Author’s response to reviews

Title: Effect of play-based family-centered psychomotor/psychosocial stimulation on the development of severely acutely malnourished children under six in a low-income setting: a randomized controlled trial

Authors:

Teklu Gemechu Abessa
(teklugem@yahoo.com; teklu.gemechu@ju.edu.et; teklu.gemechuabessa@uhasselt.be)

Berhanu Worku (brexnigussie83@yahoo.com)

Mekitie Kibebe (mwondafrash@yahoo.com)

Tsinuel Girma (tgirma@hsph.harvard.edu)

Johan Valy (johan.valy@pxl.be)

Johan Lemmens (johan.lemmens@pxl.be)

Liesbeth Bruckers (liesbeth.bruckers@uhasselt.be)

Patrick Kolsteren (patrick.kolsteren@UGent.be)

Marita Granitzer (marita.granitzer@uhasselt.be)

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Author’s response to reviews:

College of Education and Behavioural Sciences

Jimma University

P.O. Box 378, Jimma, Ethiopia

Tel:+251 917 804 540

teklu.gemechu@ju.edu.et; teklugem@yahoo.com

28 December 2018
Dr. Akila Sridhar  
(On behalf of Dr. Maria Zalm)  
Editor of ‘BMC Pediatrics’

RE: BPED-D-18-00348 “Effect of play-based family-centered psychomotor/psychosocial stimulation on the development of severely acutely malnourished children under six in a low-income setting: a randomized controlled trial”

Teklu Gemechu Abessa, Berhanu Nigussie Worku, Mekitie Wondafrash, Tsinuel Girma, Johan Valy, Johan Lemmens, Liesbeth Bruckers, Patrick Kolsteren and Marita Granitzer

Dear Dr. Akila Sridhar,

In your e-mail communication on 28 Nov 2018, you have offered us a possibility to reconsider our revised manuscript once we address issues raised by two reviewers. We would like to extend our sincerest thanks to you and the reviewers for the very constructive comments and suggestions for further improvement of our manuscript entitled “Effect of play-based family-centered psychomotor/psychosocial stimulation on the development of severely acutely malnourished children under six in a low-income setting: a randomized controlled trial (BPED-D-18-004348)”.

Following your request, we have outlined in this letter each of the (point by point) changes made based on the comments of you and the reviewers. In case we made no changes, we provided a suitable rebuttal to the comments raised. We have also included a ‘clean’ copy of our revised manuscript, along with a copy that includes all revisions highlighted in track-changes.

We sincerely thank you for this opportunity and look forward to hearing from you regarding our submission. We would be glad to respond to any further questions and comments that you may have.

Sincerely,

Teklu Gemechu Abessa
On behalf of all authors.

Technical Comments from editor:

Please clearly outline the limitations of the study within the 'Conclusions' of the main manuscript.

1.  Authors’ reply: Thank you for your critical observation and suggestion to outline the limitation of the study under “Conclusion” section. It seems that the editor is concerned about the validity of the conclusion in the presence of such limitations. We have already included the main limitations that might have influenced outcomes of the study (the lack of access to an adequate and balanced diet at home, non-intensive stimulation, the short follow-up period, and non-adherence by caregivers to strictly implement the home-based stimulation). However, we are afraid that listing all the limitations under the “Conclusion” creates the impression that the limitations are also parts of the conclusion. We therefore feel that keeping this section as is it is now seems more appropriate.

Reviewers reports:

(Frances E. Aboud (Reviewer 1): BPED-D-18-00348 “Effect of play-based family-centered psychomotor/psychosocial stimulation on the development of severely acutely malnourished children under six in a low-income setting: a randomized controlled trial”

I read an earlier version of this submission. At the time, I felt the revisions could easily be handled by the authors. So I am happy to see that authors were offered a re-submission. This revision is much improved and my questions have all been answered appropriately. The paper is ready to be published after attending to the two small comments listed below. It is very important to publish the effects of programs with very malnourished children, especially ones that provide nutrition and stimulation services. I sincerely hope that the authors will continue to offer improved services and conduct rigorous evaluations of outcomes and of the implementation process. The WHO is formulating recommendations on these issues.

2.  Authors’ reply: We thank the reviewer for the constructive comments and suggestions. We would be considering to undertake an evaluation based on an improved services.

Table 4. One of the effect sizes for Language is 0.45003. Can this be shortened to 0.45?

3.  Authors’ reply: Thank you very much! We have corrected as per your suggestion.

Table 4. Under MUACZ, an effect size is written <0.001. This notation is used for p values, but the authors might write an effect size of 0.00 if it is indeed <0.001.

4.  Authors’ reply: Thank you very much again! We have corrected as suggested.
Reviewer 2 (Reviewer 2):

PEER REVIEWER ASSESSMENTS:

OBJECTIVE - Full research articles: is there a clear objective that addresses a testable research question(s) (brief or other article types: is there a clear objective)?

Yes - there is a clear objective.

DESIGN - Is the current approach (including controls and analysis protocols) appropriate for the objective?

Not sure - key details are missing from the manuscript.

5. Authors’ reply: We have now added some more descriptions and clarifications under methods section.

EXECUTION - Are the experiments and analyses performed with technical rigor to allow confidence in the results?

Not sure - key details are missing from the manuscript.

6. Authors’ reply: We have now added more details and clarification.

INTERPRETATION - Is the current interpretation/discussion of the results reasonable and not overstated?

No - there are minor issues.

OVERALL MANUSCRIPT POTENTIAL - Could an appropriately REVISED version of this work represent a technically sound contribution?

Maybe - with major revisions

7. Authors’ reply: We have thoroughly integrated the comments of the reviewer point by point and therefore feel that the manuscript is much more improved to be reconsidered.

GENERAL COMMENTS: This study was aimed at examining the effect of play-based stimulation on the development, linear growth, and nutritional outcomes during hospital and home-based treatment of SAM children under six years of age in the low-income context of Jimma Zone, South West Ethiopia. The study design and execution of the study seem appropriate. There are however a few major (e.g. high loss to follow-up and 70% of children not receiving the aimed treatment intensity, sample size calculation) and many minor (unclear model and results presentation, unclear interpretation, too few detail) that made it difficult to judge the soundness of the study.
8. Authors’ reply: The comments of the reviewer were addressed one by one. We have now edited and added more clarifications in the manuscript for the reader to judge the soundness of the study. The high loss to follow-up has been admitted and reported. This study has been conducted in a very complex situation and real life situations. Hence, it was not possible to undertake it in line with the scientific rigor followed in designing it. Nonetheless, the challenges encountered are informative and are important lessons in planning future studies in such contexts. On the other hand, in recognition of the limitation arising from high loss to follow-up, we have considered data analysis techniques that were not included originally in the methods section. Accordingly, we considered different statistical analysis approaches (complete case analysis, direct likelihood and multiple imputation) to address this limitation. The fact that the treatment intensity was less than what was planned is a limitation but it also unveils the actual situation and contextual difficulties in implementing ideal suggestions and recommendations such as those given by WHO. The issue of sample size is also essential but the cost implication to enroll and follow-up a huge number of such study participants a big challenge. Following up each subject in his/her real home contexts, among complex physical and social environments makes the feasibility of conducting such a study with a perfect scientific vigor questionable. However, the conclusions drawn in the study were made by taking these limitations into account.

REQUESTED REVISIONS:

Methods: p.5, line 7: "single blind" is not clear without explanation, can be participants or investigators, please specify.

9. Authors’ reply: Thank you for your observation. After “single blind”, we have now added in brackets the following phrase for clarification: “data collectors not knowing the treatment group of participants”.

Sample size calculation, p.6, top: "estimates of the variance ...." I am very much questioning the approach to estimate the variance in development performance ratio of 22 healthy children. First, ratio scores are likely to be very different in severely malnourished than in healthy children, so I would expect the variance to be very different as well. Second, 22 is a very low sample size. Third, you would need to give more information on these children, e.g. are these Ethiopian children as well, age range etc.

10. Authors’ reply: Thank you again for your observation. We have already indicated that the variance calculated based on healthy children does not show the variance in severely malnourished children. The 22 children were healthy Ethiopian children living in the study area, with a mean ±SD age of 51±12.5 months, range 26-69 months. These children were not selected as part of the study design. Therefore, their number was not determined taking the issues of sample size and statistical power into account. The data from these 22 healthy Ethiopian children were used simply because we had no child development data collected using a culturally adapted
developmental assessment tool from children of the same study context. The manuscript is now edited for clarity by addressing the issues raised.

The authors add 25% to each group but there is no reasoning why 25% and not 50, 75 or 100%. I think there is a considerable risk here that the study is significantly unpowered so that no real difference can be detected. Also why a 5% difference in development score (and not 10, 15 or 20%). There is no reasoning for this. Is this in line with previous evidence? Is 5% a clinically important difference?

11. Authors’ reply: The addition of 25% is just taking the issue of time and corresponding budget into account, not statistical reasoning. Since we knew that the study was underpowered, we did not expect to see bigger differences such as 10 or 15%. That is why we set the difference to be only 5%. Even though 5% is not a clinically important difference, we were of the thought that detecting such a difference between the two groups with such a simple intervention package and in such a difficult circumstance would be a big contribution.

p.6, line 24: "an interim analysis showed a larger variance for SAM children" - as in many many other places a reference is missing, more information on this analysis is needed; was it performed by the authors, sample size etc?

12. Authors’ reply: This was an interim analysis conducted by the authors, and therefore need no reference. For clarity, the statement is improved to read: “We conducted an interim analysis which showed a larger variance in developmental performance scores…”

p.6, lines 15-25: "However, the statistical power computed from the cross-sectional data is insufficient to power the group*time interaction effect examined in the longitudinal dataset. Ideally, a pilot study or an established literature is needed to determine a more accurate sample size. The present study lacks either of the two, and is a sort of 'hypothesis generating type'. " This is a discussion, not method part.

13. Authors’ reply: We fully agree with the reviewer, but feel that keeping this information uncovered until discussion makes the reader feel uncomfortable with the method to proceed further.

p.7, line 12: Three trained female nurses does not seem to be very much for 170 SAM intervention children. Please discuss your choice.

14. Authors’ reply: Of course it is not too much, but the three nurses had to work full time on the study. In addition to their monthly salary, the nurses had to be paid as an incentive as they had to work under very difficult circumstances such long distance travel on foot on alternating rainy and sunny days, slippery roads and scorching sun. On the other hand, employing more than three full-time nurses was costly.
The intervention nurses received one week of training in the theory of child development, and one month of intensive practice in implementing developmental stimulations. Who trained the nurses and what does "intensive" mean (how many days per week, how many hours. What was trained? Did training follow a specific scheme? Needs more detail.

Authors’ reply: Thank you for the observation. We have now edited the manuscript and described “intensive” in brackets to mean 4 hours daily, and also included the following statement to clarify who trained the nurses: “A play therapist, an occupational and a physiotherapist, special educator and a psychologist in consultation with a neuroscientist and nutritionists prepared the training package.”

Here and earlier: phases need to clearly defined at the beginning. Please also give their approximate lengths.

Authors’ reply: Thank you for this observation. To keep the length of the manuscript short, we linked a reader to a source. We have therefore included in the introduction section the following statement: “There are three phases of treatment of the SAM children, and the details are available in the protocol prepared by the Ethiopian Ministry of Health [20].”

Again more detail is needed: how were caregivers involved, did this follow some standard procedure or was this left to the study nurse to decide, how was ascertained that always the primary caregiver of the child was involved? what kind of information did caregivers receive: was this just verbal information by the nurses or did it include other information material, did nurses and caregivers always speak the same language or was there an established plan how to proceed if caregivers did not understand the study nurse?

A caregiver is identified as a primary caregiver if he/she usually stays with the child during the in-patient care. It could be a mother, a father or a grandparent or any other person more connected to the child. Whether or not the carer is a primary caregiver or not is ascertained while asking for consent to participate in the study. In most cases caregivers get verbal information since many of them are illiterates. With regard to stimulation, they are given practical demonstration and some home-take play materials as well. One of the preconditions for the selection of the intervention nurses was knowledge of the two major languages spoken in the study area. Therefore, there can be no problem in this regard. The manuscript is now edited and contains the following statement for clarification: “The intervention nurses who were familiar with the local cultures, and could speak the two major languages used in the area received one week of training in the theory of child development…”

Who developed the trainings and the training material? has this been assessed prior to the intervention by standard procedures?

Authors’ reply: Answer is given under #15 above. The intervention is guided by certain procedures that the nurses have to follow such as assessing situation of the child, following the
lead of the child. The key principle is ensuring safety, making the activities enjoyable and stimulating to the child.

p.9, lines 14-22: previously you said that those who assessed the child were blinded to the child's intervention status. However, here it seems to me that the nurses who delivered the intervention also assessed the child's development? Or was this still done by another person? Please explain and provide more detail.

19. Authors’ reply: To clarify this idea, we have now added after the subtitle ‘Outcomes and measurements’, the following statements: “Outcomes were assessed by nurses trained for these purpose as testers. The testers were blinded of the treatment group to which the SAM children belong, and the intervention nurses had no role in testing.”

p.9, starting line 53: Is the RUTF that the family of the SAM child is supposed to collect from the health centre free or does it need to be paid and if so how expensive. This would also be a point for the discussion. Another point for discussion would be if there are any reasons for a family to not include their SAM child in the supplementary feeding programme such as stigma, costs, travel or fear.

20. Authors’ reply: It is now edited that RUTF is given freely and there is no problem related to cost. However, experience has shown that stigma is not attached with receiving RUTF. Rather, families with children having no malnutrition even case want to have it because RUTF is valued as highly nutritious. The issue of distance, however, has been mentioned as can be seen from the following statement in the discussion section: “In some other cases, parents could not regularly collect the RUTF due to the lack of a health center in the vicinity.”

p. 10, top: Please give more information about the "Supplementary Feeding Programme"

21. Authors’ reply: This is about a general statement about the provision that a SAM child has to obtain according to the protocol for the management of SAM children. Supplementary Feeding Programme is usually targeted at vulnerable children and women and is not accessible for all. Therefore, such services are available only at selected sites. Hence, the statement is now deleted from the manuscript.

p.10, lines 47-50: has this adaptation been done for the purpose of this study? what happened to SAM children and their families if they did not speak one of the two languages? This should be treated earlier and in general: how were language barriers dealt with?

22. Authors’ reply: The adaptation of a tool for assessing developmental outcomes was done with this study in mind. One of the two languages is an official language of the country and the other was a regional language. Parents could speak either of the two or both, and we had not encountered any problem in this regard. For clarification, the phrase “commonly used in the study area” is now added after the phrase “two local languages.”
I don't understand the sentence. Are the "study caregivers" different from the mothers of the child?

Authors’ reply: Thank you for the observation. We have now dropped the phrase “the study”.

how was adherence of caregivers assessed? Direct observation? Self-report? If the latter, I believe there is a lot of potential for reporting bias.

Authors’ reply: There is no other option than asking for self-report. It is hardly possible to be with caregivers for direct observation on how often they play with their child and provide RUTF on daily basis. However, caregivers were advised to report honestly if they want the improvement of services for their child. It has been observed that most caregivers were even sharing their feelings on personal issues with intervention workers and testers.

"Some data on factors assumed to be affecting the performances within the intervention SAM children were gathered." you need to be much more specific: which factors? What does "some data" mean? Why were these factors chosen?

Authors’ reply: We think that the factors are explained in the next sentence as follows: “Information collected include caregivers’ feelings about the general health condition of the intervention SAM child, family support and engagement in the psychosocial stimulation service, the child’s access to nearby health centers after discharge from hospital, the and availability of RUTF.” These are not exhaustive list but only some of the factors thought to be important in affecting the intervention outcomes.

is a structured questionnaire really the best tool to describe the caregivers feelings? Can questionnaires be put as an appendix or be assessed upon request?

Authors’ reply: We understand that feelings cannot be assessed though questionnaire unless the respondent is honest. We were not interested in other feelings but only about the feeling related to health condition of the child. It is also mentioned in the manuscript that “Caregivers were asked to give their subjective rating (always, sometimes, rarely and never at all)”. There has been an observation guide about the mood of the child and that of the caregiver, but this article does not focus on reporting the qualitative aspect of the study. This questionnaires were completed by the intervention workers who were also using an observation checklist. Such observation checklist is available upon request.

I think there is considerable potential for reporting bias in if / how often the child was receiving RUTF.

Authors’ reply: We admit the possibility of bias. But we have always instructed caregivers to be honest in reporting for the sake improving services for their child. Since this follow-up information was collected by the intervention workers whom the caregivers really
liked because of the support for their child, we feel that they gave them honest report. We had a qualitative data which is beyond the scope of this article.

p. 12, line 10: again, phases need to be defined. What is the "transition phase"?

28. Authors’ reply: To keep the manuscript brief, we have linked a reference which describes the three phases of treatment as already mentioned under # 16 above.

have the authors considered employing an attention control intervention in which control families would have also been visited but without conveying intervention relevant messages or actions? please discuss.

29. Authors’ reply: There was no home visit for the control families except on the date of testing after six months. The purpose of the visit was to collect data on the developmental performances, and growth nutritional status of the SAM children.

p. 13, lines 1-2: Please justify your choice of the different variance/covariance matrices?

30. Authors’ reply: The working variance-covariance matrix was an unstructured matrix, except the personal-social outcome, for which an exchangeable structure was specified. A different variance-covariance matrix was used for the personal social because of error in convergence condition with an unstructured correlation structure was used. For clarification, we have now added the phrase “due to error in convergence condition”

"The time variable was used in a GEE model, where a group-specific curvilinear evolution over time was allowed", please describe the transformation of the time variable more in detail.

31. Authors’ reply: Curvilinear evolution of time was made by adding a quadratic term to the time variable and its interaction with group in the GEE model.

p. 13: I don't understand the difference between the models described initially and that did not contain further covariates with the separate analyses on the primary models including covariates. Were these the same models ones without and ones with covariates?

32. Authors’ reply: The initial description is a general information on the GEE model with and without covariates for all the outcomes. The latter description clarifies what theses explanatory variables (covariates) are, and provide the reader an additional information that each outcome was analyzed separately.

p. 13, last line: on which grounds (evidence, plausibility) where the two interventions chosen?

33. Authors’ reply: We think the question is about the two “interactions”, not “interventions”. Baseline age and baseline score were selected because exploratory analysis showed the importance of these variables.
pp. 13-14: I find it very difficult to assess how many variables (how many categories) and interactions were actually included in the final model. Please revise this section to make that clearer.

34. Authors’ reply: There are three steps in the model building. The first step deals with the basic model. The basic model includes group/treatment, time (linear term), time*time (quadratic term), treatment*time, treatment*time*time where “time” was analyzed in two ways: as a factor variable and as a continuous variable. In the second step, the basic model basic model was expanded by adding the following explanatory variables/covariates: child’s sex, child’s baseline age, baseline developmental score, baseline WAZ or baseline MUACZ scores (for primary outcomes) baseline anthropometric z-scores (for secondary outcomes), variables which differed significantly between the two groups at baseline (maternal occupation, residential area), baseline age*time, baseline score*time.

In the final model, the following terms were added to the model in second step: comprises the *treatment*time*sex, treatment*time*baseline age, treatment*time*baseline developmental score/treatment*time*baseline anthropometric z score.

p. 14, l. 5-7: your process of variable selection (Backward selection) is quite controversial and many researchers would recommend not using it. Please justify your choice as opposed to other methods e.g. based on prior evidence.

35. Authors’ reply: You are quite right. We had not very well established prior evidence as well to keep or remove a variable for this study. There are inadequate scientific evidences on the issue of psychosocial stimulation of SAM children, and many contextual factors that the authors felt important were included initially, but those which were found to be non-significant were dropped.

p. 14, l. 21-34: please give more context to this extra analysis. Why has it been performed? How was the matching done? How was data on the healthy children obtained? Where do these children come from?

36. Authors’ reply: The purpose of the intervention is to help SAM children catch up in their developmental performances with the healthy children. Therefore the purpose of this analysis was to see how much the SAM children improved in comparison with that of healthy, non-malnourished children. The matching was done by taking data of healthy children on which the developmental assessment tool used in this study was adapted and standardized. The children were non-malnourished, living in Jimma Town & belong to better family living condition. These non-malnourished children were matched in age with that of the SAM children, and their developmental performances compared on the performance of SAM children followed for six months. A reader can get details of the adapted tool and how it is suitable to use with the current study population from the linked reference.
RESULTS

p. 15, lines 51 onwards: the high loss-to-follow up of nearly 40% needs more extensive discussion. Is the study that was powered for a sample size with 20% loss to FU still able to find results? There is both a high loss to FU and a high percentage of children not receiving the planned amount of the intervention.

37. Authors’ reply: The study with a sample size with 20% loss to follow-up may not be able to find significant results for two reasons. First, the sample size with 20% loss to follow-up is not an adequate size given the nature of the scoring system used to measure the outcomes. Second, since the intervention was not conducted with the planned intensity, the effect of the intervention could be too small and non-significant. Yet, the fact that the plan was prepared under ideal condition and could not be strictly implemented in such complex context full of unforeseeable challenges is informative to design a more feasible future interventions. The high loss to follow-up was admitted, and a supplementary statistical analysis method addressing such a limitation is now reported briefly in discussion section (see more details under #45 below).

p. 15, lines 56-59: "The rate of loss to follow-up did not differ significantly between the two groups (42.4% control versus 33.1% intervention, p=0.08). » Even though the p-value is above 0.08, I still think that there is a difference in FU between intervention and control group, at least this is borderline.

38. Authors’ reply: The reviewer is perfectly right, but as far statistical inferences are concerned this may not have much influence on the final conclusion of the study. To give further elaboration, the following sentences are now added to the manuscript. “The high loss in the control group could be attributed to the lack of frequent contact and support. The home visits after discharge from hospital and provision of play materials might have encouraged more SAM children in the intervention group to remain in the study.”

p. 16: "However, the percentage of lost to follow-up is higher among males: 32.7% of the girls and 42.1% of the boys were lost to follow-up." Do you interpret this as a chance finding or can you give reasons for this finding? Should be taken up in discussion.

39. Authors’ reply: It is difficult to give valid interpretation from the data, and we thus suggest this issue to be investigated further. We have now included the following statement in the discussion. “The present study also showed that there is no significant difference in the effect of the intervention based on sex of child. But the reason for a higher loss to follow-up among boys than girls needs further investigation..”

p. 17, line 3: please give reasons why age matching was not possible.
40. Authors’ reply: We have now added the following clause to clarify the statement: “because there was a rare possibility for simultaneous enrollment of children of similar ages into hospital.”

p. 18, line 3: is the higher effect on GM in the intervention group meaningful when the two groups achieve basically the same scores at the home FU? is that picked up in discussion?

41. Authors’ reply: The two groups achieve similar score at a later time means that the intervention effect achieved earlier was not sustained. This has been discussed in terms of the non-significant effect at home arising from the lack of adequate diet and care at home compared to the services during the hospital stay period.

p. 18, first paragraph: I found it very difficult to locate the effect sizes and p-values given in the text in Table 4. please revise Table 4 to make it more understandable, it needs to be clearer with better structure. Additionally specify the following: is the effect listed under "control group" the effect that we would assume to see also in the intervention group given there was no intervention? why do you sometimes list two effects and sometimes only one? Specify what the effect size is. Is the p-value for the difference over time? It is also not clear to me why all these different models were needed (full model, reduced model, comprising time as factor or as continuous variable. Why did you not decide on one of the models and present results consistently from this one model? All this needs to be made much clearer.

42. Authors’ reply: The table summarizes the time effect and the intervention effect. We have organized the table based on comments from an earlier reviewer. A clarification has been provided in the table using the superscript “a” in table footnote. As you correctly said, the effect listed under "control group" is the effect of time, and show the change observed both on the control and the intervention group given there was no intervention (i.e., the effect of time). The “intervention effect” shows the difference in performance between the control and the intervention groups along with the p-value for the significance of the difference. Two effects were listed when both the full model and reduced model showed significant time effects. This has been marked by symbols used as superscripts of the parameter estimates. We have reported the result of using time as factor variable and as a continuous variable because we indicated under method section why it is important to use both approaches. There are limitations and strengths of using either approach. Findings from both approaches were presented in the table for the reader to make his/her own discretion when interpreting the results. This is in fact given as a comment in earlier review. With the basic GEE model, a full model was used to determine parameter of the intervention effect and to calculate the effect size. The p-value is for the difference over-time between the control and the intervention groups.

p. 18, line 33: please explain more clearly how you constructed this interaction, what does follow-up mean?
43. Authors’ reply: This part is already explained under method section and does not need to repeat it here. Therefore, this statement is deleted. We retained only a statement that describes a result in line with your comments under # 46 below to avoid the mixing up of method/result and discussion.

The same relates to Table 5: it needs to be much clearer and more intuitive, one (but not the only !) example are the interactions: you probably did not include them all at once in the model?

44. Authors’ reply: The construction of Table 5 is based on the suggested analysis presented in methods section. Explanatory variables were included all at once in the model. The presentation of both significant and non-significant terms in the table was based on the comments from earlier reviewers.

p. 18, line 48/49: it is confusing that here again a different model is mentioned which I believe has also not been described previously in methods.

45. Authors’ reply: This part is now deleted. The following statements are added in discussion section: “There was a high loss to follow-up in the study. Hence, different statistical approaches other than those initially planned were applied to examine if there was a significant influence from the high loss-to-follow-up. The findings from three methods (complete case analysis, direct likelihood and multiple imputation) were compared using linear mixed model, and showed the same conclusion.”

ADDITIONAL REQUESTS/SUGGESTIONS:

The manuscript lacks a lot of clarity; tables and figures need revision to make them clearer; many statements without proper referencing; no strict distinction between methods/results/discussion etc.

46. Authors’ reply: Thank you for your critical comments. We have now edited and made proper references to tables; removed or moved some statements to a section where they rightly belong.

Editorial Policies

Authors’ reply: Editorial polices have been read and the manuscript has been prepared accordingly.