Reviewer's report

Title: The effect of motivational interviewing on oral healthcare knowledge, attitudes and behaviour of parents and caregivers of preschool children compared to traditional dental health education: A feasibility study

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Reviewer: Denise Duijster

Reviewer's report:

Dear Dr. Stefan Listl,

Thank you for your invitation to review manuscript ‘The effect of motivational interviewing on oral healthcare knowledge, attitudes and behavior of parents and caregivers of preschool children compared to traditional dental health education:a feasibility study’.

This manuscript reports on an exploratory trial to evaluate the effect of motivational interviewing (MI), in contrast to traditional health education, on oral health knowledge, attitudes and behaviours in a sample of 79 parents of preschool children in Trinidad. Evidence on the effectiveness of MI at improving oral health is inconclusive; therefore more research into this area is welcome. The manuscript addresses a topic that is relevant to the readers of this journal.

In general, the manuscript is well-written and clearly structured. The authors used appropriate techniques to support the credibility of data, which include the use of validated questionnaires to measure attitudes and behaviours, and the use of topic guides and member-checks to support the quality of focus group data. However, there are a number of methodological and statistical limitations within the manuscript. My main concerns are as follows:

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 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.

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

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’.

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?’.

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.

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 choose 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).

7. Instruments for data collection
The authors mention that they used an adapted instrument to measure oral health knowledge (Naido et al.), oral health self-efficacy and oral health fatalism (Finlayson et al.). Since these were adapted, it would help to specify which items 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.

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. Due to the small n and the setup of the scales, it could be that the variables are not normally distributed and then these should be tested non-parametrically (Wilcoxon signed-rank test and Mann-Whitney U, respectively). No regression analysis or ANOVA’s should then be performed.

- 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.

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 advice the authors to review the discussion for claims and reduce where necessary. The conclusion is more carefully written.

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).

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.

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.
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.

13. General
The manuscript could be written more concisely, by removing general and very detailed information (e.g. general information 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).

Discretionary revisions

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

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.

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

d) Instruments: (page 8, line 198): 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.

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:
I declare that I have no competing interests