Author’s response to reviews

Title: Patterns and socio-demographic correlates of domain-specific physical activities and their associations with adiposity in Chinese men and women: a cross-sectional study of 0.5 million adults

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Author’s response to reviews: see over
Response Letter

We have now revised the manuscript in the light of helpful comments from the editor and two reviewers. Our responses to the specific comments raised are provided below, along with the line numbers of the related text in the revised manuscript. With exception of a few purely background descriptive comments, the verbatim comments from the reviewers are also shown below.

Reviewer 1:

MAJOR COMPULSORY REVISIONS:

1. In view of the main characteristic of cross-sectional studies, I consider that the major limitation of this work is the absence of discussion about the potential of the sample. Good inferences from cross-sectional studies are based on representative samples, and this work leaves many questions about to whom the findings apply. I suggest that the authors discuss this limitation with greater emphasis.

RESPONSE: We agree that when a cross-sectional study is conducted specifically to provide estimate on the prevalence of a particular risk exposure or disease for a given population, the study population needs to be representative otherwise the estimates of prevalence won’t be generalizable. Our China Kadoorie Biobank (CKB) study was set up as a prospective cohort study rather than a one-off cross-sectional survey [1] and the current paper is not intended to provide estimates on prevalence of physical inactivity, obesity, or any other particular factor in China or any particular province of China. Instead, it is merely to describe the patterns and correlates of physical activity and the associations of physical activity from different domains with adiposity. As such, the representativeness of the study population is not necessarily required [2,3]. The associations observed can still be generalized to population at large and may also inform future prospective analyses on associations of physical activities with disease outcomes. To address the concern raised, we have now added the following text in the discussion “Although our study population is not intended to be representative of China as a whole or of any particular province[1], the observed associations, e.g. household activity in relation to adiposity, may still be generalizable to other settings or populations at large [2,3]. The true association might even be stronger than that observed in this study since the 10 study areas probably do not include China’s full regional extremes.” (page 13 line 3-7).
2. This work leaves a big question: in what way large-scale studies may generate new hypotheses? In the same sense as these findings can be expanded to the population plan, as well as raise new strategies for government action in promoting physical activity in different areas of China. I suggest that the authors pay attention to these issues.

**RESPONSE:** The most important reason for conducting large-scale Epidemiological studies is that only large-scale studies can reliably detect moderate but important, public health-relevant associations. The current paper aims to assess the cross-sectional associations of different domain specific physical activities with adiposity and several other factors. Although the study findings cannot be used to establish any causality, they will certainly be helpful to generate new hypotheses and to plan future prospective analyses on effects of physical activities on a range of major health outcomes such as ischemic heart disease, stroke, diabetes and certain cancers. Findings from these prospective studies will further inform the development of public health policies on physical activity. The following sentence has now been added at the end of manuscript to reflect this: *Findings from prospective studies will further inform the development of public health policies on physical activity in China and other low- and middle-income countries where chronic disease burden is increasing.* (page 15 line 17-19)

3. The TITLE is too long. The authors should make it more objective, such as: “Patterns and socio-demographic correlates of physical activity and their associations with adiposity in Chinese adults: a large-scale study”. Also, throughout the text, I suggest that the authors do not use the term “0.5 million adults” preferring the term "large-scale study", since their inferences are based on a sample of 466,605 adults.

**RESPONSE:** The title has now been revised to “Patterns and socio-demographic correlates of domain-specific physical activities and their associations with adiposity in the China Kadoorie Biobank study”.

**MINOR ESSENTIAL REVISIONS**

1. I suggest that the authors decrease the text of the results section, it is very extensive, and makes many references to supplementary material. I think the authors should focus more on the findings meets the main research question.
RESPONSE: Given the fact that there are several socio-demographic factors, several adiposity indices, and several physical activity domains under investigation, the results section is indeed rather long. We have now managed to reduce the length of the results section from 1288 words to 1133 words (page 9 line 4 to page 12 line 12).

2. I suggest that the authors clarify some definitions, as “non-sedentary sports-related leisure time activities” (?), and make the text more objective (page 7/line 24-25).
RESPONSE: Done. The definition of household activity has now been revised to “activities spent on household chores” (page 7 line 24) and the definition of active-recreational activities to “activities spent on sports during leisure time, but not sedentary leisure-time activities (mainly TV viewing)” (page 7 line 25).

3. Needs some language corrections before being Published.
RESPONSE: Further proof-read of the manuscript has been done by a few native English speakers.

Reviewer 2:

1. The socio-economic determinants of sedentary leisure time should be investigated. Whether sedentary leisure-time associated with adiposity indices should also be tested. In other words, this parameter should have the same weight as the other domains of physical activity in all tables and figures as it may be equally as important.
RESPONSE: These have already been analysed and reported previously [3]. This reference has been provided in this manuscript. In summary, sedentary leisure time was shown to be positively associated with annual household income level and adiposity.

2. On top of education, income, urban/rural, working/retired, smoking may be a strong, sex-specific (especially in China) determinants of both body weight and physical activity. Have the authors considered smoking in their analyses?
RESPONSE: In our study population, levels of adiposity were indeed the lowest among current smokers and highest among ex-smokers. All our analyses have been adjusted for the potential confounding by smoking (see Statistical Analyses section and figure legends). Due to constraints of space, we were not able to describe the effects of smoking in this manuscript.
3. Figure 1 is not that interesting as there could be marked heterogeneity in the “nonworking” group (retirement, unemployability, disabilities, etc.)

**RESPONSE:** As suggested, Figure 1 and the related text have been removed from the manuscript.

4. The results section is too long. Also, there is a word or some words missing in the first sentence of the results.

**RESPONSE:** The results section has been shortened (see above) and the misplaced ‘and’ at the end of the first sentence has now been removed.

**References:**


