Reviewer’s report

Title: Information and communication technologies for approaching smokers: A descriptive study in primary healthcare

Version: 2 Date: 31 October 2014

Reviewer: Raquel Paz Castro

Reviewer’s report:

Re-Review

With reference to the answers to my first review:

1. I still miss an estimation of how representative the three health care centres are in respect to the whole country. E.g. there isn’t any centre of the south part (Andalucia), where maybe the ICT use could be different from the more central or north regions. This should at least be discussed in the limitations of the study.

2. The author highlights in the abstract the participants have been randomly recruited, but no further specification has been undertaken in the methods part to explain this procedure.

3. I still miss an example of the questions regarding ICT availability and use. Did the interviewers ask an open question regarding how often the participant used ICT’s or where the questions pre-categorized in non, less than 1/week, 1/week etc.?

8. I still think that the authors should emend their report of the results in the whole paper. A few examples:

- In the introduction section they should adjust the percentages to the same decimal points.
- In section ‘subjects’, second last paragraph, they refer to non-significant differences between participants and non-participants, but they don’t declare critical values of X-square tests nor exact p-values.
- In section “Results” the authors tend to round the results in the first paragraph (e.g. 90% instead of 87.5%).
- In section ‘limitations and future directions’, second last paragraph, they affirm that the differences in nicotine dependence between users and non-users aren’t clinically significant, but it isn’t clear, what ‘<0.4’ means and where they took it from. This affirmation seems contradictory to the sentence in the abstract of the article, namely: “Compared with non-ICTs users, users declared lower consumption of tobacco, younger onset age, and lower nicotine dependence.”

Major Compulsory Revisions

- It is unclear to me, which analyses were undertaken in Table 1 and Table 2. As an example: Does a p-value of less than 0.001 in Table 1 ‘gender’ mean, that non-users and users are different, or male and female in general?
If the authors intended to cover the differences btw non-users and users, there should be a p-value for each line (compare table 3, 4 and 5). I recommend splitting table 1 and 2, into two tables. First the Descriptives of the sub-samples (non-users/users) and then compute OR and their p-values btw non-users and users similar to table 3, 4 and 5.

**Minor Essential Revisions**

- The authors didn’t calculate any multinomial logit model (=to take more than one independent variable into the calculation), but calculated binary logistic models. They did although take into account, the frequency of use of ICT’s, which is in my opinion a clear improvement and I would recommend to keep the analyses as they are now. But they should replace multinomial with binary through the whole article.

- In section ‘statistical analysis’ they affirm to use ANOVAs for testing the association btw dichotomic and polythomic variables, but in my opinion they meant dichotomic and continuous.

- The comments in the results section on Table 4 and 5 are incomplete. They point out that the results are the same (except for gender) as in table 3 but this isn’t true for social class in low use of ICT.

- The first limitation in section ‘limitations and future directions’ points out, that there can’t be said anything about the causality in this study, but I doubt, that this is a limitation at all in a mainly descriptive study.

**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.

**Declaration of competing interests:**

I declare that I have no competing interests