Reviewer’s report

Title: Challenges and benefits of integrating diverse sampling strategies in the Observation of Cardiovascular Risk Factors (ORISCAV-LUX 2) study

Version: 0 Date: 05 Nov 2018

Reviewer: Reviewer 2

Reviewer’s report:

PEER REVIEWER ASSESSMENTS:

OBJECTIVE - Full research articles: is there a clear objective that addresses a testable research question(s) (brief or other article types: is there a clear objective)?

No - there are minor issues

DESIGN - Is the current approach (including controls and analysis protocols) appropriate for the objective?

Yes - the approach is appropriate

EXECUTION - Are the experiments and analyses performed with technical rigor to allow confidence in the results?

Yes - experiments and analyses were performed appropriately

INTERPRETATION - Is the current interpretation/discussion of the results reasonable and not overstated?

No - there are minor issues

OVERALL MANUSCRIPT POTENTIAL - Could an appropriately REVISED version of this work represent a technically sound contribution?

Yes - current version is technically sound

PEER REVIEWER COMMENTS:
GENERAL COMMENTS: The study describes and discusses the different sampling strategies adopted in and the operational issues associated with a population-based study of cardiovascular risk factors in Luxemburg (ORISCAV-LUX). The study uses data from 2 waves of the survey (2007-09 and 2016-17) and focuses on assessing population coverage (by comparing known socio-demographic and health risk profiles of participants and non-participants) and sample representativeness (by comparing the 2nd wave sample to the Luxemburg national population).

The paper is generally well written and easy to understand, though a few parts appear somewhat redundant. I have no concern regarding methodology or statistical analysis. There are, however, a few minor issues with the reporting of results that need to be addressed. Moreover, there seems to be occasional over-reliance on p-values rather than effect sizes when interpreting the results. Please see below for specific comments.

REQUESTED REVISIONS:

Specific comments:

1. (Abstract/Objective). The wording of the objective in the abstract is too complicated and not easy to understand at first read. The authors should consider rephrasing this into a testable research question(s), in alignment with the statements in the main text (background last paragraph).

2. (All manuscript/reporting of p-values). Actual P-values (rounded to 3 decimal places) should be reported in text rather than expressing statements of inequality (P < .05), unless P < .001. The principle is that not everyone takes a strictly Pearsonian view of probabilities as absolutely, categorically significant or absolutely, categorically non-significant. For example, P values of 0.051 and 0.049 should be interpreted similarly despite the fact that the 0.051 is greater than 0.05 and is therefore not "significant" and that the 0.049 is less than 0.05 and thus is "significant." Reporting actual P values avoids this problem of interpretation. Moreover, P values should not be listed as not significant (NS) or in terms of inequalities because, for meta-analysis, not providing exact P values is a form of incomplete reporting. Also, P values less than .001 should be reported as P < .001. Expressing P to more than 3 significant digits does not add useful information as extreme results are sensitive to biases or departures from the statistical model.

3. (Abstract /results). Please report sample sizes separately for each group being compared.

4. (Abstract /results). The authors rely solely on P-values to report differences between survey participants with non-participants; this is difficult to understand, because p-values are just one piece of information. However, p-values are a function of sample size; with a large enough sample size even small, unimportant differences, will produce low p-values. As P-values tell nothing about effect sizes or sizes of differences, the authors should report summary statistics for the comparisons together with p-values.
5. (Abstract /results + results page 6). In relation to the comment above, the authors report that "participants were significantly younger" (compared to non-participants). But this is less than 2 years difference in average ages (44 vs 45.8) - is this a practically important difference in age between the two groups that one should take seriously into account? Similarly, I would also like to see some consideration of effect sizes in relation to the comparison of self-reported health perception.

6. (Abstract /results). A single p-value for cardio-metabolic profile makes no sense, as the profile consists of several factors.

7. (Methods / alternative strategies). The reasoning for the sample size (n=4737) sought from the CTIE is clear, but why was a substantially lower sample size (n=1431) sought from the EHES-LUX?

8. (Methods / sample size). Sufficient references have been provided to previous papers reporting the study design protocol for the ORISCAV-LUX. However, in methods, under "alternative strategies", the reader of this paper is informed about a required minimum sample of 4737 subjects, which sounds large. If I understand correctly, this accounts for a large anticipated non-participation rate and is not the minimal sample size required for adequate statistical power. It may be helpful if the latter is reported in this paper.

ADDITIONAL REQUESTS/SUGGESTIONS:

Specific comments on issues of presentation:

9. (Background). First two paragraphs report common knowledge and appear redundant.

10. (Results, page 7, line 25). The age groups reported here are not consistent to those reported in Table 3.

11. (References) References 5 and 6 cite the same paper twice. Please correct.

12. (Discussion). First paragraph does not seem to add much to the discussion and could be deleted.

Note: This reviewer report can be downloaded - see attached pdf file.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.
Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

No

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

Not relevant to this manuscript

Quality of written English
Please indicate the quality of language in the manuscript:

Acceptable

Declaration of competing interests
Please complete a declaration of competing interests, considering the following questions:

1. Have you in the past five years received reimbursements, fees, funding, or salary from an organisation that may in any way gain or lose financially from the publication of this manuscript, either now or in the future?

2. Do you hold any stocks or shares in an organisation that may in any way gain or lose financially from the publication of this manuscript, either now or in the future?

3. Do you hold or are you currently applying for any patents relating to the content of the manuscript?

4. Have you received reimbursements, fees, funding, or salary from an organization that holds or has applied for patents relating to the content of the manuscript?

5. Do you have any other financial competing interests?

6. Do you have any non-financial competing interests in relation to this paper?

If you can answer no to all of the above, write 'I declare that I have no competing interests' below. If your reply is yes to any, please give details below.

This reviewer has been recruited by a partner organization, Research Square. Reviewers with declared or apparent competing interests are not utilized for these reviews. This reviewer has agreed to publication of their comments online under a Creative Commons Attribution License attributed to Research Square and was paid a small honorarium for completing the review within
a specified timeframe. Honoraria for reviews such as this are paid regardless of the reviewer recommendation.

I agree to the open peer review policy of the journal. I understand that my name will be included on my report to the authors and, if the manuscript is accepted for publication, my named report including any attachments I upload will be posted on the website along with the authors' responses. I agree for my report to be made available under an Open Access Creative Commons CC-BY license (http://creativecommons.org/licenses/by/4.0/). I understand that any comments which I do not wish to be included in my named report can be included as confidential comments to the editors, which will not be published.

I agree to the open peer review policy of the journal