

## PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (<http://bmjopen.bmj.com/site/about/resources/checklist.pdf>) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Longitudinal Panel Data Study of Self-Rated health among Migrants in French-speaking Switzerland, 2003-2017
<b>AUTHORS</b>	Mota, Pau; Saez, Marc; Selby, Kevin; Bodenmann, Patrick

### VERSION 1 – REVIEW

<b>REVIEWER</b>	Martin Lindström Social Medicine and Health Policy Department of Clinical Sciences in Malmö Lund University Sweden
<b>REVIEW RETURNED</b>	17-Dec-2019

<b>GENERAL COMMENTS</b>	<p>Manuscript: "Longitudinal study of self-rated health among migrants of the CoLaus study in French-speaking Switzerland, 2003-2017"</p> <p>This panel data study from the French speaking part of Switzerland aims to assess the association between self-rated health, country of birth, socioeconomic factors and poor health in a prospective cohort study of adults aged 35-75. The results show that migrant status was not associated with poor SRH. Instead, differences were observed in terms of gender, age, several social determinants. The introduction, hypotheses, methods, results and discussion are mostly sound, and the references seem relevant, However, required improvements needed are listed below.</p> <p>There is no need for further English language examination of this manuscript.</p> <p><b>Title</b> The study design should be given in the title, not only that it is a longitudinal study.</p> <p><b>Abstract</b> The participation rate (%) among those invited to participate may be given in the abstract. Is it really correct to depict this study as a "prospective cohort study" in the abstract when in fact it is a panel data study? A "prospective cohort study" would rather be a study with a baseline and outcomes in the form of diagnosis codes (deaths or incidence of disease) during follow-up time based on survival analysis.</p> <p><b>Introduction</b> The introduction is short and informative, but lacks two important points:</p>
-------------------------	---

The introduction should give a better presentation of the diversity of the composition of the immigrant population in different European countries. In the French part of Switzerland, it is obvious that the immigrant population is dominated by European labor immigrants from other, often neighboring, European countries. In other European countries, the immigrant population is dominated by low-wage labor from countries outside Europe or refugees from countries outside Europe. This means that the health or the self-rated health of immigrants are not always comparable or generalizable to other European countries. The migration process and the health connected with it includes three important steps in the process: 1) The conditions in the of birth before migration, 2) the act of migration itself, and 3) the conditions in the country of immigration.

#### Methods

It seems that the participation rate in the first wave was low, approximately 34% (6,733 of a total 19,830 invited). This low participation rate should be given in per cent units in the beginning of the Methods section, and it may be given in the abstract (see above). Despite the comparatively low participation rate, the final sample of participants seem fairly representative of the population with regard to immigrant status, 40% in wave 1, 37% in wave 2 and 37% in wave 3 compared to 43% in the general population. What is the risk of selection bias, i.e. the risk that there are other directions of associations in the parameter population compared to the final studied sample? Given the low participation rate, the risk of selection bias is not necessarily low.

The self-rated health item with five alternatives is the one used internationally with the same number and content of the alternatives.

The other variables used also seem valid.

The statistics seem generally correct, although I suggest a specialist in Bayesian statistics to also review this manuscript. The description of the statistics entails several question marks including the increase of the level of significance (i.e. alpha), as well as the incorporation of uncertainties such as model uncertainty, missing data and unobserved confounding. Why was the Integrated Nested Laplace Approach (INLA) used in the pure Bayesian framework? This is not explained or motivated in the Methods section or anywhere else, but I think it should be.

#### Results

Table texts should be given above the tables, not below the tables (as opposed to figure texts, which should be given below figures). Table texts should give full information regarding time (when was the study conducted?), place (where was the study conducted?) and person (age and sex characteristics, if present also name/acronym of the population).

#### Discussion

It is not a weakness that the immigrant population in this study is more European and more similar to the native population than in many other European immigrant studies. Instead, the question of generalizability should be discussed. The findings in this study are interesting, but restricted to this population, or maybe also to populations with very similar ethnic compositions. In contrast, the findings of this study cannot be generalized to West European countries or cities with very large populations from e.g. the third

	<p>world or the Balkans. The findings are still interesting, but this should be much more thoroughly discussed.</p> <p>The question regarding selection bias should be discussed as a potential weakness (see Methods section above).</p>
--	---

<b>REVIEWER</b>	<p>Theoni Stathopoulou National Centre for Social Research Athens Greece</p>
<b>REVIEW RETURNED</b>	11-Feb-2020

<b>GENERAL COMMENTS</b>	<p>The references should be updated. The results should be more extensively discussed.</p> <p>The study contributes to literature on migrant health in French speaking Switzerland. The authors argue that «differences in SRH between migrants and natives [have not yet been documented] in Switzerland». However data for Switzerland are available from the European Social Survey since 2002. Several studies analyzing the ESS data have provided documentation about SRH and/or depression in the country (e.g Van de Velde S, Bracke P, Levecque K. Gender differences in depression in 23 European countries. Cross-national variation in the gender gap in depression. SocSci Med 2010;71:305–13; Eikemo TA, Bambra C, Huijts T, Fitzgerald R. The first pan-European sociological health inequalities survey of the general population: the European Social Survey rotating module on the social determinants of health. Eur Sociol Rev2016;33:137–53; Stathopoulou, T., Stornes, P, Mouriki, A, Kostaki, A, Cavounidis, J, Avrami, L, McNamara, C, L, Rapp, C &amp; Eikemo, A.T., (2018) Health inequalities among migrant and native-born population in Greece in times of crisis: The MIGHEAL study. European Journal of Public Health, Vol. 28, sup. 5).</p> <p>Furthermore the bibliographical documentation is limited for some topics (i.e the migration as a SDH) or needs to be updated. The definition of the study population needs further clarification. Is the country of birth a sufficient criterion to designate migrants from natives? What is the status of second-generation migrants born in Switzerland? Should citizenship be used instead? The authors refer to “civil status” in line 201, p.9, without providing any further explanation of its “categories”. Adding contextual information about the country’s migration profile would help the reader better understand the findings of the study. Assessing the results in relation to the “healthy immigrant effect” would provide a more thorough discussion. It would be interesting to know if there is a variation in results recoding SRH’s “fair” to “good” and “very good”. Among the strengths of the study are indeed the big sample size and the treatment of uncertainty due to missing data. The level of significance (alpha) should be specified. Tables or data confirming that the assumptions of the three statistical tests (Student’s t-test, Mann-Whitney U test for quantitative variables and Pearson’s chi-square for qualitative variables) were met could be added to the paper.</p>
-------------------------	--

<b>REVIEWER</b>	<p>Paul Norman University of Leeds, UK</p>
<b>REVIEW RETURNED</b>	10-Mar-2020

**GENERAL COMMENTS**

Longitudinal Study of Self-Rated health among Migrants of the CoLaus study in French-speaking Switzerland, 2003-2017.  
bmjopen-2019-035812

Thank-you sending me this to review (on 04/03/2020 submitted 25/11/2019). The paper is promising and interesting but for me needs some generally straightforward revisions and perhaps, if agreed, some re-running of the models. There are some typos which I will not detail here on the grounds that a re-review is likely to be needed but do encourage the authors to carefully copy edit.

Some detailed comments

There needs to be better differentiation between immigration and subnational migration both of which have had attention in the literature. Yes I do get that this is about immigrants compared with Swiss born. Re p 4 line 85 and p 5 line 91 almost everyone will have experienced a migration process (but only some internationally). (Since the authors evidently have an interest in CVD, they may be interested now or for future work in Shackleton et al. (2018) which also notes the stress of the migration event itself which is relevant to the authors' background discussions here.)

P 5 line 111 onwards. Please make it clearer at this point when the SRH is captured and at what time-point this is the modelled outcome of interest. Also, I may have missed it but is health status captured at baseline and has this been controlled for? If not, this needs reflection on in the discussion. Many longitudinal studies of health-selective migration specify healthy at the outset (e.g. Norman et al., 2005 and follow-ups; though sub-national studies).

In the same section, there are other health conditions mentioned but then not in the tables reporting the models. For me, there is sufficient potential by concentrating on SRH and p 11 lines 233 could be cut.

P 6 lines 117-8 this sentence is out of place, I think.

P 6 lines 126-7 since you don't report, the first few tests are not needed to be listed (for me).

The tricky variable for me which, until more convincing or adjusting the categories, is the years living in Switzerland. There are several aspects here. Tables 1 and 3 have both different reference levels and different categories. In terms of the literature, if I recall correctly, the main interests are in recent immigrants (health in the previous few years) and longer-term (whether health converging with indigenous). The base levels of <25 and < 32 are just too long (for me). Also, surely the length of time for the further categories are simply reflecting people's ages? I would suggest being able to differentiate Swiss / Non-Swiss, recent migrants and perhaps continuous (rather than categorical) for time in Switzerland.

I wonder if there are too many effectively overlapping explanatory variables; i.e. Socio-Economic Status, Education, Job Type, Current professional status and Income? The relatively small numbers in Current professional status mean that patterns are hard to see and lead to non-significant inevitably. I wonder if this could be dropped.

	<p>The paper is therefore in a situation whereby a little less and simpler could readily come out as being more. I look forward to seeing a revision after the authors have reflected a little.</p> <p>&amp; see  Norman P, Boyle P &amp; Rees P (2005) Selective migration, health and deprivation: a longitudinal analysis. <i>Social Science &amp; Medicine</i> 60(12): 2755-2771</p> <p>Shackleton N, Darlington-Pollock F, Norman P, Jackson R, &amp; Exeter D (2018) Longitudinal deprivation trajectories and risk of cardiovascular disease in New Zealand. <i>Health &amp; Place</i> 53: 34-42  <a href="https://doi.org/10.1016/j.healthplace.2018.07.010">https://doi.org/10.1016/j.healthplace.2018.07.010</a></p>
--	--

<b>REVIEWER</b>	Arlette Ngoubene-Atioky Goucher College, U.S.
<b>REVIEW RETURNED</b>	19-Mar-2020

<b>GENERAL COMMENTS</b>	<p>Overall, it is a good that this type of research is starting to emerge in Switzerland. However, I wonder if it is the correct focus when exploring self-reported health (SRH).</p> <p>What is missing is how discrimination, biases, and stereotypes of migrants affect their SRH (considering historical legislation and political main powers in Switzerland about immigration/foreigners) and how this is certainly linked to low SES in some migrants.</p> <p>It also does not talk about the differences within migrant communities (intersectionality) which influence who feels better than others. I am a bit curious about the decision to focus solely on SES and mental health symptoms when globally it is more an outcome of societal issues affecting immigrants' SRH. This focus makes it so that it is a problem with the immigrants solely (they are poor, mentally ill individuals) rather than a societal problem --&gt; there needs to be at least a line on this in the introduction or indication of what theory is grounding your research.</p> <p>The introduction does not mention at all about the immigrant health effect, which has been corroborated in studies cross-culturally -&gt; the introduction needs to go beyond Europe (unless this is a European-focused journal?) in exploring self-reported health</p> <p>Where are the hypothesis and research questions? - those needed to be clearly stated</p> <p>line 19 - missing "of"</p> <p>line 141 - what confounding variables were tested? be specific and link it to the table (are the variables in the table all the ones used in this study?) Was difference in country of origin among immigrants explored? Was race/ethnic group explored? Was sex and gender identity also considered?</p> <p>173-174, 184-190 - due to the limited consideration of other factors in the intro, the results are not surprising.</p> <p>175-178 - which is also found in many other research and speaks to intersectionality (age and immigrant status)</p>
-------------------------	--

	<p>178-183 - the immigrant health effect. This is not described in the discussion section.</p> <p>239-242 - so, you are inferring that being a single female migrant parent puts you at higher risk of lower SRH than being a migrant family (couple with child)? Keeping in mind that you found that migrant couples with children had lower SRH... This is confusing and does not corroborate well your finding. Think about the construct of childfree status - there are several articles on this.</p> <p>243-244 - absolutely and more research and understanding of the construct of SRH in the introduction would make this study more robust.</p> <p>245-249 -- missing that you offered very limited attention to why there is low SES among migrants in Switzerland, what factors compounded it (Swiss climate), and the intersectionality (child status, age, sex, experienced discrimination, country of origin, race/ethnic group, education level) of immigrants that speak very much about SRH and does not seem to have been studied here).</p> <p>Reviewing the first table, I am concerned of missing data for SES, monthly gross income - how did that influence what and the type of results you found (a limitation?)? Also, gender is inaccurately defined (missing trans or other category), alcohol consumption is also a concern (a yes does not mean much as it does not speak of the quantity/extreme consumption). Linking the tables to the results section will be extremely beneficial and explaining in the results what each focuses on would be helpful (table 2 and 3)</p>
--	---

### VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: Martin Lindström

Institution and Country: Social Medicine and Health Policy

Department of Clinical Sciences in Malmö

Lund University

Sweden

Please state any competing interests or state 'None declared': None declared.

Please leave your comments for the authors below

Manuscript: "Longitudinal study of self-rated health among migrants of the CoLaus study in French-speaking Switzerland, 2003-2017"

This panel data study from the French speaking part of Switzerland aims to assess the association between self-rated health, country of birth, socioeconomic factors and poor health in a prospective cohort study of adults aged 35-75. The results show that migrant status was not associated with poor SRH. Instead, differences were observed in terms of gender, age, several social determinants. The introduction, hypotheses, methods, results and discussion are mostly sound, and the references seem relevant. However, required improvements needed are listed below.

There is no need for further English language examination of this manuscript.

Title

The study design should be given in the title, not only that it is a longitudinal study.  
Thanks. Done.

#### Abstract

The participation rate (%) among those invited to participate may be given in the abstract.  
Is it really correct to depict this study as a “prospective cohort study” in the abstract when in fact it is a panel data study? A “prospective cohort study” would rather be a study with a baseline and outcomes in the form of diagnosis codes (deaths or incidence of disease) during follow-up time based on survival analysis.  
Thanks. Done.

#### Introduction

The introduction is short and informative, but lacks two important points:  
The introduction should give a better presentation of the diversity of the composition of the immigrant population in different European countries. In the French part of Switzerland, it is obvious that the immigrant population is dominated by European labor immigrants from other, often neighboring, European countries. In other European countries, the immigrant population is dominated by low-wage labor from countries outside Europe or refugees from countries outside Europe. This means that the health or the self-rated health of immigrants are not always comparable or generalizable to other European countries.  
We agree with the reviewer in this statement. Nevertheless, we just mention that research on Self-rated health in migrant’s populations has been done in neighbour European countries in the last 20 years but not in Switzerland where there are just few articles on migration and health. We fully agree that the composition of immigrant population in different European countries is very different and this is why we do not want to compare countries or make generalisation and just focus on the French part of Switzerland.

The migration process and the health connected with it includes three important steps in the process: 1) The conditions in the of birth before migration, 2) the act of migration itself, and 3) the conditions in the country of immigration.

We agree on this point and we also include that the migration process is often not only a displacement from one to another but several countries or places are often involve in the same journey. Several migrants have spent several years in other countries than the country of birth and even when established in the country of immigration the trips back and forward from other countries make the process even more complex. Within this framework, we agree completely with the statement of the reviewer. Unfortunately, we just have the data available once the migrants are establish in the country of immigration as they participate in the study but similar data gather before the migration process would allow us to better understand the effect of the migration process itself on the health of the migrant, if any.

#### Methods

It seems that the participation rate in the first wave was low, approximately 34% (6,733 of a total 19,830 invited). This low participation rate should be given in per cent units in the beginning of the Methods section, and it may be given in the abstract (see above). (Thanks. Done) Despite the comparatively low participation rate, the final sample of participants seem fairly representative of the population with regard to immigrant status, 40% in wave 1, 37% in wave 2 and 37% in wave 3 compared to 43% in the general population.  
What is the risk of selection bias, i.e. the risk that there are other directions of associations in the parameter population compared to the final studied sample? Given the low participation rate, the risk of selection bias is not necessarily low.

As we say in the text, the Colaus cohort is a simple, non-stratified random sample (lines 82, 83).

Selection bias will occur not when the sample is small but when it is not random. In our case, we avoid selection bias as we use a random sample. However, it is true that some subjects may have been lost to follow-up (drop-out), which would distort the randomness of the initial sample. For this reason, we control for possible confounders (both observed and unobserved) in a multivariate model (i.e. logistic regression).

However, the reduced sample size, even being a random sample, implies a reduced statistical power. For this reason, we increase the probability of making a type I error (risk) (lines 142 to 146).

The other variables used also seem valid.

The statistics seem generally correct, although I suggest a specialist in Bayesian statistics to also review this manuscript. The description of the statistics entails several question marks including the increase of the level of significance (i.e. alpha), as well as the incorporation of uncertainties such as model uncertainty, missing data and unobserved confounding. Why was the Integrated Nested LaPlace Approach (INLA) used in the pure Bayesian framework? This is not explained or motivated in the Methods section or anywhere else, but I think it should be.

One of us is already an expert in Bayesian statistics. We have attempted to clarify this point in the new version of the manuscript. Lines 146 to 150,

## Results

Table texts should be given above the tables, not below the tables (as opposed to figure texts, which should be given below figures). Thanks. Done.

Table texts should give full information regarding time (when was the study conducted?), place (where was the study conducted?) and person (age and sex characteristics, if present also name/acronym of the population). Thanks. Done.

## Discussion

It is not a weakness that the immigrant population in this study is more European and more similar to the native population than in many other European immigrant studies. Instead, the question of generalizability should be discussed. We agree. Discussion text has been modified. The findings in this study are interesting, but restricted to this population, or maybe also to populations with very similar ethnic compositions. In contrast, the findings of this study cannot be generalized to West European countries or cities with very large populations from e.g. the third world or the Balkans. The findings are still interesting, but this should be much more thoroughly discussed. We agree.

Discussion text has been modified.

The question regarding selection bias should be discussed as a potential weakness (see Methods section above).

We agree on this point too and we include the likely selection bias as a weakness of the study.

Reviewer: 2

Reviewer Name: Theoni Stathopoulou

Institution and Country: National Centre for Social Research

Athens

Greece

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

The references should be updated. Please see additional info in the file attached. The results should be more extensively discussed.

Thank you very much for these valuable comments. Even if the literature on the social determinants of health in Switzerland is quite large and one of the co-authors (P. Bodenmann) being the author of

several articles on this topic, few articles focus on Self-rated health and migration in Switzerland. The references added by the reviewer will be evaluated to be included in the article if need it be. The rest of the comments will be also take into consideration.

Reviewer: 3

Reviewer Name: Paul Norman

Institution and Country: University of Leeds, UK

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

Longitudinal Study of Self-Rated health among Migrants of the CoLaus study in French-speaking Switzerland, 2003-2017. [bmjopen-2019-035812](https://doi.org/10.1136/bmjopen-2019-035812)

Thank-you sending me this to review (on 04/03/2020 submitted 25/11/2019). The paper is promising and interesting but for me needs some generally straightforward revisions and perhaps, if agreed, some re-running of the models. There are some typos which I will not detail here on the grounds that a re-review is likely to be needed but do encourage the authors to carefully copy edit.

Some detailed comments

There needs to be better differentiation between immigration and subnational migration both of which have had attention in the literature. Yes I do get that this is about immigrants compared with Swiss born. Re p 4 line 85 and p 5 line 91 almost everyone will have experienced a migration process (but only some internationally). (Since the authors evidently have an interest in CVD, they may be interested now or for future work in Shackleton et al. (2018) which also notes the stress of the migration event itself which is relevant to the authors' background discussions here.) We agree on the comments on the migration process and that Switzerland and the panel data used for this study has a high rate of non-Swiss born compared to other countries. In our opinion, this is one of the strengths of our study. We are not particularly interested on the CVD what is the main and original focus of the CoLaus study but on using the panel data to study migration and the social determinants of health. The large number of the sample and the longevity of the CoLaus study (almost 17 years) make it unique in Switzerland and give us the opportunity to compare the evolution of the Self Rated Health between those who had experienced an international migration process and those who do not. The subnational migration is out of the scope of this study.

P 5 line 111 onwards. Please make it clearer at this point when the SRH is captured and at what time-point this is the modelled outcome of interest. Done P5 line 102.

Also, I may have missed it but is health status captured at baseline and has this been controlled for? If not, this needs reflection on in the discussion. Many longitudinal studies of health-selective migration specify healthy at the outset (e.g. Norman et al., 2005 and follow-ups; though sub-national studies).

We agree on this latter point too and we thank you to make us aware of it. We include this comment in the discussion as a weakness of the study.

In the same section, there are other health conditions mentioned but then not in the tables reporting the models. For me, there is sufficient potential by concentrating on SRH and p 11 lines 233 could be cut. Thanks. Done.

P 6 lines 117-8 this sentence is out of place, I think. Thanks. Removed.

P 6 lines 126-7 since you don't report, the first few tests are not needed to be listed (for me). Thanks. Removed.

The tricky variable for me which, until more convincing or adjusting the categories, is the years living in Switzerland. There are several aspects here. Tables 1 and 3 have both different reference levels and different categories. In terms of the literature, if I recall correctly, the main interests are in recent immigrants (health in the previous few years) and longer-term (whether health converging with indigenous). The base levels of <25 and < 32 are just too long (for me). Also, surely the length of time for the further categories are simply reflecting people's ages? I would suggest being able to differentiate Swiss / Non-Swiss, recent migrants and perhaps continuous (rather than categorical) for time in Switzerland.

This is a very good point and we thank the reviewer for raising the point. We have included this point as a limitation in the discussion.

I wonder if there are too many effectively overlapping explanatory variables; i.e. Socio-Economic Status, Education, Job Type, Current professional status and Income? The relatively small numbers in Current professional status mean that patterns are hard to see and lead to non-significant inevitably. I wonder if this could be dropped.

It overlaps in some respects, but not in most. In emigrants, a high educational level does not always correspond to a higher job category and, therefore, with higher incomes.

The paper is therefore in a situation whereby a little less and simpler could readily come out as being more. I look forward to seeing a revision after the authors have reflected a little.

& see

Norman P, Boyle P & Rees P (2005) Selective migration, health and deprivation: a longitudinal analysis. *Social Science & Medicine* 60(12): 2755-2771 .

Shackleton N, Darlington-Pollock F, Norman P, Jackson R, & Exeter D (2018) Longitudinal deprivation trajectories and risk of cardiovascular disease in New Zealand. *Health & Place* 53: 34-42  
<https://doi.org/10.1016/j.healthplace.2018.07.010>

Thank you for these excellent references. First one Included in the discussion

Reviewer: 4

Reviewer Name: Arlette Ngoubene-Atioky

Institution and Country: Goucher College, U.S.

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

Overall, it is a good that this type of research is starting to emerge in Switzerland. However, I wonder if it is the correct focus when exploring self-reported health (SRH).

What is missing is how discrimination, biases, and stereotypes of migrants affect their SRH (considering historical legislation and political main powers in Switzerland about immigration/foreigners) and how this is certainly linked to low SES in some migrants.

It also does not talk about the differences within migrant communities (intersectionality) which influence who feels better than others. We agree that this analysis would be of interest, we thought about it before running the models, nevertheless the small number of cases within each country of birth, and the nature of the data doesn't allow us to do them.

I am a bit curious about the decision to focus solely on SES and mental health symptoms when globally it is more an outcome of societal issues affecting immigrants' SRH. We completely agree on this and we try to analyse the weight of these different variables to the SRH. The results of the study enforce this opinion too. This focus makes it so that it is a problem with the immigrants solely (they

are poor, mentally ill individuals) rather than a societal problem --> there needs to be at least a line on this in the introduction or indication of what theory is grounding your research. The results and the discussion of the study focus on the social determinants of health rather than migration so it meets the same argument.

The introduction does not mention at all about the immigrant health effect, which has been corroborated in studies cross-culturally -> the introduction needs to go beyond Europe (unless this is a European-focused journal?) in exploring self-reported health

Where are the hypothesis and research questions? - those needed to be clearly stated  
This study is an exploratory assessment of the Self Rated Health between people born in Switzerland and people born abroad participating in the CoLaus study. It is an adhoc study and the hypothesis and research questions are imbedded in the objective of the study.

line 19 - missing "of" Thanks. Added.

line 141 - what confounding variables were tested? be specific and link it to the table (are the variables in the table all the ones used in this study?) Was difference in country of origin among immigrants explored? Was race/ethnic group explored? Was sex and gender identity also considered?

As we write in our manuscript (lines 119-121)

The control variables were gender, age, the Socio Economic Status (SES) following the Hollingshead scale, the educational level, job type, current professional status, monthly household gross income and alcohol consumption.

Furthermore, in lines 135 to 140 we explain how we control the unobserved confusion

The control variables were gender, age, the Socio Economic Status (SES) following the Hollingshead scale, the educational level, job type, current professional status, monthly household gross income and alcohol consumption.

173-174, 184-190 - due to the limited consideration of other factors in the intro, the results are not surprising.

Thank you for the insight.

175-178 - which is also found in many other research and speaks to intersectionality (age and immigrant status)

We agree on this.

178-183 - the immigrant health effect. This is not described in the discussion section.

Literature on the healthy migrant effect has been added.

239-242 - so, you are inferring that being a single female migrant parent puts you at higher risk of lower SRH than being a migrant family (couple with child)? Keeping in mind that you found that migrant couples with children had lower SRH... This is confusing and does not corroborate well your finding. Think about the construct of childfree status - there are several articles on this.

Thank you for the insight. Would be interesting to read them and go through them but this goes beyond the scope of this article.

243-244 - absolutely and more research and understanding of the construct of SRH in the introduction would make this study more robust.

Thanks for the insight.

245-249 -- missing that you offered very limited attention to why there is low SES among migrants in

Switzerland, what factors compounded it (Swiss climate), and the intersectionality (child status, age, sex, experienced discrimination, country of origin, race/ethnic group, education level) of immigrants that speak very much about SRH and does not seem to have been studied here).

This could be link to the selection bias of the migrants in the sample of the CoLaus study. Migrants with lower SES are not participating in the study.

Reviewing the first table, I am concerned of missing data for SES, monthly gross income - how did that influence what and the type of results you found (a limitation?)? Also, gender is inaccurately defined (missing trans or other category), alcohol consumption is also a concern (a yes does not mean much as it does not speak of the quantity/extreme consumption). Linking the tables to the results section will be extremely beneficial and explaining in the results what each focuses on would be helpful (table 2 and 3)

This is an adhoc study and even if we share, several of these limitations on the current data we could not redefined any of these variables.

### VERSION 2 – REVIEW

<b>REVIEWER</b>	Paul Norman School of Geography University of Leeds
<b>REVIEW RETURNED</b>	16-Apr-2020

<b>GENERAL COMMENTS</b>	Thanks very much for promptly and clearly addressing the comments made by the referees. For my own comments and, as far as I can tell, the other reviewers' comments, I believe you have responded well and have adjusted both the text and presentation of materials as well as is possible given the constraints of the data, etc., which you acknowledge.
-------------------------	--

<b>REVIEWER</b>	Arlette J. Ngoubene-Atioky Goucher College, U.S.
<b>REVIEW RETURNED</b>	29-Apr-2020

<b>GENERAL COMMENTS</b>	The article is critical in offering some understanding of the factors related to the wellbeing of Swiss migrants. Part of the limitations that have been critical to the study of the wellbeing of migrants are the lack of focus/attention about acculturation, intergroup relations, experienced discrimination which may impact directly or indirectly the relationship between level of education/age and SRH of migrants. Other factors for migrant couples with children could be the availability of services and support for children rearing, etc. Without any mention or inclusion of issues, the study has a concerning construct/vision of migrants (lower education leads to lower SRH --- the picture is usually more complex and this needs to be stated). This needs to be explicitly included in the explanation of the results and/or the section limitations.
-------------------------	--

### VERSION 2 – AUTHOR RESPONSE

Dear reviewer,

Thank you very much for your valuable comments. We agree that these are limitations of the study and we update the manuscript accordingly.