Author’s response to reviews

Title: Promoting parenting strategies to improve tooth brushing in children. Design of a non-randomised cluster-controlled trial

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The Shine! intervention: promoting parenting strategies to improve children’s tooth brushing behaviour. Design of a clustered controlled trial

BMC Oral Health

Dear Editor,

Thank you and the reviewers for taking the time to review a revision of our manuscript. Please see our responses to the reviewers’ comments below and a description of the changes included in the revised manuscript.

The first reviewer raised several new issues that weren’t raised in the first round of review and did not derive from the changes we made. We tried to address these comments as much as possible, or we provided an explanation.
Reviewer 1 (Peter Milgrom)

This revised manuscript is greatly improved but still requires a major revision.

We thank the reviewer for his time to review a revision of our manuscript, and for the comments and suggestions for improvement.

1. Overall, it requires editing by a native English speaker. For example, there is an incomplete sentence that begins on line 9 of the abstract. This design is not really a "cluster randomized trial" as the clusters are not randomized. Rather this appears to be a prospective cohort design. Another example, line 31, page 8, is the use of the word "convenient". The correct word is "convenience" but it is not used appropriately in any case as the unit of analysis is the child. This may be moot if the design is not a cluster randomized. If the study is determined to be cluster randomized, the authors should review and complete the appropriate CONSORT checklist for cluster trials.

We notice that there might be some confusion regarding the design of the ‘Shine! study. This paper describes a non-randomised cluster controlled trial, and indeed not a randomised cluster-controlled trial. In the manuscript we used the term ‘cluster-controlled trial’, but for clarity we have now added the word ‘non-randomised’ throughout the manuscript. We disagree with the term prospective cohort, because cohorts refer to observational studies. The ‘Shine!’ study is an experimental study with a control group; allocation to the intervention or control group happened at practice-level and not at the individual-level (hence, the term ‘cluster’).

We thank the reviewer for noticing the error with the word ‘convenience’. We have now changed this (see page 8, line 27 and 23, line 48). We believe that ‘convenience’ is the correct term for our sampling method, since intervention practices were encouraged to voluntarily apply for participation in the study (see Methods, Study sample, General dental practices, page 8).

Based on the reviewer’s suggestion, we asked a native English speaking colleague to read our manuscript. Sentence 9 in the abstract has now been rephrased (see page 2).
2. Very often in the manuscript there is a confusion between parent and child behavior. At times the focus is on the parent brushing the child's teeth where other times the focus seems to be on child brushing. The manuscript needs to be consistent.

We understand the confusion the reviewer raises. Guidelines recommend that parents brush their children’s teeth and supervise tooth brushing until children are ten years old. Therefore, our intervention concentrates on parents’ tooth brushing behaviour for their child.

A sentence on the guideline recommendation has been added to the introduction (see Background, page 4, line 41-43). Furthermore, we deleted the term ‘children’s tooth brushing behaviour’ throughout the manuscript, and where necessary, changed it into ‘tooth brushing in children’. We clarified that with this, we refer to tooth brushing by a parent for the child (see Methods, Outcome measures, page 15; and Data collection, Behavioural outcome, Tooth brushing in children, page 15-16, line 58-2).

3. The review of the relevant theory is in several different places in the manuscript and is repetitive.

We have restructured the manuscript as follows; the theory of the intervention is described under the heading ‘Underlying theory – Principles from learning theory’ (see Introduction, page 6 and 7); the steps / content of the ‘Shine!’ intervention is now described under the heading ‘Development and procedure of the ‘Shine!’ intervention’ (see Methods, Intervention, page 11-13). Duplicate information has been removed.

4. Line 22, page 9. Clarify who is making the list of patients and randomizing. If this is being done by a researcher, this may be a violation of patient privacy. Also, the authors should clarify how the actual exclusions are to be defined. They seem to be excluding potential participants who might benefit from the intervention and limiting the generalizability of the results.

We would like to refer to our response to comment 1 that this is a non-randomised controlled trial. Dental practices in the intervention group (who voluntarily applied) will recruit ten children who will all receive the ‘Shine!’ intervention, while children recruited from the control practices will all receive care as usual. In the Method section we describe that the coordinating dental therapist will randomly select patients from his/her own practice (not the researcher): “Eligible children will be listed from the patient registry and given a random number by the coordinating
dental therapist, using the website http://random.org. Subjects will be approached in numerical order, until at least ten children are included in the study.” (see Methods, Study sample, Children and their parents, page 9, line 27-34).

This is the first trial to evaluate the ‘Shine!’ intervention, so we are firstly interested to assess the intervention’s potential in the general population visiting a dental practice. Therefore, we decided to exclude children with indication of a very high caries risk. We agree that these children may benefit from the intervention; therefore, we are currently planning to conduct a similar trial in Scotland to evaluate the ‘Shine!’ intervention in a high-risk population, but that’s beyond the scope of this paper.

5. The flow diagram in figure 2 should conform to the CONSORT guidelines for cluster trials.

We included Figure 2 to graphically present the number and description of research visits as part of the intervention, and what data will be collected during each research visit.

The CONSORT flow diagram for clustered controlled trials includes information on the number subjects assessed for eligibility, enrolled, followed-up and analysed. This information is very relevant, and will be presented in the results paper when this information is available.

6. The online references should be archived at webcite or a similar place so that they remain available in the future. Could the authors provide a more complete reference for #3 (web reference points to a general UK government website) and #49?

We have provided a more complete reference for #3 and #49 (see References, page 28 and 35). We are not familiar with archiving weblinks at webcite, and this is not a requirement for BMC Oral Health, so we would prefer to leave this decision to the editorial office.

7. All of the supplementary material is in English while the language of the study is Dutch. The authors should provide the supplementary material in the language of the study. The general principle is that another investigator should be able to repeat the study based on the information provided.
We have now provided the supplementary materials in Dutch (the original files). We have decided to also keep the supplementary materials in English, since this was a request from another reviewer.

8. The information given to the practices should be provided as a supplement, in Dutch.

The information letters for the control and intervention practices, as well as the information letter for participants in the intervention and control practices, have now been provided as a supplementary material (in Dutch).

Key concerns that remain:

9. The primary outcome(s) of a trial need to be specified a priori. This is fairly clear for the caries measure but still lacks precision. How will the ICDAS information be used to derive an outcome measure? A lot of the caries lesions are likely to be in the enamel where measurement is notoriously unreliable. Will they use the active vs inactive lesion measure? When this is cleared up the abstract will need to be modified along with the body of the paper.

We agree that there is a risk of unreliable scores when enamel lesions are scored and/or when the full ICDAS index is used (distinguishing between 6 stages of the caries process). Therefore, we decided to use the merged ICDAS scoring system, using only 3 stages (see Methods, Data collection, Clinical outcome, page 17, line 29-34), which is the recommended ICDAS scoring system for population and clinical caries research. Herewith, we do not distinguish between active and inactive caries lesions.

In the statistical analysis section, we now describe how we will compute the ICDAS scores for the analysis (see Methods, Statistical analysis, page 18, line 5-7). We will combine ICDAS scores ‘B and C’ (moderate and extensive caries) and scores ‘A, B and C’ (initial, moderate and extensive caries). The reason we also include initial lesions in the latter score, is because the follow-up of our trial is only 24 months, and therefore initial caries lesions could provide relevant information on caries progression in the intervention versus the control group. The reliability issue will be addressed by the training and minimum inter- and intra-reliability requirements (see our response to comment 18).
Because of the restricted number of words in the abstract, we could not provide these details in the abstract.

10. The authors' presentation is unclear as to how they expect the intervention to work. There is no real model. This leaves open the question of how they will interpret the results and concern about post hoc analyses. There are three variables that apparently will be used to assess the behavioral outcome but no detail on how these variables will be combined or how this "outcome" will be calculated. What point in time is primary (see line 51, page 10)?

With regard to the reviewer’s comment about the behavioural outcome, we now provide more information on why we selected three proxy measures and how they will be analysed.

Given the challenges with objectively measuring the practice of tooth brushing in children, three proxy measures will be used that each have strengths and limitations. Questionnaires provide useful information on parents’ perceived self-efficacy and self-reported tooth brushing behaviour for their child, however, there is a risk of socially desirable responses. Children’s dental plaque accumulation gives an indication of the efficiency of tooth brushing and state of oral hygiene but has its limitation that it presents snapshot data (see Discussion, 6th paragraph, page 23, line 5-25).

The three variables will not be combined, but separately analysed (see Statistical analysis, page 19, line 58). These results together will provide better insight to whether changes have occurred in the practice of tooth brushing as a result of the ‘Shine!’ intervention, in comparison to using only one proxy measure. See also our response to comment 12.

In response to the reviewer’s first comment, we would like to refer to the sections in the manuscript where it is explained how the practice-based intervention model is composed.

- The components of the intervention are clearly described in the Methods section on page 11 to 13. They include 1) the identification of parents’ barriers to tooth brushing (based on literature) and 2) discussion of strategies to tackle the barriers using conversation techniques that have shown to be effective. The strategies per barrier are presented in Table 1.

- The underlying theory of the intervention is presented and explained in the background section on page 6-7. The intervention is based on principles from learning theory – a well-established theory from behavioural psychology.

- The mechanisms by which the intervention is hypothesised to work is also described in the Background section on page 6: "The hypothesis of the ‘Shine!’ intervention is that the promotion
of specific parenting strategies will facilitate oral hygiene behaviour change in parents, resulting in the improved practice of twice daily tooth brushing in children (behavioural outcome) and subsequently, lower development of childhood dental caries (clinical outcome).” The evidence for the effectiveness of tooth brushing to prevent dental caries has been presented in the introduction.

- We also provided Figure 1 in which the model is schematically presented.

We deleted the word ‘primary’ on page 10, line 51.

11. There is no hypothesis suggesting that they expect to plateau and then diminish although they build in booster interactions.

We think the reviewer refers with ‘booster interactions’ to the multiple contact moments between the parent and oral health professional, as part of the intervention. We built in recall visits because it is known from literature that multiple contact moments and follow-up on the progress of action points is important to reinforce behaviour change (as opposed to having only one contact moment).

12. Similarly, there are variables such as the plaque index that require some explanation. How does this fit into the model? What if the plaque index shows a lot of plaque, the parent self-report indicates regular twice a day toothbrushing, and there is no caries increment? How will this be interpreted?

We have included a sentence in the data collection section, explaining why we included plaque scores as a proxy measure for tooth brushing: “Dental plaque indices are used to assess an individual’s level of plaque control, providing an indication of the efficiency of tooth brushing and the state of oral hygiene.” (see Methods, Data collection, Children’s dental plaque scores, page 16, line 41-44).

Our hypothesis is that all three behavioural proxy measures (parents’ reported tooth brushing behaviour for their child, parents’ self-efficacy and children’s plaque scores) will improve in the intervention group at follow-up, in comparison to the control group. If analyses reveal that only one or two measures indicate improvement, or if parental reports are in conflict with clinical scores, these findings will then need to be very carefully interpreted and critically discussed in
the results paper. We cannot yet anticipate what the results will be, and therefore we do not consider it appropriate to already speculate about the interpretation of potential results in the design paper.

13. The hypothesis testing in the analysis section needs to be clearer. This is a clinical trial. As is customary, the hypotheses should be tested without adjustment for co-variates. Then, differences between the groups should be systematically investigated. If there are to be subgroup analyses they need to be prespecified to be sure they are adequately powered.

We agree with the reviewer that it is unusual to adjust for confounders in a clinical trial, as one can assume that co-variates are equally distributed between the intervention and control group due to randomisation. However, this is a non-randomised controlled trial (see our response to comment 1), so there is a chance that co-variates are unevenly distributed.

Therefore, we will first test the hypotheses without adjustment (only controlling for clustering of observations within dental practices – due to the clustered design). We will analyse differences in co-variates between the intervention and control group at baseline. If there are significant differences, we will then either adjust the analysis for these co-variates or - in case of sufficient power - we will perform a sub-group analysis. This has now been clarified in the statistical analysis section (see Methods, Statistical analysis, page 20, line 15-22).

Since we do not know if / what co-variates will significantly differ between the intervention and control group at baseline, we could not pre-specify sub-group analyses for the power calculation.

14. The authors need to specify that the analysis follows the intent to treat principle. There are various points where parents may drop out, miss appointments or be excluded. This creates the potential for bias, which is not addressed.

Based on the reviewer's suggestion, we have now included the performance of an intent to treat analysis in the analysis section to account for participants that will be lost to follow-up (see Methods, Statistical analysis, page 20, line 22-25).

15. A major confound appears to be potential differences in how the children were cared for during this period. There does not appear to be any collection of information about differences in the practices. It is hard to believe that children who are at higher risk might not have received
treatments that modify the results. The practice matching does not address this problem and the ICDAS exam might not capture this.

The intervention and control practices are using the same in- and exclusion criteria, so we expect that the number of ‘low’ and ‘medium-high risk’ children will be equally distributed among the intervention and control group (high risk children are excluded – see our response to 4). We also emphasise in the instructions for the control and intervention practices to provide ‘care as usual’ following national guidelines, so presumably the methods of care won’t differ much between practices (see Training of oral healthcare professionals, page 13-14).

However, since this is a non-randomised trial, there is indeed always a risk that there will be differences in the patient population and care methods between intervention and control practices. Therefore, we have now added (under heading ‘co-variates’) that we will extract information from dental records on the restorative and preventive care that children received during the study, so that (in case of significant differences) we can take this into account in the analysis and interpretation of data (see Methods, Data collection, Co-variates, page 18, line 17-24).

16. Note also that in many countries children of this age group are in preschools or kindergartens where toothbrushing is a supervised required activity.

The argument of the reviewer holds in many countries, but in the Netherlands, tooth brushing in Kindergarten is not a standard practice. It depends on the policy of the individual Kindergarten; often tooth brushing is considered a responsibility of the parent(s)/carers (at home). Furthermore, The Netherlands has the largest proportion of women working part-time in Europe, so there are many children that do not attend Kindergarten.

17. If it is determined that this is indeed a cluster randomized design, additional attention is required in the sample size section. The authors should clarify that the number of clusters and cluster size were determined concurrently. Further, they should report the plots of power or precision against cluster size to enable identification of points beyond which further increases in cluster size make no material contribution to the study. They should also report the sample size required under individual randomization. The authors might also justify their decision to have the same number of test and control practices.

If this is actually a prospective cohort design, this section will require modification in any case.
We would like to refer to our response to comment 1 that this is a non-randomised cluster-controlled trial. The reason we decided to use clusters (allocating the intervention at practice level instead of individual level) is to minimize the risk of contamination bias. To compare differences between the intervention group and control group in behavioural and clinical outcomes, our unit of analysis will be the individual child. Therefore, in our power calculation, we calculated the number of individual children required. Yet, given the clustered design of our study, we will need to adjust our analysis for clustering of observations within practices. This requires additional power, and therefore we increased the sample size with 15% to account for clustering in the analysis (see Methods, Power calculation, page 10).

When there are a large number of clusters (as in our study, where we will have 40 practices / clusters), the additional power that is required is much lower than when there are only a few clusters.

18. The authors need to specify the required level of inter and intra-rate reliability required for certification of examiners. Also, the authors should specify when the clinical training will occur. One assumes it will be just before the exams?

The training will be organised 23 months after inclusion of the first participants. During the training, the dentists’ intra and inter-examiner reliability will be assessed; a minimum weighted kappa of 0.60 for both the intra and inter-examiner reliability is required to certify as an examiner (see reference 37). In case of lower reliability scores, more training will be provided. This information has now been added to the manuscript (see Methods, Data collection, Clinical outcome, page 17, line 44-54).

19. There needs to be a section added on blinding.

Information on blinding has now been added to the manuscript (see Methods, Blinding, page 20, line 41-46): “The research team will be blind to whether the participants are assigned to the intervention or control group, when scoring dental plaque and performing the data analysis. Study participants and dental professionals providing the intervention and / or collecting data cannot be blinded.”

20. The authors need to specify the criteria for certifying the interventionists.
The criteria we specified in order to provide the intervention are 1) to be a certified dental therapist and 2) to have completed the full ‘Shine!’ training. The first criteria has been described in the inclusion criteria for dental practices (see Methods, Study sample, page 8): “Practices will be eligible to participate if they have employed a certified dental therapist who is able to carry out and coordinate the study within their practice.” The training is described on page 14.

Reviewer 2 (Deborah Polk):

The manuscript has been extensively improved and the authors have addressed all of my questions. Thanks, too for writing the modifications in red, which made it very easy to identify.

We thank the reviewer for her time to review a revision of our manuscript.

Minor changes

1. In the Methods section, the following phrase is hard to read: "at least ten three to four-year-old children". In other papers, I have seen it handled this way: "at least ten 3- to 4-year-old children."

As suggested by the reviewer, we have changed the sentence to ‘3 to 4-year old children’, and we now use this style of reporting age throughout the manuscript.

2. Lines 14-15 of page 24 is confusing (Training of oral healthcare professionals). Is a word missing?

We have changed this sentence to: “Dental practices in both the intervention and control group will separately be informed about the study procedures, which includes information on the recruitment of participants and methods of data collection, as well as information on the
provision of care as usual and dental health education according to national guidelines [4, 28].” (see Methods, Training of oral healthcare professionals, page 15, line 7-14).

Reviewer 3 (Marja-Liisa Laitala)

The authors have thoroughly revised the manuscript according to my comments. However, when inquiring about the intraexaminer reliability the authors explained about the INTERexaminer reliability (page 17). The authors should at least consider the stability of clinical data recorded by one individual (consistency of a single examiner in the application of the ICDAS index over time).

We thank the reviewer for her time to review a revision of our manuscript.

We have now also added information on the assessment of the intra-examiner reliability of the dentists (see Methods, Data collection, Clinical outcome – children’s dental caries experience, page 17, line 46-54).