Reviewer's report

Title: Individual, Social Environmental and Physical Environmental Correlates with Physical Activity among Canadians: a Cross-sectional Study

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Reviewer: Ester Cerin

Reviewer's report:

This manuscript uses data from a large national survey of adult Canadians conducted in 2002 to examine the independent associations of individual, social, and environmental factors with meeting the current physical activity guidelines for health benefits. The findings reported in the manuscript have implications for the planning of intervention strategies for promoting engagement in physical activity in Canada. Additionally, this study provides further support for a social-ecological approach to the study of physical activity behavior. Consequently, I think that this is an article of potential importance in its field.

I have several suggestions that may help improve the usefulness and presentation of the findings of this manuscript. These are outlined below.

Major compulsory revisions:

1. Following Principal Components Analysis, factor scores of individual, social, and environmental factors were used in the logistic regression analyses. Although this is statistically acceptable, such an approach makes interpretation of the findings more difficult. This is because factor scores do not use the same metric of the original scales. When using the original scale metric, we can easily interpret and understand the meaning of regression coefficients, in this case representing the difference or increase in the dependent variable associated with a unit increase in the independent variable or factor on the original Likert scale. With factor scores we cannot do this as they are standard normal deviates with mean 0 and standard deviation 1. The meaning of a factor-score unit depends on the variability of responses on the items composing a factor. I therefore suggest that the authors consider redoing the analyses using more-easily interpretable composite scores for each factor. It would be optimal if they simply computed the mean response on the items for each factor. I understand that this is going to increase the correlation between the predictors and that, for this reason, some of the significant independent associations may become non-significant. However, this type of analysis would provide more interpretable and 'realistic' results. The fact that some associations may become non-significant is not a limitation as many of the factors considered in this study are theoretically expected to overlap (e.g., barriers and self-efficacy). The use of factor scores in regression analyses may resolve the problem of shared variance and collinearity but, in my opinion, hinders our understanding of the factors associated with physical activity participation and their inter-relationships.
2. Use of factor quartiles: Given that most relationships between factors and odds of engaging in sufficient PA appear to be approximately linear (rather than curvilinear), I suggest the authors avoid using quartiles in their analyses. The use of a continuous score should provide more statistical power. Also, quartiles do not provide clear information on the actual magnitude of the effect (i.e., what is the value range in a quartile?).

3. Logistic regressions: The authors conducted logistic regression analyses for the whole sample and by sex, age groups, and educational attainment. I think that considering the moderating effects of sex, education and age is important. However, to examine moderation effects, the authors should test the statistical significance of interaction terms. Stratification does not provide a direct answer to the issues of moderating effects. With stratification we can identify a difference in regression coefficients, but we do not know whether this difference is ‘reliable’ (i.e., statistically significant). I suggest the authors do the following: (1) report the main effect model for the entire sample; (2) examine the significance of the interaction effects of sex, age, education by factors on PA; (3) report only the significant interaction effects.

3. Non-adjustment for income in logistic regression models: although educational attainment and income are positively correlated, it would be useful to examine how these independently contribute to meeting the PA guidelines. Education and income appear to affect PA through common as well as distinct mechanisms (see our recent paper in Social Science and Medicine). If the extent of overlap between education and income is too high (collinearity problems), the authors may state this and opt to use education as the only socio-economic indicator in the regression analyses.

Minor essential revisions:

1. There are several social-ecological models of health behavior. Hence, on page 3 of the Background section, it would be appropriate for the authors to state that ‘Social-ecological models propose that health behaviors …’

2. On page 4 (research questions), it would be better to say that this study examined the independent contributions or adjusted effects rather than direct effects of individual, social, environmental factors on PA. ‘Direct’ effects refer to the unmediated effects of a factor. However, the authors did not examine mediated effects. Hence, it is somewhat inappropriate to talk about direct effects.

3. Were any of the self-report measures of individual, social, and environmental factors based on previously validated scales? If so, could the authors report those? If not, the authors should state that the scales were developed by XX for the purpose of the national survey.

4. Social environment variables: I can count 8 items. On page 8 the authors mention 10 items. I assume this is a typo.

5. Physical environment variables: Was a definition for neighborhood provided?
6. Policy variables: I agree that actual policy can affect physical activity levels in populations. However, I cannot see how participants’ opinions about whether the government should be responsible for PA-related initiatives can be a measure of policy factors. The questions listed in this section measure opinions about appropriate levels of government responsibility. In my view, these questions are related to participants’ attitude towards physical activity rather than to actual or perceived policy factors (i.e., participants that adopt or would like to adopt an active lifestyle might feel that the government should take responsibility for providing an environment conducive to PA). I suggest the authors omit these variables from their analyses as these do not correspond to the true meaning of ‘policy’ from a social-ecological model perspective.

7. Statistical analyses: Could the authors explain why they chose eigenvalues greater than 1.4 as a criterion for retaining a factor in their Principal Components Analysis? It is common to adopt eigenvalues > 1.0.

8. Table 2: Did the authors test the significance of the unadjusted associations between these variables and PA measures? It would be useful to have these reported.

9. It would be useful to have a table with the descriptives (mean, SD, and, if needed, median and IQR) for the independent variables. Also, please provide the median and IQR for the PA variables.

10. Discussion - Page 12: the authors may also consider that self-efficacy is also a result (not only a determinant) of engagement in PA. Claudio Nigg’s study indicated that changes in PA affect self-efficacy rather than the opposite.

11. Discussion – Page 13: The greater effects of barriers on PA in women than in men may be also due to men enjoying PA more than women; to some men having higher levels of occupational PA than women (while perceived barriers to PA are usually understood within a leisure-time context).

12. Discussion – Page 13: Mediating variable analyses can be also assessed without using structural equation modeling.

Level of interest: An article of importance in its field

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.