Reviewer's report

Title: Number of teeth and myocardial infarction and stroke among elderly people

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Reviewer number: 1

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In this manuscript Syrjälä et al., investigate the association between tooth loss and myocardial infarction/ ischemic stroke in an elderly population.

Major Compulsory Revisions

The study is interesting but some issues need clarification/elaboration. I am most concerned with the quality of the study group and the analysis conducted. The authors state that the study population was selected using random sampling, but rather little information is provided on the sampling strategy, recruitment, or the impact of differential participation. This poses some problems to assess the potential impact of selection bias. This is particularly relevant considering that 308 subjects from the originally 700 selected were not included in the final study group.

I think that some aspects of the study suggest confounding due to socio-economic factors, which affect tooth loss, periodontal disease, cardiovascular diseases, and survival. This need to be discussed.

I am afraid that my specific comments are not page-related but the document had no page numbers.

Specific comments:

1) In the discussion it is stated: “... On the condition that these..., ...may lead to accelerated atheroma infection.” This is not clear and I think that it is not correct. If teeth were lost it is probably because of a form of infection and that means earlier exposure, if teeth are retained, the argument of exposure to infection does not hold much as it would be a question of earlier or later exposure, which this study design cannot disentangle. I recommend eliminating this sentence.

2) In the discussion-section the authors state: “In order to eliminate confounding”. This should be rephrased to something like ‘reduce’ as we never know whether we have eliminated confounding or not.

3) In the discussion-section the authors state: “… some residual confounding existed is not totally excluded”. Should be changed to “… some residual confounding existed cannot be excluded”.

4) The subsection ‘Limitations of the study’ needs rephrasing. The authors need to make a better case for periodontitis if they want to include this. In the next paragraph, the authors insist posing periodontal infection as the underlying factor. This needs some elaboration if it is going to be included. I would prefer to limit the discussion of causation to an explanation of the existence of many potential causes of tooth loss and the uncertainty of using this parameter. A discussion of the social dimensions of tooth loss is missing. People lose their teeth because somebody makes a decision (usually the dentist) and the patient accepts this decision. Some patients would prefer to retain their teeth.

5) Same section: The authors discuss on the ‘validity of tooth loss’, and mention the periodontal condition, this is again an unsubstantiated focus on periodontal disease. The validity of tooth loss has to do with the extent to which we measure tooth loss when we record the number of teeth present. I think that the use of the word validity should be change to something like “the extent to which tooth loss reflects periodontal disease and/or caries”. They actually could use the term ‘validity’ when they refer to a geriatrician recording tooth loss because this could be a source of poorer validity of the recordings.

6) In the text: “These data suggest..., ... or that biases may have prevented us from detecting the true association”. Is not clear and need rephrasing to something simpler. The following sentence “However... and cardiovascular diseases exist” is superfluous and can be eliminated.

7) The manuscript needs language revision. The authors are advised to contact an independent reviewer proficient in English.

8) Was the examiner blinded to case status? If not, the possibility of bias due to this has to be acknowledged.

9) If the study included an overrepresentation of elderly women (who had never smoked), we are talking about a very special subgroup of any population. This should be stated and discussed more extensively.

10) A discussion of the fact that the lack of association found in this study only represents survivors of myocardial infarction/ ischemic stroke may also be of value, as this has never been discussed in the literature on the topic.

11) Subjects in the dentate group were more likely to have low education (Table 1), please discuss the implications of this.

12) In table 3, univariate analyses included in columns before the multivariate models are needed. They would illustrate how different variables related to the outcomes before adjustment. Pressing all the covariates in a model at once is a legitimate option, but I think the authors need to discuss why they included this modeling approach instead of selecting relevant covariates in a process like the likelihood ratio system suggested by Hosmer and Lemeshow.

13) Please, elaborate on the increasing PPR in figs. 1 and 2 according to the number of teeth.
14) The sample size calculations done before the study ought to be presented.

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

**Quality of written English:** Needs some language corrections before being published

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