Author's response to reviews

Title: The effect of motivational interviewing on oral healthcare knowledge, attitudes and behaviour of parents and caregivers of preschool children: An exploratory cluster randomised controlled study

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Author's response to reviews: see over
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The Editor

BMC Oral Health

Dear Sir/ Madam,

RE: The effect of motivational interviewing on oral health knowledge, attitudes and behaviour of parents and caregivers of preschool children compared to traditional dental health education: An exploratory cluster-randomised controlled study.

We are submitting the above-mentioned revised manuscript for consideration by *BMC Oral Health*.

In this resubmission, we have attempted to address all the comments and suggestions made by reviewer 1 and 2.

These comments and our response is listed below.

Sincerely

Dr. Rahul Naidu
RESPONSE TO REVIEWER 1:

Major compulsory revisions

1. Aim

The aims and objective of the study are clearly stated, but the rationale for their study should be better described; e.g. how their study is innovative and/or contributing to the existing literature on MI and dental health.

The rationale for the study has been developed in a revised background and introduction pages 3-6 (lines 63-135).

The title and conclusion implies that this study is intended a ‘feasibility’ study on the potential usefulness of MI, but this is not reflected in the introduction and aim. The authors should therefore describe the exploratory nature of their study in the introduction. The word ‘exploratory’ or ‘pilot’ would have the preference over ‘feasibility’, as a feasibility study also suggest an evaluation of technical feasibility (e.g. implementation) and cost-effectiveness of a program.

The word ‘feasibility’ has been replaced by ‘exploratory’ in the Background, Aim and Title of the study.

Minor point: The aim in the introduction should also include ‘knowledge’ in line with the title and aim in the abstract.

The title and aim of the study have been amended to include ‘knowledge’.

2. MI literature

The authors refer to three studies that have shown promise in improving oral health of preschool children, but there are also controversial studies that showed a null-effect. These studies need to be reviewed and also incorporated.

A recent study of an MI intervention for preschool study reporting negative findings (Ismail et al 2011) a systematic review of MI and oral health (Gao et al 2013) and recommendations from clinical guidelines (SIGN) have been included in the introduction (page 5 , lines 116-125).

3. Study design

The design of the study should be stated in the abstract and methods section (e.g. exploratory cluster-randomized controlled trial and semi-structured focus groups) in addition to ‘quantitative and qualitative methods’.

The design ‘exploratory cluster-randomized controlled study’, has now been stated in the title, abstract and aim of the study.
4. Sample size and power calculation

The authors should include a power calculation of the necessary sample size. Questions that need to be answered are ‘Why were only 6 preschools chosen?’ and ‘Was the sample of 79 participants sufficient for the objectives of this study?’

The explanation for the inclusion of only six preschools is now included on page 7 (lines 157-161).

A post-hoc power analysis is now presented on page 17 (line 386).

5. Response rate

Information should be provided on total number of parents that were initially approached to participate and subsequently, the response rate should be given.

Response rate in now included on page 17 (lines 382-383). The low response and study power are discussed in the ‘Limitations of the study’ page 26-27 (lines 607-612).

6. Mode of delivery of the intervention

More details should be provided on the delivery of the intervention. How many parents participated in one DHE or MI session? And what was the rationale for the delivery of DHE and MI in a group setting? The limitations of group counselling are well-discussed in the discussion, but this makes me wonder why the authors did not chose for one-to-one sessions (or whether the feasibility in group sessions was a specific objective of their study, which should then be made clear in the introduction).

Numbers of participants in the group sessions are now included on page 8 (line 184) and page 9 (line 198). The use of MI delivered as a group-talk is included in the background on page 6 line 136-139, and as a study objective, page 6 (line 145).

7. Instruments for data collection

The authors mention that they used an adapted instrument to measure oral health knowledge (Naidu et al.), oral health self-efficacy and oral health fatalism (Finlayson et al.). Since these were adapted, it would help to specify which item were maintained (or if all were used), how they were scored (response categories, 4-/5-point Likert scale) and how total scores were computed. The computation of the 4 construct-scores of the RAPIDD measure should also be described.

The development of the questionnaire and item scoring are now discussed in more detail on page 11-12 (lines 253-271).
8. Results: statistical analysis and presentation of continuous variables

The authors describe that mean scores of the continuous variables (brushing frequency, self-efficacy, fatalism and RAPIDD constructs) were analyzed using linear regression and ANOVA. For each variable they present a figure with one p-value. My concerns with this are as follows:

- a) The difference between baseline and follow-up (for both the test-group and control-group) should be tested with a paired T-test, and b) the difference between the test-group and the control-group (at follow-up) should be tested with an independent samples T-test

The figure for each variable only provides one p-value, so it is not clear what this p-value reflects. Although the figures are nicely demonstrating the values, I would suggest to present means and 95% CI’s of the continuous variables in one table to save space.

The linear regression analyses and figures have been removed. Difference between test and control-group area at follow-up are now presented in one table (Table 7), based on an independent sample t-test, with means and 95% CI’s. A paired t-test was not possible as pre and post intervention data could not be matched to individual participants.

9. Discussion: claims of findings

In the discussion, the authors often report that the findings showed ‘an increase’ / ‘trend’ although not statistically significant. Given the non-significant relationships (which could indicate random error) and the small sample size, the authors should be cautious in claiming too many effects from this exploratory study. Also the focus group data are only based on the views of 6 parents, which could be highly biased and non-representative. Therefore, I would advise the authors to review the discussion for claims and reduce where necessary. The conclusion is more carefully written.

Throughout the manuscript, reference to trends in the data have been removed and only significant findings are presented.

10. Discussion: limitations

The majority of limitations are well-described in the discussion section. Two important limitations that should be added are:

a) Only one person was trained to provide the MI-talk, and thus he/she provided all MI interventions. The quality of the delivery of the MI method may not be representative of other MI-providers (e.g. better or worse) and therefore the authors are very limited in their ability to generalize their conclusions on the effectiveness of MI (as this may have highly depended on the provider).

This is now discussed in Limitations of the study’ page 28, point 6.
b) A focus group interview was only conducted for the test-group, therefore the authors cannot conclude whether parents’ experiences were more positive or negative in comparison to regular dental health education. Although limitations are well-described, the authors should provide details on how these limitations have implications for the interpretation of their results. E.g. the small sample size (which may have caused type-2 errors), and the low response rate, which may have resulted in a more motivated group of participants. The small-response rate may also imply that there is limited interest from the population to participate in the intervention, which then has implications for the potential coverage of the intervention.

The lack of a focus group with the participants from the control group is now mentioned in limitations of the study on page 28 (point 9 line 651-653).

The effect of the small sample size, a more motivated group at baseline and possibility of type II errors, are now mentioned in the Limitations of the study on page 27, point 1 (lines 610-612).

Minor essential revisions

11. Results: Sample characteristics. The characteristics of the sample should ideally be presented in a table, since the sample characteristics description in the results section is not fully complete.

Sample characteristics at baseline are now presented in Table 5.

12. Results: Presentation of categorical variables. Table 5 to 8 could easily be merged into one table, which also makes it easier to compare the test and control group at baseline and follow-up.

The results in Table 5 and 8 in the original submission have now been merged into one table in the revised manuscript (Table 6).

13. General

The manuscript could be written more concisely, by removing general and very detailed information (e.g. on qualitative research, detailed description of results in tables). The manuscript could benefit from some editorial changes (e.g. clear distinction between headings and subheadings and consistent use of punctuation).

The general information on qualitative research has been removed and the detailed description of results. Different font-sizes have now been used for headings and sub-headings.

Discretionary revisions

a) Theoretical framework: suggestion to already briefly introduce the conceptual framework of MI (the Transtheoretical / Stages of Change model) in the introduction.

More detail on the TTM and its relationship with MI have been now been included in the background, page 4 (lines 88-98).
b) Telephone follow-up (page 8): suggestion to add follow-up times (after 2 weeks and 1 month) in the text, in addition to Table 3.

This information has been added to the text, page 10 (line 230) in addition to Table 1.

c) Table 3: suggestion to mention this table at the section ‘Experimental design and MI protocol’ (then it will become table 1)

Table 3 is now Table 1 in the revised manuscript.

d) Instruments: suggestion to remove ‘(MO group talk)’ as it may suggest that data was only collected for the test-group at baseline and after 4 months.

This has been done.

RESPONSE TO REVIEWER 2:

1. In the introduction the authors may wish to make reference to Gao et al (2014) which is a systematic review of the uses of MI in dental settings.


Reference to this research is now included in the Background page 4 (lines 99-103). Reference [10].

2. The lack of equivalence in the control and intervention groups at the start of treatment is very problematic, and it seems impossible to control for this statistically given the very small numbers available for analysis.

This issue is now mentioned in ‘Limitations of the study’, page 27, point 3 (line 629).

The regression analysis has been removed in the revised manuscript.

3. The number of analyses performed is large given the small sample sizes. Could the authors restrict the number of analyses to only primary analyses or correct for the number of analyses performed?

Only primary analyses are presented in the revised manuscript.

4. A post hoc analysis of power might be informative given that I was unable to find an apriori sample size calculation.

A post hoc power analysis is now included in the results on page 17 (line 386). The study power and issue of Type II errors is mentioned in the Limitations of the study on page 27, point 1 (lines 611-612).

5. The major problem I have with this is the nature of the intervention and control groups. We have known for some times that interventions based on dental health education in the absence of fluoride
application are largely ineffective, if not harmful in terms of increasing inequalities. Why then was this chosen as a control?

The ineffectiveness of traditional dental health education with respect to behaviour change is mentioned in the Background page 3 lines 66-69. However traditional DHE is still included in many oral health promotion programmes and familiar to the dental nurses. DHE was therefore considered viable for the control group.

In the systematic review by Gao et al of studies that compared MI to CE (conventional health education) found that MI outperformed CE in improving oral health behaviours in families with preschool children. This is believed to have resulted from greater uptake of preventive dental services, in particular, fluoride varnish. The value of eliciting this specific behaviour change has now been included on 5 page (lines 111-115).

Secondly we have no information on the integrity of the intervention - how do we know that MI is what was delivered here? There are techniques for assessing the extent to which the delivery matches the principles of MI (such as the use of the MITI, Moyers et al 2010). Reviews of treatment integrity in MI studies have noted that it is notoriously difficult to achieve. The intervention as described has clear elements of goal setting and planning - both of which have been identified as highly effective brief interventions for behaviour change (see for example Michie et al 2013).

The issues of fidelity to MI is now discussed in ‘Limitations of the study’ on page 27, point 2.


In the revised manuscript, these studies have been cited as reference [6] and [43] respectively.

6. The drop-out rate in the intervention is four times that of the control group – the authors should discuss this at length including the implications for the findings and the acceptability of the intervention.

Figure 1 was in error in the original submission. The much higher drop-out rate was in the control-group and the figure has been corrected in the revised manuscript. This issue is discussed on page 26 (lines 597-603).