Reviewer's report

Title: Long-term follow-up of a high- and a low-intensity smoking cessation intervention in a dental setting - a randomized trial

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Reviewer: Christopher P Niemiec

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Review for BMC Public Health of “Long-term follow-up of a high- and a low-intensity smoking cessation intervention in a dental setting: A randomized trial”

This manuscript presents long-term follow-up data comparing high- and low-intensity smoking cessation interventions in a dental setting. Indeed, although tobacco use contributes to premature morbidity and mortality worldwide, little is known about whether typical assessments of long-term maintenance of tobacco abstinence (at 1-year post-intervention) predict maintenance over longer periods of time. Therefore, I agree with the authors that it is an important public health goal to begin to develop an understanding of the factors that predict lifelong maintenance of tobacco abstinence. Yet I have several concerns with the manuscript that tempered my initial enthusiasm for the work, which I will consider next.

Major Compulsory Revisions

1. In the Abstract, the authors stated that one aim of their work was to assess whether the relative difference between high-intensity treatment (HIT) and low-intensity treatment (LIT) would persist from the 12-month follow-up to the long-term (5 – 8 year) follow-up. I found this to be an odd aim for a study, especially given that there were no statistically significant differences between HIT and LIT on 7-day point prevalence or 6-month continuous abstinence (2 of 3 abstinence outcomes). Does it matter that a non-significant difference is stable over time? One important task for the authors is to provide a rationale for why stability of non-significant group differences over time is an important aim for a study. There may be clinical relevance to the observed non-significant group difference of 7% (as the authors suggest in the second paragraph of the Discussion), and thus it is important for the authors to underscore how/why such a difference is clinically relevant.

2. One of the major limitations of this study was the lack of a “usual care” condition that could be used for comparison purposes. Indeed, the data suggest that there are robust quit rates for HIT and LIT on 7-day point prevalence and 6-month continuous abstinence, but without a “usual care” condition it is impossible to know how these conditions compare to what was available “in the community”. As the authors note in the second paragraph of the Background, the international literature indicates that about 10% of smokers who try to quit are
abstinent at 12 months if no support is available. I wonder if this estimate can be used as a “comparison condition” in lieu of the lack of a “usual care” condition.

3. It is interesting to note that the same relative difference in quit rates was observed on each of the abstinence outcomes (31% vs. 24% on 7-day point prevalence; 26% vs. 19% on 6-month continuous abstinence; 12% vs. 5% on sustained abstinence). Yet whereas this 7% difference was significant on sustained abstinence, it was not significant on 7-day point prevalence or on 6-month continuous abstinence. It will be important for the authors to address why the same relative difference in quit rates was significant for one outcome but not for the other two outcomes.

4. In the middle of the Background, the authors suggest that tobacco cessation programs are cost-effective. It would be useful for the authors to provide such estimates rather than just refer the reader to other publications. Of more importance, a major way in which this manuscript could be improved is to conduct a cost-effectiveness analysis of HIT vs. LIT, especially given the seemingly dramatic group differences in cost (treatment intensity) and the lack of group differences in effect (non-significant group differences on 2 of 3 abstinence outcomes).

5. Taken as a whole, my impression of the Background is that it was rather “jumpy”, as the authors moved from topic to topic without a clear development of their primary and secondary research questions. For example, the authors devoted 1 sentence to “dental clinics” although this is the setting for the study.

6. It will be important for the authors to provide more information on the operationalization of HIT and LIT. It is asking too much of the reader to review the “original study” in order to obtain a coherent understanding of the study conditions. Also, it will be important for the authors to speculate on the theoretical mechanisms that may underlie any observed differences in quit rates (statistically and/or clinically significant) between HIT and LIT. At present, the reader is left to wonder why there is a 7% relative difference in quit rates between HIT and LIT. (And future research may be developed to test such mechanisms of change.) Finally, given that some readers may question the validity of self-reported quit rates (rather than biochemical validation of quit rates), it will be important for the authors to address this in the Methods as well as in the Discussion (see paragraph directly before Conclusion).

7. The mean long-term follow-up time was 6.3 years with a range from 5.4 years to 7.6 years. It will be important for the authors to ensure that length of long-term follow-up time did not differ between HIT and LIT, as one interpretation of the higher quit rates observed in HIT is that participants in that condition provided data closer in time to the planned cessation date and were, therefore, more likely to be abstinent.

8. The authors report group comparisons on 5 outcomes, namely, 7-day point prevalence, 6-month continuous abstinence, sustained abstinence, number of smoke-free days, and number of days from baseline to first quit. However, only 2 of 5 outcomes were found to be significantly different between groups. Indeed, if the authors adjusted their level of significance to .01 to account for the number of
tests conducted, then they would find no significant differences between groups. It will be important for the authors to speak to the “contribution to the field” made by their work.

Minor Essential Revisions

1. I noted a discrepancy between the Abstract (response rate of 85% corresponds to N = 284) and the Results (response rate of 85% corresponds to N = 241). It will be important for the authors to clarify these estimates.

2. Results for “other support”, “pharmaceuticals”, “snus”, and “still trying” are presented without much, if any, description and/or theorizing in the Background. If these analyses are retained, then I suggest that they be presented as “Preliminary Results” before the abstinence outcomes.

3. Results for “gender differences” are not statistically significant (p = .051 and p = .171), yet the authors present those data as though they are significantly different (“women had higher abstinence rates…”).

4. I was not able to understand the material that was presented in the section, “Transitions between smoking statuses from 12-month to long-term follow-up”.

5. Taken as a whole, my impression of the Discussion is that there was too much data presented in the first 2 paragraphs.

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

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests.