoring or blocking out the announcements, in which case they would have difficulty remembering the announcements during survey interviews. It could also be that the survey disproportionately samples households in which the female head of household (the target respondent) is generally out of earshot of the announcements when they are being made. Possible reasons could include responsibilities that take her away from her house during the normal micking time or she may live in an area of the town where the mentor did not visit. Households for the survey were chosen non-randomly and none of the data collected provide a method for controlling for location during the micking activity or proximity to the mentor's micking route. More study is necessary to understand the reason behind the low levels of announcement recall in the treatment villages.

If BRAC finds that the reason was that announcements were being made infrequently, I recommend that BRAC implement the following changes if they want to continue with the micking program:[6]

1. Greater involvement by ELA program staff at the branch and village level:
    a. Promote discussion of the micking program between mentors and ELA program staff during their regularly scheduled meetings to encourage more consistent and continuous application of the program.

    b. Encourage ELA program assistants to charge the megaphone batteries at the BRAC office for mentors in villages without reliable access to electricity. Six mentors specifically mentioned having difficulty keeping the battery charged in their village, and most of them mentioned having paid community members to charge the megaphone. By reducing the burden on mentors to find ways to charge the megaphones in villages without reliable electricity, BRAC may be able to increase mentor participation in the program. It is interesting to note that the three villages with the lowest percentage of respondents who reported hearing the announcements are all villages where the mentors mentioned difficulty keeping the megaphones charged.

2. Broaden participation in the program:
    a. Delegation of responsibility to other ELA members: As mentors feel is appropriate, they can involve more members of the club in the program, while remaining responsible for its implementation. Two of the top three villages in terms of percentage of respondents who reported hearing the announcements were villages where club members have taken part in the micking activities. In some villages, low rates of program implementation seemed to be a result of mentors who had to leave town for family reasons. In these cases it could be helpful for other girls in the club to take over responsibility for the micking program, just as they do for other club activities. Also, many girls (though not all) reported enjoying the micking. In one village a member who had made announcements reported that she was excited to do the program and did not feel shy about it, since she greatly admired the mentor who normally made the announcements. Turning responsibility for the program over to girls who enjoy the activity would likely increase the consistency of announcements.

    b. Consider implementation of a megaphone swapping system within branches. This evaluation looks only at effects after a fairly short, six-month time frame. Whether

[6] All the following information was gained during field visits to each of the villages during the fourth and fifth months of the program.

continued use of the program will continue to shift household practices in similar ways over long time periods is difficult to say. Some mentors in villages outside of the study who heard of the program, expressed interest in sharing the megaphones among several villages. While a system for keeping track of the megaphone would put additional responsibility on ELA staff, it is not infeasible, given that there are already mentor meetings that happen regularly where megaphones could be traded between mentors from different villages in the branch. Such a system could prevent fatigue while allowing exposure to more villages.

3. Improve training of survey staff: Remind survey enumerators that they should not promise rewards to respondents for completing surveys. Both mentors and follow-up survey enumerators reported instances in which survey respondents recalled being told that the baseline survey was being conducted to prepare for a program, by BRAC, to give out bednets in the village. Many mentors reported community members asking them when BRAC would be delivering nets. These inconsistencies in messaging may have hurt the effectiveness of the program, as well as BRAC's reputation in these communities. It is of the utmost importance that all research staff are trained not only in the mechanics of administering surveys, but are also given a good understanding of the way collected data will be used so they can properly inform survey respondents.

Conclusion

BRAC's micking program has limited but actual effects on community behavior. Maintenance of the program may continue to shift net acquisition patterns and encourage greater community discussion of bednets, at least in the short term. Whether other outcomes would be affected as a result of improved program implementation is not clear. It seems that the sample in this study might not be representative of Eastern Uganda, so it is hard to say how generalizable the program effects would be to other populations. Considering the low cost to BRAC of the micking program, I recommend that it continue, at least in those branches that already have the megaphones, following the recommendations above.

Appendix A: Empowerment and Livelihood for Adolescents (ELA) Program Description

Source: BRAC Uganda ELA Program materials

Introduction

BRAC has successfully pioneered and scaled its adolescent empowerment program in Bangladesh, which today reaches 300,000+ adolescent girls, providing them with life skills training, safe spaces, livelihood support and microfinance loans. Started in 1993 in the form of reading centers, ELA is set up for adolescent girls who dropped out of formal schooling. In 2002, BRAC began providing adolescent girls with financial services, recognizing that real empowerment is possible only if the girls have financial independence. In 2005, with support from the Nike Foundation, BRAC began combining the life-skills components (life-skills training, safe space etc) and the livelihood components (financial services, livelihood training) to create a unified and holistic adolescent intervention.

In Uganda, the program was introduced for the continuation of adolescent education especially for girls between the ages of 13 -19 years. ELA was set up for adolescent girls who dropped out of formal schooling, those who have never gone to school, who are in school, those who are married and adolescent mothers. The key approach to the programme is the use of a methodology that is empowering for the adolescents and gives them the voice and the capacity needed to take on responsibilities.

Components of ELA:

1. Club activities
2. Community participation
3. Life skills based education
4. Livelihood skills training
5. Financial literacy
6. Microfinance

Mentors in ELA

Club management

The clubs and the training courses are run and managed by the mentors. One leader from each club is selected and trained by BRAC Uganda to be adolescent leaders. They have to be at least 19 – 22 years old. These leaders are responsible for management of all the clubs' activities and conducting the training courses. Not only does this help to develop their leadership skills, it also gives the mentors a small honorarium which contributes to their educational and family expenses.

6.2: Some of the considerations for selecting an adolescent club leader/mentor?

- Age has to be between 19 -22
- She must be a permanent resident of the village/community
- She has to be willing to work for 4 hours a day (club opening hours)
- She should have the leadership quality, commitment and interest to work for adolescents.
- She should be socially accepted in the community.
- she **Must not** be a student

- she should be Ready to attend training and monthly refresher courses
- She should have the capacity to run the club and other related activities effectively and efficiently
- Preference should be given to those who have leadership experience either at school or in the community.
- She should have studied up to and have qualification of S-2
- She should not be member of any microfinance group.

6.3 Process of mentor selection

1. Make a possible list of capable mentors
2. There should be 3 names of possible mentors for each club
3. Set date and day for the interview.
4. Area Officer and Project Assistant will make a final list of successful mentor per club
5. Send mentor for basic training.

Appendix B: Placebo regression results detailing balance

P-values below 0.10, indicating statistically significant differences between treatment and control, are highlighted in red.
All standard errors were clustered at the village level.

	All follow-up (did not attrit)				Attrited (did not complete follow-up)				All baseline completion			
	control	treatment	all	p-value	control	treatment	all	p-value	control	treatment	all	p-value
Proportion of family members sleeping under a bednet	0.329373	0.340799	0.335058	0.79161	0.3425558	0.4408123	0.3934814	0.2076816	0.3311593	0.3553558	0.3432679	0.5295212
Proportion of family members suffering from malaria	0.554153	0.534196	0.544225	0.770904	0.5112819	0.4953595	0.5030294	0.8844443	0.548344	5.29E-01	0.5384353	0.7761043
Proportion of women who are pregnant or mothers of young children	0.081389	0.088476	0.084915	0.756986	0.0841571	9.52E-02	8.99E-02	0.7635713	0.0817637	0.089461	8.56E-02	0.7409321
Proportion of pregnant women who sleep under nets	0.364	0.416133	0.390577	0.490266	0.5	0.2916667	0.3913043	0.2626579	0.3813953	0.399537	0.3906723	0.7793324
Household owns one or more nets	0.559524	0.625251	0.592223	0.268817	0.5949367	0.6823529	0.6402439	0.3706388	0.5643225	0.6335616	0.5989717	0.1936817
Household owns two or more nets	0.327381	0.366734	0.346959	0.397698	0.3164557	0.3882353	0.3536585	0.3965145	0.3259005	0.369863	0.3479006	0.3508933
Household owns three or more nets	0.140873	0.168337	0.154536	0.442083	0.1012658	0.2352941	0.1707317	0.0153753	0.135506	0.1780822	0.1568123	0.2299863
Household owns no nets	0.440476	0.37475	0.407777	0.268817	0.4050633	0.3176471	0.3597561	0.3706388	0.4356775	0.3664384	0.4010283	0.1936817
Household owns only one net	0.232143	0.258517	0.245264	0.488359	0.278481	0.2941176	0.2865854	0.8409135	0.238422	0.2636986	0.2510711	0.4200095
Household owns only two nets	0.186508	0.198397	0.192423	0.695028	0.2151899	0.1529412	0.1829268	0.2578093	0.1903945	0.1917808	0.1910883	0.961129
Household owns only three nets	0.095238	0.094188	0.094716	0.960607	0.0506329	0.1411765	0.097561	0.052571	0.0891938	0.1010274	0.0951157	0.5507607
Household owns four or more nets	0.045635	0.074148	0.059821	0.155304	0.0506329	0.0941176	0.0731707	0.3869821	0.0463122	0.0770548	0.0616967	0.1558865
Nets were purchased	0.623239	0.569132	0.594958	0.635804	0.6808511	0.5762712	0.6226415	0.3701456	0.6314199	0.5702703	0.5991441	0.5600271
Nets were provided for free	0.304444	0.390244	0.345349	0.384585	0.3880597	0.4861111	0.4388489	0.5680365	0.3152805	0.4045643	0.3583584	0.3712343
Hangs net before evening	0.173729	0.2	0.186766	0.738105	0.2763158	0.3552632	0.3157895	0.6071281	0.1879562	0.2218115	0.204775	0.696399
Hangs net at evening	0.161017	0.204301	0.182497	0.420384	0.1578947	0.25	0.2039474	0.1651479	0.1605839	0.2107209	0.1854913	0.3271493
Hangs net after evening	0.108051	0.113979	0.110993	0.909417	0.0394737	0.0394737	0.0394737	1	0.0985401	0.103512	0.1010101	0.9174831
Hangs net before sleep	0.557203	0.48172	0.519744	0.517832	0.5263158	0.3552632	0.4407895	0.3777231	0.5529197	0.4639556	0.5087236	0.4639269
Reasons for nonuse of net												
Not habituated	0.158358	0.160819	0.15959	0.968206	0.0961538	0.1489362	0.1212121	0.5965749	0.1501272	0.159383	0.1547315	0.883089
Feel uncomfortable/suffocated	0.067449	0.076023	0.071742	0.825834	0.0769231	0.0851064	0.0808081	0.8916435	0.0687023	0.0771208	0.07289	0.8264902
Bad smell of ITN	0.008798	0.017544	0.013177	0.457413	0.0192308	0	0.010101	0.3028653	0.0101781	0.0154242	0.0127877	0.638346
Shortage of Net	0.706745	0.669591	0.688141	0.698101	0.7115385	0.7021277	0.7070707	0.9506159	0.7073791	0.6735219	0.6905371	0.7309848
Small living room	0.029326	0.032164	0.030747	0.851035	0.0769231	0	0.040404	0.0470582	0.0356234	0.0282776	0.0319693	0.6197607
Use mosquito coil/spray	0.029326	0.04386	0.036603	0.588742	0.0192308	0.0638298	0.040404	0.2646785	0.0279898	0.0462725	0.0370844	0.4712651
Identified rules of net use												
Net has to be hanged before evening	0.234343	0.273292	0.253579	0.747097	0.3164557	0.443038	0.3797468	0.5058051	0.2456446	0.297153	0.2711268	0.6899795
Can't be washed more than two times in 6 months	0.044444	0.045549	0.04499	0.964324	0.0506329	0.0379747	0.0443038	0.6461501	0.0452962	0.044484	0.0448944	0.9718145
Can't be washed directly in ponds/river	0.052525	0.039338	0.046012	0.643946	0.0506329	0.0506329	0.0506329	1	0.0522648	0.0409253	0.0466549	0.6765784
ITN to be retreated every six months	0.082828	0.093168	0.087935	0.835932	0.1392405	0.1139241	0.1265823	0.7492534	0.0905923	0.0960854	0.0933099	0.9162481
All of the above	0.084849	0.035197	0.060327	0.269562	0.0506329	0.0379747	0.0443038	0.7291537	0.0801394	0.0355872	0.0580986	0.2946696
Don't know	0.50101	0.513458	0.507158	0.929534	0.3924051	0.3164557	0.3544304	0.6002466	0.4860627	0.4857651	0.4859155	0.9983215
Family member has died from malaria	0.412475	0.399194	0.405841	0.875435	0.4556962	0.3764706	0.4146341	0.5660179	0.4184028	0.3958692	0.4070873	0.7913425
Proportion of children sleeping under a net	0.387243	0.395497	0.391252	0.87784	0.422449	0.4375	0.4295699	0.9045633	0.3916667	0.4005464	0.3959656	0.8621585
Proportion of vulnerable family members sleeping under a net	0.381559	0.394761	0.38794	0.793597	0.4291667	0.4302721	0.4296825	0.9919837	0.3880145	0.3993037	0.3934464	0.8097934

No member has any schooling	0.011928	0.008016	0.00998	0.601836	0.0126582	0.0117647	0.0121951	0.9579745	0.0120275	0.0085616	0.0102916	0.6580098
At least one member with primary level schooling	0.250497	0.222445	0.236527	0.535779	0.1898734	0.2	0.195122	0.9173543	0.242268	0.2191781	0.2307033	0.6332655
At least one member with junior level schooling	0.017893	0.022044	0.01996	0.775176	0	0.0117647	0.0060976	0.3442074	0.0154639	0.0205479	0.0180103	0.6850589
At least one member with S1-S4 level schooling	0.5666	0.53507	0.550898	0.565189	0.5189873	0.5764706	0.5487805	0.5516055	0.5601375	0.5410959	0.5506003	0.7323346
At least one member with S5-S6 level schooling	0.119284	0.142285	0.130739	0.612882	0.2025316	0.1529412	0.1768293	0.5955293	0.1305842	0.1438356	0.1372213	0.7850579
At least one member with Bachelor level schooling	0.021869	0.042084	0.031936	0.251862	0.0506329	0.0235294	0.0365854	0.3803598	0.0257732	0.0393836	0.0325901	0.4018375
At least one member with Masters level schooling	0.011928	0.028056	0.01996	0.212632	0.0253165	0.0235294	0.0243902	0.9542854	0.0137457	0.0273973	0.0205832	0.2933899
Aggregated poverty scores												
Type of roof	6.49503	6.578629	6.536537	0.691773	6.556962	6.4	6.47561	0.5937239	6.503436	6.552496	6.527945	0.7984038
Type of wall	9.968127	10.29435	10.13026	0.488684	9.769231	10.48235	10.1411	0.2739692	9.941379	10.32186	10.13178	0.4062144
Number of children	5.738095	5.809619	5.773679	0.914004	7.265823	7.741176	7.512195	0.5892444	5.945111	6.090753	6.017995	0.8276697
HH head education level	3.884921	3.8998	3.892323	0.926766	3.987342	4.211765	4.103659	0.4192138	3.898799	3.945205	3.922022	0.7850113
fuel used	6.264	8.834356	7.534884	0.104686	7.263158	9.035294	8.198758	0.3473812	6.395833	8.864111	7.627826	0.112205
own tvradio	2.552	2.399594	2.476334	0.671851	2.857143	2.952941	2.907407	0.8438587	2.592721	2.480969	2.536797	0.7641084
Number of meals eaten in HH	12.7515	13.22672	12.98792	0.558723	12.12987	12.29762	12.21739	0.9134052	12.6684	13.0917	12.88042	0.6289902
2 pairs of clothes/member	10.19277	10.26122	10.22672	0.901119	10.12987	9.741176	9.925926	0.702371	10.18435	10.18435	10.18435	1
1 pair of shoes/member	8.694611	8.963783	8.828657	0.7187	9.730769	8.614458	9.15528	0.2210797	8.834197	8.913793	8.874029	0.9095674
What they did when ran out of salt	6.767535	7.258656	7.011111	0.529965	7.051282	7.117647	7.08589	0.9567219	6.805893	7.237847	7.021683	0.5441727
Disaggregated poverty measures												
Thatch/straw roof	0.083499	0.064516	0.074074	0.51742	0.0632911	0.0941176	0.0792683	0.4400701	0.080756	0.0688468	0.0748065	0.6502923
Iron Sheet/tin roof	0.900596	0.929436	0.914915	0.325485	0.9367089	0.8941176	0.9146341	0.2895822	0.9054983	0.9242685	0.9148753	0.4668133
Cement/Other roof	0.015905	0.006048	0.011011	0.114474	0	0.0117647	0.0060976	0.3442074	0.0137457	0.0068847	0.0103181	0.2590376
B. Bricks w/ cement	0.650099	0.701613	0.675676	0.506542	0.6025641	0.7294118	0.6687117	0.25897	0.6437177	0.7056799	0.6746988	0.4133492
B. Bricks w/ mud	0.119284	0.120968	0.12012	0.963781	0.1538462	0.1058824	0.1288344	0.2985267	0.1239243	0.1187608	0.1213425	0.8841573
Mud poles and others	0.218688	0.171371	0.195195	0.555393	0.2435897	0.1647059	0.202454	0.4173172	0.222031	0.1703959	0.1962134	0.501872
Unburnt bricks	0.00994	0.006048	0.008008	0.627214	0	0	0	.	0.0086059	0.0051635	0.0068847	0.6206178
Four or more children	0.002	0.004132	0.003049	0.637008	0	0.0238095	0.0123457	0.2805363	0.0017301	0.0070423	0.004363	0.2953526
Three children	0.17	0.179752	0.174797	0.77598	0.1794872	0.0714286	0.1234568	0.1130572	0.1712803	0.1637324	0.1675393	0.8217482
Two children	0.248	0.270661	0.259146	0.621752	0.1923077	0.1428571	0.1666667	0.4265445	0.2404844	0.2517606	0.2460733	0.8017914
One child	0.26	0.223141	0.24187	0.19792	0.1794872	0.297619	0.2407407	0.1256375	0.2491349	0.2341549	0.2417103	0.6123756
No children	0.18	0.159091	0.169715	0.45994	0.1153846	0.1904762	0.154321	0.2145264	0.1712803	0.1637324	0.1675393	0.7822696
HH head: no grade completed	0.068	0.075356	0.071645	0.744921	0.0512821	0.0595238	0.0555556	0.7990561	0.0657439	0.0730435	0.0693842	0.7114226
HH head: Primary	0.38	0.344196	0.36226	0.554712	0.3589744	0.2380952	0.2962963	0.1716096	0.3771626	0.3286957	0.3529922	0.4302895
HH head: Above primary	0.552	0.574338	0.563068	0.728949	0.5769231	0.702381	0.6419753	0.2382722	0.5553633	0.5930435	0.5741544	0.5806537
Firewood	0.477046	0.262195	0.370594	0.103891	0.3896104	0.2470588	0.3148148	0.3622045	0.4653979	0.2599653	0.3627706	0.1128568
Charcoal/Paraffin etc.	0.520958	0.731707	0.625378	0.10635	0.5974026	0.7529412	0.6790123	0.3175158	0.5311419	0.7348354	0.6329004	0.111369
Have TV/Radio/Cassette	0.002	0	0.001007	0.315321	0	0	0	.	0.0017331	0	0.0008658	0.3157629
Do not have TV/Radio/Cassette	0.632	0.598377	0.615307	0.709443	0.7142857	0.7294118	0.7222222	0.9046845	0.6429809	0.6176471	0.630303	0.7868703
One meal/day	0.074148	0.030364	0.052367	0.03639	0.0519481	0.0357143	0.0434783	0.56173	0.0711806	0.0311419	0.0511265	0.0501441
Two meals/day	0.162325	0.188259	0.175227	0.79289	0.2987013	0.3095238	0.3043478	0.9549629	0.1805556	0.2058824	0.1932409	0.8167001
Three+ meals/day	0.763527	0.779352	0.7714	0.880315	0.6493506	0.6547619	0.6521739	0.978771	0.7482639	0.7612457	0.754766	0.9101229
Everyone has two sets of clothes	0.844311	0.851626	0.847936	0.874988	0.8333333	0.8117647	0.8220859	0.8035684	0.8428325	0.8457539	0.8442907	0.9477647
Not everyone has two sets of clothes	0.149701	0.144309	0.147029	0.905853	0.1538462	0.1882353	0.1717791	0.6820593	0.1502591	0.1507799	0.150519	0.9904829
Everyon has one pair of shoes	0.790419	0.814889	0.802605	0.7187	0.8846154	0.7831325	0.8322981	0.2210797	0.8031088	0.8103448	0.8067299	0.9095674

Not everyone has one pair of shoes	0.209581	0.185111	0.197395	0.7187	0.1153846	0.2168675	0.1677019	0.2210797	0.1968912	0.1896552	0.1932701	0.9095674
Borrowed/did without salt	0.383234	0.340122	0.361895	0.544058	0.3589744	0.3529412	0.3558282	0.9567219	0.3799655	0.3420139	0.361039	0.5572468
Purchased salt	0.612775	0.659878	0.636089	0.506068	0.6410256	0.6470588	0.6441718	0.9567219	0.6165803	0.6579861	0.6372294	0.5217679
Chronic Deficit	0.11	0.157576	0.133668	0.297367	0.1282051	0.1294118	0.1288344	0.984882	0.1124567	0.1534483	0.1329879	0.3159352
Deficit	0.69	0.670707	0.680402	0.809485	0.6282051	0.5647059	0.595092	0.5835056	0.6816609	0.6551724	0.6683938	0.741516
Breakeven	0.16	0.127273	0.143719	0.633794	0.2051282	0.2823529	0.2453988	0.5758569	0.16609	0.15	0.1580311	0.8317265
Surplus	0.04	0.044444	0.042211	0.876887	0.0384615	0.0235294	0.0306748	0.7220899	0.0397924	0.0413793	0.0405872	0.9560013
Very poorer	0.076	0.132797	0.104313	0.117217	0.0769231	0.2117647	0.1472393	0.0836675	0.0761246	0.1443299	0.1103448	0.047221
Poorer	0.376	0.354125	0.365095	0.824792	0.4102564	0.2117647	0.3067485	0.0262341	0.3806228	0.3333333	0.3568966	0.6084905
Same	0.308	0.277666	0.292879	0.726313	0.3846154	0.3294118	0.3558282	0.7079323	0.3183391	0.2852234	0.3017241	0.712975
Better	0.184	0.195171	0.189569	0.850833	0.1025641	0.2352941	0.1717791	0.0633448	0.1730104	0.2010309	0.187069	0.6124297
Much better	0.056	0.040241	0.048144	0.623949	0.025641	0.0117647	0.0184049	0.5251666	0.0519031	0.0360825	0.0439655	0.5957081

Appendix C: Poverty Score Index

Poverty Indicator	Score
Type of roof	
Thatch/Straw	0
Iron sheet/tin	7
Cement/other	12
Type of wall	
Burnt bricks with cement	12
Burnt bricks with mud	7
Mudpoles and others	6
Unburnt bricks	0
Number of children	
Four or more	1
Three	3
Two	6
One	7
None	15
HH head education level	
No grade completed	0
Primary	3
Above primary	5
fuel used	
Firewood	0
Charcoal/Paraffin	12
own tvradio	
Yes	4
No	0
Number of meals eaten in HH	
One	0
Two	8
Three or more	15
2 pairs of clothes/member	
Yes	12
No	0
1 pair of shoes/member	
Yes	11
No	0
What they did when ran out of salt	
Borrowed/Did without	0
Purchased More	11
Self-Described Poverty based on income/expenditures*	
Chronic Deficit	0.5
Deficit	1
Breakeven	1.5
Surplus	2
Self-Described poverty based on neighbor's poverty status*	
Very Poorer	0.5
Poorer	1
Same	1.5
Better	2
Much Better	2.5

* These indicators were only in the follow-up survey, and are not used in any baseline poverty measures.

Appendix D: Malaria Knowledge Index

Knowledge Indicator	Score
When does family hang bednets?	
Hangs net before evening	4
Hangs net at evening	3
Hangs net after evening	1
Hangs net before sleep	1
Rules of net use identified by respondents	
At least one correct answer	2
Identified three correct answers	3
Identified all four correct answers	4
Did not know	0
Entire Household used bednet previous night	
Yes	4
No	0

Additional Malaria Knowledge Indicators

What can people do to prevent malaria?

Score is a sum of points for each answer given (max possible is 5)

Regular net use	1
Improved sanitation/food	0
Clean bushes	1
Remove stagnant water	1
Use mosquito coils or insect spray	1
Preventative meds for preg. women	1

Appendix E: Tests of Representativeness

	Busia					Mukono					East Central				
Variable	all5	DHSurvey5	BRAC5	ttest5	pvalue5	all2	DHSurvey2	BRAC2	ttest2	pvalue2	all4	DHSurvey4	BRAC4	ttest4	pvalue4
hhsize	5.137	5.118	5.182	0.277	0.782	4.638	4.602	4.748	0.562	0.574	5.112	4.855	5.281	3.061	0.002
tvradiocassette	0.536	0.548	0.505	-0.982	0.326	0.818	0.776	0.946	4.694	0.000	0.630	0.707	0.578	-4.398	0.000
wall_1 (b. brick w/ cement)	0.279	0.150	0.583	12.324	0.000	0.578	0.486	0.849	8.093	0.000	0.547	0.364	0.665	10.202	0.000
wall_2 (b.bricks w/ mud)	0.061	0.079	0.016	-3.061	0.002	0.066	0.039	0.144	4.472	0.000	0.166	0.200	0.144	-2.402	0.016
wall_3 (mudpoles and others)	0.589	0.669	0.401	-6.432	0.000	0.341	0.454	0.007	-10.777	0.000	0.216	0.274	0.179	-3.746	0.000
wall_4(u. bricks)	0.072	0.102	0.000	-4.602	0.000	0.016	0.021	0.000	-1.759	0.079	0.071	0.162	0.012	-9.861	0.000
roof_1(thatch/straw)	0.286	0.335	0.166	-4.367	0.000	0.126	0.157	0.034	-3.905	0.000	0.130	0.240	0.057	-9.201	0.000
roof_2(iron/tin)	0.700	0.648	0.829	4.624	0.000	0.853	0.819	0.959	4.209	0.000	0.855	0.744	0.929	8.895	0.000
roof_3 (cement/other)	0.014	0.018	0.005	-1.193	0.233	0.020	0.025	0.007	-1.323	0.186	0.014	0.016	0.014	-0.322	0.748
chcnt_1 (4+)	0.187	0.195	0.168	-0.770	0.441	0.195	0.183	0.231	1.243	0.214	0.192	0.231	0.165	-2.722	0.007
chcnt_2 (3)	0.198	0.184	0.234	1.434	0.152	0.146	0.141	0.161	0.586	0.558	0.244	0.177	0.289	4.283	0.000
chcnt_3 (2)	0.228	0.206	0.283	2.105	0.036	0.214	0.197	0.266	1.751	0.081	0.209	0.184	0.227	1.715	0.087
chcnt_4 (1)	0.170	0.179	0.147	-0.996	0.320	0.163	0.166	0.154	-0.329	0.742	0.189	0.202	0.181	-0.874	0.382
chcnt_5 (0)	0.217	0.236	0.168	-1.888	0.059	0.283	0.313	0.189	-2.889	0.004	0.165	0.206	0.138	-3.001	0.003
net	1.115	1.129	1.081	-0.496	0.620	0.794	0.705	1.069	3.346	0.001	1.093	0.798	1.290	6.066	0.000
totalnetuse	0.206	0.177	0.278	2.840	0.005	0.196	0.101	0.504	11.561	0.000	0.199	0.150	0.232	3.356	0.001
fuel_1 (firewood)	0.679	0.921	0.077	-35.634	0.000	0.592	0.760	0.090	-17.616	0.000	0.606	0.749	0.515	-7.889	0.000
fuel_2 (charcoal)	0.321	0.079	0.923	35.634	0.000	0.408	0.240	0.910	17.616	0.000	0.394	0.251	0.485	7.889	0.000
HH head sex	0.777	0.777	0.776	-0.023	0.981	0.606	0.687	0.345	-7.523	0.000	0.689	0.728	0.664	-2.264	0.024
HH head age	42.914	45.615	36.280	-6.662	0.000	38.869	40.711	33.265	-5.096	0.000	39.634	39.238	39.901	0.765	0.444

Appendix F: Bibliography

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