Author's response to reviews

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

Authors:

Sai Yi Pan (sai_yi_pan@phac-aspc.gc.ca)
Christine Cameron (cccameron@cflri.ca)
Marie DesMeules (marie_desmeules@phac-aspc.gc.ca)
Howard Morrison (Howard_Morrison@phac-aspc.gc.ca)
Cora Lynn Craig (cc craig@cflri.ca)
Xiaohong Jiang (Xiaohong_Jiang@phac-aspc.gc.ca)

Version: 2 Date: 5 September 2008

Author's response to reviews: see over
September 4, 2008

RE: #2056753089207845 "Influences of individual, social environment and physical environment on physical activity participation among Canadians: a cross-sectional study"

Dear Editors,

Thank you very much for your letter of August 5, 2008 inviting us to submit a revised version of above-mentioned manuscript. We have revised the manuscript according to reviewers’ comments. The detailed response to the comments is attached below. We hope that the revised manuscript is now suitable to be published in BMC Public Health.

Sincerely yours,

Sai Yi Pan, M.D.
Public Health Agency of Canada

Reviewer's report: Jorge Mota

This study addressed a timely issue but often described in the literature. While the study has some merits it has different points that need a clarification before the paper is suitable for publication. Some comments and possible amendments are placed below:

General comments

Short version of IPA doesn’t assess different dimensions of Pa such as recreational, …….We only get total PA values.
Response: Yes, the short version of IPA doesn’t assess different domains of PA and we get total PA only. We have rewritten the sentence in the abstract to make this clearer (page 2).

In general the authors are too many references in some parts that could be clearly deleted because they are just an over reporting citation , which, from my pint of view makes any sense
Response: We have now deleted some references according to the reviewer’s comments.

Specific comments
Page 3 , 1st par. Authors mentioned older adults, why. It doesn’t make sense in this context.
Response: We mentioned older adults because those additional benefits of regular PA are more important for seniors and mainly demonstrated among seniors, for example, increasing ability to maintain functional independence and
decreasing risk of falls and fractures are more important benefits for seniors than for younger people. We have changed the phrase “older adults” to “seniors” to make it clearer (page 3).

The end of 2nd parg needs a reference
Response: A reference has been added (page 3).

What the real problem under study is? The perceptions? or how the variables from the ecological model affects PA? Studies about environmental features and policy are well known in the literature even in Canadian population.
Response: We have now added that how the variables from the ecological models affects PA (page 3-4). Although studies about environmental features and policy are well known in the literature, researches using multilevel approaches on PA correlates are less common in Canada than in other countries. We have revised the paragraph accordingly (page 4-5).

If, as stated, one of the aims is to look at these variables influences on PA by gender, age and education, this points should be raised before instead to try to explain the importance of study for the lack of information. This is a big epidemiological study, which is worthy. So, the reader needs to understand how the variables under study are related or not so far and the relevance of its interpretation at individual and population level.
Response: We have now raised the point according to the reviewer’s comment (page 4).

Results- the age groups should be clarified. The idea of one group comprising individuals aged 15 until 40 years old has several implication even with regard to results interpretation.
Response: We agree that the 15-year-olds are different from the 30-year-olds, therefore we redid the stratified analysis with 4 age groups: 15-19, 20-39, 40-64 and 65-79 years.

I the methodological approach included a questionnaire that assessed different points such as self-efficacy, facility availability and so forth why are the authors carried out a exploratory factor analysis?
Response: Variables such as self-efficacy, perceived health benefits of PA, perceived barriers to PA, social support, facility availability were assessed by many items. Using the factor analysis to get a single score from these many items for a variable enabled us to assess the overall effect of a variable on PA.

In statistical analysis the expression “table 1” is repeated.
Response: We have now removed the expression “table 1” as reviewer suggested.

Discussion should be enhances. I do think that authors might increase their discussion points if they just try to pointed-out the novelty of their findings and
what they can bring for further developments in this area. I do think that just interpret their data by comparison with those that agree or disagree is far behind the potential of this data base.
Response: As reviewer suggested, we have now increased our discussion on implication for further research and for intervention strategy to promote PA.

I don’t be sure that authors can use the correct interpretation for the differences of PA according the dimensions analysed (please see limitations). At least they need to be coherent regardless they talk about men or women. Once again the problem of the age groups need a strong comment in this analysis (15 years-old is completely different of 25, 30 and 40 years-old).
This is true for other issues. How can a 15 years old feels more barriers and have less free time than older ones.
Response: We have now redone the analyses on 4 age groups: 15-24, 25-39, 40-64 and 65-79. People over 65 years old are usually retired so they usually have more free time than younger people. However, less free time is only one of the barriers to PA. We have revised the paragraph accordingly.

This study addresses several variables and I do feel that would be more interesting to make a global point of the findings found. Authors might make the point of the most important and new outcomes using them to give potential advices for further studies, developments or intervention strategies (for instance perceived health benefits was not related to PA. Authors stated that limited item for assess perceived PA benefits would be a limitation. Why is this not true for other positive interactions?)
Response: According to reviewer’s comment, we have now added discussion on implication of our study results for further studies and developments of intervention strategies.
Authors stated as a limitation that the survey assessed total PA only…. This is true so change the description in abstract as my previous comment
Response: We agree with the reviewer and have made the change (please see response to the first of the general comments) (page 2).

Reviewer’s report: Ester Cerin
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.

Response: Although factor-score is less easily interpretable, it takes into consideration of shared variance and collinearity rather than simply computation of a mean response on the items of a factor. However, since the reviewer considers this is a major compulsory revision, we have redone all analyses using mean response on the items for each factor (see statistical analysis and all tables).

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?).

Response: We agree with the reviewer and have redone the analysis using continuous score of the factors.

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.

Response: We have already reported the main effect model for the entire sample in the original version (see table 3). We have also tested the significance of the interaction effects of sex, age, education and family income by factors on PA and they are statistically significant. Therefore we reported the results of stratified analysis by sex, age, education and family income. We have now revised the text of the statistical analysis section to reflect this statistical significance of interaction terms (page 9-10).

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.

Response: There are two reasons that the analyses were not adjusted for family income: 1). Education and income are highly related; 2). There are 788 records with a missing value for family income variable. We have now stated this in the statistical analysis section (page 9).

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 …’

Response: We agree with the reviewer and have now corrected the sentence accordingly (page 3).

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.

Response: We agree with the reviewer that it is better to say “independent contributions” rather than “direct effect”. We have now made the change (page 5).

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.
Response: The scales were developed by the Canadian Fitness and Lifestyle Research Institute for the purpose of the national surveys of the Physical Activity Monitor. We have now added this statement in the method section (page 9).

4. Social environment variables: I can count 8 items. On page 8 the authors mention 10 items. I assume this is a typo.
Response: Yes, it was a typo and it should be 8 items. We have now corrected it (page 8).

5. Physical environment variables: Was a definition for neighborhood provided?
Response: The questionnaire asked questions about “the physical activity facilities and programs offered locally in your community” and there was no definition for community or neighbourhood regarding the size of the community or neighbourhood.

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.
Response: According to reviewer’s suggestion, policy variable has been deleted from the analyses.

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.
Response: The choice of an eigenvalue depends also on the scree plot and interpretability of identified patterns. In this case, if we chose an eigenvalue >1.0, there would be another two factors to be retained (at 1.1) and that would make it very difficult to interpret the patterns.

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.
Response: We have now reported all unadjusted ORs for all variables (see tables).
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.
Response: We have now added the descriptives for independent variables in table 1 and provided the median and IQR for PA level in the results section.

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.
Response: We agree with the reviewer that engagement in PA can affect a person's self-efficacy. We have now revised the paragraph accordingly (page 13).

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).
Response: We agree with the reviewer’s comment and have now revised the paragraph accordingly (page 15).

12. Discussion – Page 13: Mediating variable analyses can be also assessed without using structural equation modeling.
Response: Yes, the mediating effect can be assessed by structural equation modeling and other methods, but this paper mainly examines independent effects. We have now revised the sentence accordingly.

Quality of written English: Needs some language corrections before being published
Response: The manuscript has now been edited and some language corrections have now been done.