Author's response to reviews

Title: How do psychosocial determinants in migrant women in the Netherlands differ from these among their counterparts in their country of origin? A cross-sectional study

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Author's response to reviews: see over
To the Editor BMC Public HEalth

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Dear Editor,

Thank you for e-mail at January 19th, inviting us to submit a revised version of the manuscript entitled “How do psychosocial determinants in migrant women in the Netherlands differ from these among their counterparts in their country of origin? A cross-sectional study” (MS: 1513215674178667). We are grateful for the kind remarks and suggestions of the reviewers and have revised the original manuscript accordingly.

Below you find the point-by-point response to the concerns of the reviewers.

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<th>Reviewer 1</th>
<th>Response to reviewer Melissa L Stigler</th>
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<td>Thank you for the opportunity to review your good manuscript. I enjoyed it!</td>
<td>We thank the reviewer for the compliments about our manuscript. Below, we will address the comments subsequently.</td>
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<td>1. Is the question posed by the authors well defined? Yes. It is.</td>
<td>2. Regarding the analyses, we adjusted the text to clarify that we only used a sub-sample in this study (page 9).</td>
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<td>2. Are the methods appropriate? Yes. They are. This includes their study design and sampling methodology, as well as their description of measures. In the analysis section, it would be helpful to clarify that only a sub-sample of participants were used in this study - namely, the smokers. At present, that is not explicit in the text (but is noted, given the sample sizes, in the Tables). This is a &quot;minor essential revision.&quot;</td>
<td>2. We thank the reviewer for the positive comments.</td>
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<td>3. Are the data sound? Yes. They are. They were appropriately gathered across two contexts at almost the same point in time. Thus, the comparison being made is a reasonable one.</td>
<td>3. We thank the reviewer for the positive comments.</td>
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4. Does the manuscript adhere to the relevant standards for reporting and data deposition? I'm unsure what the standards for data deposition are in this context (Netherlands). The authors are reporting their data and results appropriately in this manuscript. A few comments related to the Results and Tables include,

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<td>a) Table 1a. Should the significance be attached to Creole, not Hindustani? This is what appears to be indicated in the text in Results, Characteristics? This is a &quot;minor essential revision.&quot;</td>
<td>4a. We changed the significance to the Creole. (Table 1a, page 21)</td>
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<td>b) Results, smoking related beliefs, first paragraph. Table 2 suggests that the partners of the Surinamese women in the Netherlands accepted their smoking more than the partners of the women in Surinam itself (subjective norm, partner). This is not noted in the text. This is a &quot;minor essential revision.&quot; Consider carrying this through the Discussion, too.</td>
<td>4b. The reviewer is right and we changed the text according to the suggestions. We also mentioned it in the discussion (page 11, 12 and 14)</td>
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<td>c) Results, smoking related beliefs, second paragraph. Table 3 also suggests that the subjective norm of partner discriminates these two samples, too. The OR there is 6.49 and is statistically significant (95% CI: 2.71 - 15.56). This is also not noted in the text. This is a &quot;minor essential revision.&quot; Consider carrying this through the Discussion, too.</td>
<td>4c. The reviewer is right again and we also changed the text on this point (page 11). As described above, we address the subjective in the discussion as well now (page 14).</td>
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<td>d) What does a &quot;POSITIVE cognitive attitude&quot; (Table 3; and Results, smoking related beliefs, second paragraph) mean? The Dutch sample was less likely to believe there were negative health data in Table 2 suggest that the consequences of smoking. It would or consider changing how the scale is named? It is a bit confusing as is. This is a be helpful to clarify this point, in a footnote. &quot;minor essential revision.&quot;</td>
<td>4d. The reviewer is right that the name of the scale is confusing. Hence, we changed the name of the scale in “perceived no negative health consequences of health” in the table as well as in the text (Table 3, page 23 and page 11 respectively).</td>
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5. Are the discussion and conclusions well balanced and adequately supported by the data? Yes. They are adequately supported by the data. Consider pulling the results specific to their partners through the discussion, too. The proximal social environment of the Dutch Surinamese (partner, friends) appears to confer more risk than the proximal social environment of the non-migrant Surinamese. This is a "minor essential revision."

5. We agree with the reviewer that we should pay more attention to the positive social influences among women in the Netherlands. We add this to the discussion (page 14).

6. Are limitations of the work clearly stated? Yes. This is nicely done in the Discussion section and highlights the most major of limitations.

6-9 These comments require no response from our side

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished? Yes. These links are appropriately made.

8. Do the title and abstract accurately convey what has been found? Yes, the title and abstract accurately convey what you have found.

9. Is the writing acceptable? Yes, the writing is acceptable.

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<th>Reviewer 2</th>
<th>Response to reviewer Loraine Greaves</th>
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<td>1. This article addresses an important and critical question regarding migration and smoking prevalence. It is a question common to many countries where immigration rates are high, and the number of different migrant groups wide ranging. The key questions in this field revolve around: what happens to the rates of migrants when they migrate? How do these rates reflect or compare to their country of origin? How are they affected by dominant cultures in destination countries? How can these patterns be understood, predicted and changed, if necessary?</td>
<td>1. We thank the reviewer for her positive comments on our research topic.</td>
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2. All of these questions are more of less addressed by this article, which focuses on one migrant group in the Netherlands, Surinamese women. However, while these questions are addressed, the introduction and discussion sections do not reflect the complexity of the debates regarding acculturation, second generation uptake, integration, racism and discrimination that surround the difficulty of answering these questions. Instead, the authors limit themselves to addressing some psychosocial variables to explain these patterns in a behavioural framework, without addressing the context in which these behaviours evolve and develop. This comment applies to both assessing the Dutch sample as well as the Surinamese sample.

2. We agree with the reviewer that changes in the proximal determinants of smoking as described in this manuscript have to be seen in the context of more distal factors involved in migration processes, including acculturation. We have addressed the acculturation processes in the introduction now (page 4).

3. The methods are appropriate for addressing this specific question, in this more limited framework. The data are sound and appear relevant. One question in this domain would be the relevance of "smoking is cosy"? What does this mean, exactly, and how would it be understood by both samples?

3. A better translation of the word we meant (in Dutch: “gezellig”) would be ‘sociable’. So we have changed cozy in sociable. We expect that both samples would have understood the word the same as Surinamese in Suriname speak Dutch as well and they should know the word in Dutch (Table 2, page 23).
4. The discussion and introduction areas of this article could be enhanced, by situating this study and its results in the questions raised in point 2. It would be more helpful to a wider audience if some of these issues surrounding social context could be addressed, along with psychosocial characteristics. In addition, it would be useful if the authors could try to offer a comment on generalizability to readers. How do these results matter to others in countries with no similar group (Surinamese), but similar issues of migration and smoking prevalence. Similarly, how does Surinam compare to other countries of origin? (for example, what are the trends in smoking for women and men, history, stage of the epidemic, types of tobacco used other than cigarettes (esp among women), etc?) These questions about both the Netherlands and Surinam need to be placed in a wider context for readers to be able to apply these findings and build on them in other contexts.

5. The limitation is the cross sectional design. The authors identify this. It might help to explore this limitation a bit more in light of trends such as those mentioned above, in both Netherlands and Surinam.

5. We explore the consequences of the cross sectional design now in light of the global smoking patterns, i.e. the tobacco epidemic (page 12).

6. The title and writing are acceptable. The abstract needs to clarify that increase in prevalence refers to the migrant group, not the population of the destination country. This confusion is apparent in a few places in the mss.

6. We mentioned more explicitly that the prevalence figures are about the migrant group on pages 2 and 10.

**Response to your questions:**

- **Please include full details of any ethical approval sought to conduct the study, and full details of informed consent from the participants.**

We included the following text:

In both studies, ethical approval was gotten via the regular procedures during the time of the study. All participants provided a written informed consent (page 7).
- Please also indicate whether the data used in the study is freely available

Data from the CVFRO study are available via the Statistics Bureau for Health Care of Surinam (BOG). Data from the SUNSET study are available at Department of Public Health, Academic Medical Center- University of Amsterdam, The Netherlands after approval of the research question by the SUNSET team (correspondence to: Irene van Valkengoed, PhD; Dept. of Public Health, J2.209; Academic Medical Centre, University of Amsterdami; Amsterdam; The Netherlands; e-mail: i.g.vanvalkengoed@amc.uva.nl).

We hope that our revised manuscript will now be favorably considered for publication.

Yours sincerely, also on behalf of the co-authors,

Vera Nierkens