Reviewer's report

Title: Low cardiorespiratory fitness is associated with metabolic syndrome independent of physical activity in Hong Kong Chinese midlife women

Version: 2 Date: 6 April 2013

Reviewer: Patricia Hageman

Reviewer's report:

Overall Comments:
The authors have completed an extensive revision of their manuscript with major improvements noted in the following areas. The authors:

1) addressed the rationale for the original aim of the study in the introduction to inform the reader about the problem and prevalence of MS in Chinese, citing appropriate and relevant references;

2) did cite their own work regarding CRF for Chinese women as compared to ACSM standards, which strengthens the relevance and importance of their work; and

3) provided relevant clarity in the discussion section about the population to which this study’s results from the original purpose is being considered.

When assessing the work, the following points were considered.

1. Is the question posed by the authors well defined? The original aim and purpose was very well-defined in this revision. A new aim was added (cited on page 6, line 3-6) to “test whether the associations between PA/CRF and MS are different across BMI categories (on the basis of absence or presence of MS and BMI levels of less than 25 kg/m2 or at least 25 kg/m2). Regarding inclusion of the second aim and its associated analysis, new issues were identified – see details under Major Compulsory Revision item 3.

2. Are the discussion and conclusions well balanced and adequately supported by the data? The discussion and conclusions about the original aim were well written in the revision. There are concerns about the second aim and its analysis - see details under Major Compulsory Revision item 3.

3. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished? In this revision, the authors have included their recent prior publications. They responded in part to the reviewers, but not in the manuscript, their rationale for selecting the Baecke TI (total index) versus the WTI (Weighted Total Index) physical activity measures, which is of concern – see details under Major Compulsory Revision item 2. In the revision, the authors did make reference to their prior work on CRF in this population— it would be informative to have the authors’ view and rationale for breaking the CRF into tertiles and then using the cut-score for the lowest tertile as the cut-score, versus using the mean score from their prior research on women of the same age.
4. Do the title and abstract accurately convey what has been found? The authors revised their title to “Low cardiorespiratory fitness is associated with metabolic syndrome independent of physical activity in Hong Kong Chinese midlife women” from their former title “Metabolic syndrome in mid-life women is associated with low physical activity and low cardiorespiratory fitness.” This is reflective of and relevant to the original aim. The new title does not seem inclusive enough to reflect the original and new aim related to BMI. Regarding the abstract, the authors provided a useful background in the abstract relevant only to the original and not the new aim. The revised abstract does not include the specific purposes/aims of the study.

5. Is the writing acceptable? Overall the writing is markedly improved for clarity as related to aim 1 throughout the paper – minor corrections are needed as noted.

Major Compulsory Revisions

1) The authors provided a useful background in the abstract relevant to only the first and not second aim; however, the specific purposes/aims of the study were not included in the revised abstract.

2) Of original concern was that the authors had not clearly acknowledge their prior published works of high relevance to this paper. In the revised manuscript, the authors have included the references to their prior work. It is essential that the manuscript inform the reader about any discrepancies between prior work and current work. It is noted that there are conflicts between results of a prior publication that supported the validity of the Baecke Weighted Total Index but not the Total Index in Chinese women, yet the analysis in the current study used the TI.

The authors, in the reply to authors but not included in the manuscript, acknowledged that in prior work, the Baecke questionnaire, WTI was found to have a higher correlation with mean energy expenditure than total index – [their prior paper stated WTI was significantly correlated with the 3-day diary and the TI was not]. In the reply to reviewers, the authors stated their rationale for using TI (because it was normally distributed and that it had a higher correlation with MS than with WTI). Because physical activity is a key measure for the specific aims of the study, and to make this work appear scientifically sound, the manuscript should fully disclose the results of prior work and include the rationale for their decision to use TI versus WTI in the current study, so that the reader is fully informed to make his/her own assessment.

3) I respect my fellow reviewer’s comment, which was classified as a minor comment in that critique, that the work of Ortega et al, 2013, as related to “metabolically healthy but obese phenotype” is an intriguing concept. Ortega et al (2013) suggested that high fitness should be considered a characteristic of metabolically healthy but obese phenotype – a point that could be noted in the discussion section. [Ortega FB, Lee D, Katzmarzyk PT, et al. 2013. The intriguing metabolically health but obese phenotype: cardiovascular prognosis and role of fitness. Eur Heart J 34, 389-397.]
In response, the authors respectfully included a sub-analysis whereby women were categorized into 4 groups on the basis or absence of MS (excluding waist circumference) and BMI levels of ≥ 25 kg/m² or at least 25 kg/m² to define women as not-obese or obese. By including this new specific aim and analysis, new major issues are raised.

a. With the addition of this new aim, the abstract and introduction in the revised manuscript did not include sufficient background literature or rationale for adding the new aim.

b. The rationale for a cut-score of 25 kg/m² to define obesity is not provided. In reviewing the Ortega et al 2012 article, page 392, obesity was defined using the NIH-based cut score of ≥ 30 kg/m², using an extensive sample of 43,265 in order to capture a sufficient sample size of metabolically healthy but BMI-based obese and BMI-based based normal weight for analysis. They found relatively small effect differences between the extreme groups, leaving out the “middle” group of BMI as noted by von Haehling et al (2013). [von Haeling S, Hartmann O, Anker SD. 2013 Does obesity make it better or worse: insights into cardiovascular illnesses. Eur Heart J 34, 330-332.]. It would appear that the authors’ work differed from the Ortega et al article, in that they divided Chinese women in two groups, one group representing normal weight, and one group representing overweight and obese.

In further study of this area, von Haeling et al 2013, suggests that findings from studies of Caucasians “should not be extrapolated to other ethnic groups such as Asians from whom other cut-offs have been defined to describe obesity” citing The Cooperative Meta-analysis Group of the Working Group on Obesity in China who suggests defining overweight as BMI ≥ 24 to 27.9 kg/m² and obesity as BMI ≥ 28 kg/m², another.[Zhou BF. 2002. Cooperative Meta-Analysis Group of the Working Group on Obesity in China. Predictive values of body mass index and waist circumference for risk factors of certain related disease in Chinese adults – study on optimal cut-off points of body mass index and waist circumference in Chinese adults. Biome Environ Sci 15, 83-96]

c. The discussion section is limited related to the new specific aim and its results. The authors note their small sample size as a potential reason for not finding differences between groups in CRF, in contrast with two previous studies they cited in the discussion section, (Ortega et al and Messer et al 2010 – whose sample was BMI of 34.2 +/- 2.7), which used different methods that this submitted work. The reader would benefit from knowing what the mean scores of BMI were for those Chinese women classified into the four categories for their analysis.

4) What is the true prevalence of MS in your women? On page 2, line 12 – it is 21.7%, on page 13 line 4 it is 21.7% and on page 18, line 21 it is 27.1%.

Minor Essential revisions –

1) Abbreviations MS-O-, MS-O+, MS+O-, MS+O+ are not universally recognized – and in addition, would you consider operational definition of obese is “overweight and obese”?
2) P values generally extend to 2 past the decimal unless the P is less than .001 or .0001 (page 13)
3) page 13, line 3, insert “The” in front of “majority”
4) page 14, line 15 “CRR” should read “CRF”
5) page 15, line 17 “cautions” should read “caution”
6) page 16, line 3, “against “ should read “against”
7) page 16, lines 20 and 21 should insert “a” after “had” and “risks” should be “risk” and delete “having”
8) page 17 “contribute” should read “contributes”
9) Page 17 line 9, “need” should be “needs”
10) page 18, line 5, “mean” should be replaced by a more appropriate word such as “method” and “to” should be replaced with “for”
11) page 19, line 13, “represents” should read “represent”
12) Tables 4 and 5 and Figure 1 should denote that the definition of MS excludes waist circumference

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Needs some language corrections before being published

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare I have no competing interests.