Author's response to reviews

Title: Transition overtime in household latrine use in rural Bangladesh: a longitudinal cohort study

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Version: 7 Date: 14 June 2014
Author's response to reviews: see over

Reviewer's report
Title: Transition in households’ latrine use over time in rural Bangladesh: a longitudinal cohort study
Version: 6, Date: 31 December 2013
Reviewer: Benjamin Arnold
Reviewer's report:
This study summarizes the results of a large, longitudinal cohort followed in rural Bangladesh to measure the change in sanitation conditions observed during a program implemented by BRAC. The study makes an important contribution to the literature documenting the current state of sanitation conditions in rural Bangladesh, and the changes that take place over time. The authors have improved the manuscript in their revision. There are still a number of areas where the authors need to improve the clarity of their reporting that I have summarized below.

MAJOR COMPULSORY REVISIONS

(1) The manuscript still has numerous grammatical mistakes throughout and needs to be edited by a native English speaker. The quality of English is poor enough that it is not suitable for publication without extensive editing, in my opinion (though I defer to the editors).

Editorial assistance for the manuscript was taken from the Nextgenediting Global Initiative (www.nextgenediting.com) based in United Kingdom. Corrections were made from page 1 to page 23 including attached figures and tables.

(2) Table 4
The effects of Survey Year and Household Economic status would be more clear if the authors used a single reference category (e.g., baseline and ultra-poor). For example, with survey round the analysis should use a single reference year (baseline, 2006), and then compute the Risk Ratio for sanitary latrine use for midline and endline against baseline. The analysis currently compares midline against a combination of baseline and endline, which in my opinion does not make sense. The revised analysis would be easily accomplished using a categorical factor variable for survey round equal to 0, 1, 2 for baseline, midline, and endline. The analogous approach should be used for household economic status (single reference group).

The log-binomial regression analysis has been revised according to the reviewer’s comments. Please see the revised Table 4 attached with the manuscript.

(3) Discussion. Page 17
“A major limitation of this study is the absence of control groups which may trigger the questions whether the changes in sanitation situation is because of WASH interventions. However, inclusion of baseline survey prior to the WASH intervention and visiting the same households in midline and end line surveys allow that the changes are due to the intervention.” As I noted in my first review of this article: The second sentence that claims that changes in latrine use over time can be attributed to the WASH program because the same households were visited over time. However, without a control group, there is no way to identify what would have happened under a counterfactual scenario of no intervention. The changes observed could under-estimate or over-estimate the effect of the program on latrine use and conditions. If the authors are willing to assume that in
the absence of the program there would be no change in toilet conditions in this population then the changes would be 100% attributable to the intervention, but that is a very strong assumption and the authors should clarify whether they are willing to make this assumption, and if so, what their justification is for making it.

Update: The authors have not changed this text in the latest revision and I think that they need to incorporate nuance to their interpretation for this to be a scientifically defensible position to take. Specifically, they need to be explicit about their assumption. For example, the second sentence could be revised along the lines of:

“Our attribution of all changes in latrine coverage and use to the BRAC program makes the assumption that in the absence of the program there would be no change in toilet conditions in this population over the study period. Without a control group, we cannot definitively conclude whether the estimated effects are an over-estimate or an under-estimate of the program’s impact. However, we expect that the differences between survey rounds are a reasonable estimate of the program’s effect on sanitation conditions because…” (then go into the couple of anecdotal observations that conclude the paragraph)

The second sentence has been revised following the lines of reviewer. Please see the last paragraph of ‘discussion’ on page numbers 17 and 18 of the manuscript.

MINOR ESSENTIAL REVISIONS

(4) In the Title, “households’” should be replaced by “household”

In the title, correction has been made by replacing proper word. Please see ‘the title’ on page number 1 of the manuscript.

(5) Abstract, Conclusion
This sentence in the conclusion does not make sense as written:
“Awareness rising about sanitary latrine use and periodic home visits to monitor usage and maintenance of latrines are required to consider for improvement.”

Correction was done by deleting this sentence in the ‘conclusion’ of abstract (Please see page number 3).

(6) Introduction, Page 4
“Therefore, unhygienic sanitation practices threaten public health spreading diarrhea, typhoid and other diseases by bacteria in feces.” The word “bacteria” should be replaced by pathogens – many fecal-oral enteric pathogens are not bacteria (e.g., rotavirus, Cryptosporidium, …).

Introduction: The word “bacteria” was replaced by the word “pathogens” (please see page number 4, second paragraph (line number 6) of the “background” section).

(7) Methods, Page 8
“A total of 29,985 households at baseline, 29,885 households at midline and 26,404 households at end line were interviewed. The reasons for missing households at follow-up surveys were unavailability, migration or death”. This information should be reported in the Results (not Methods), following the STROBE guidelines:


The information was reported in the results section following the STROBE guidelines. (Please see “background characteristics of the study samples” under the results section on the page number 11)

(8) Methods, Page 10
This sentence: “Chi-square was used to compare the differences between indicator values. The significance level denoted by p-value in statistical tests was considered less than or equal to 0.05.” should be moved to the Statistical Analysis Section. But, I doubt that a Chi-square test is appropriate given the study design and so this text should probably simply be deleted (see next comment).

The sentence has been deleted following the comments by reviewer.

(9) Methods, Page 10
“Chi-square was used to compare the differences between indicator values.” This is vague and it is unclear what tests the authors conducted. Furthermore, given the non-independence of study households over time and within village, a Chi-Square test would be inappropriate in this setting (the test assumes independent observations). If the authors are describing tests between latrine use at different rounds, then they should use the same statistical methods that they used for their predictive association analyses -- namely log-binomial regression with robust standard errors to account for clustering -- using survey round as the exposure of interest. It appears the authors have done this in Table 4 (though see comment #2, above), so it’s unclear why the Chi-Square test would be needed.

The sentence has already been deleted following the comments by reviewer.

(10) Methods, Page 10
“Adjusted log-binomial model was used to compute relative risk (RR).” The authors should cite an article to support their chosen analysis method. For example:

An article was cited to support chosen analysis method. Please see statistical analysis section on the page number 10.

(11) Methods, Page 10
The study included 30 villages in each upazilla. It is well known that toilet use outcomes could be correlated at the village level. The analysis should account for village level clustering in their outcomes using robust standard errors. Did the analysis include this type of standard error calculation? If not, the authors need to revise their analysis. If it did use robust standard errors, then the authors need to explain that in their statistical methods section.

Use of robust standard error was explained in statistical analysis section (Please see last line of the statistical analysis section on the page number 10). Robust standard error was also mentioned in the revised Table 4.

(12) Methods, Page 10
This sentence does not make sense as written: “The analysis helped determine if exposure to the factors had an effect over the outcome.”

The sentence was deleted.

(13) Methods, Page 10
The section “Operational Definition of a Sanitary Latrine” should be moved to an earlier position in the methods section (before the analysis description). I suggest the authors follow the STROBE guidelines for organizing the manuscript (citation above).

The section “operational definition of a sanitary latrine” was moved to the methods section before the analysis description. (Please see page 8)

(14) Table 3.
The authors have improved Table 3 from the first version, but it is still fairly confusing. For example, the first column sums to 100%, but then the table changes to row percentages so the columns no longer sum to 100%. I found this very disorienting. I think that the Table legend should more clearly describe what the tables contain, and if journal guidelines permit it, a vertical line or some other delimiter should separate the baseline values from the other values in the table to clearly separate the different types of information.

Necessary changes were made in Table 3 to make it clearer. Please see revised Table 3 attached with the manuscript.

Reviewer’s report
Title: Transition in households’ latrine use over time in rural Bangladesh: a longitudinal cohort study
Version: 6 Date: 2 January 2014
Reviewer: Abera Kumie
Reviewer’s report:

All my comments were accommodated well. I have small worries on the analysis: how well the clustering effect and the timing were handled: ie how controlled the effect of that specific village where a given HHs is belonged. One of the reviewers had this concern and the analysis was modified using adjusted log-binomial model.

There is still a need to consult a biostatistician if a mixed model (multi level analysis) is appropriate or not. This model is useful to control the within (time) and between effects (cluster or villages).

Pls make copy of this to other reviewers.

The comment has already been addressed through other reviewer’s comments.