Reviewer’s report

Title: Metabolic syndrome in mid-life women is associated with low physical activity and low cardiorespiratory fitness

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Reviewer: Virginia Aparicio

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Comments to the Author

The authors of this manuscript aimed to investigate the relative and combined associations of physical activity and cardiorespiratory fitness with the prevalence of metabolic syndrome in midlife women.

This is clearly a study, which in terms of the experimental design is well conceived and controlled. The study results are not surprising or particularly innovative, but bring something more to the understanding of the relationship CRF-PA-MS in a specific population with different lifestyle and nutritional habits and a lower prevalence of MS than others countries.

As the authors address, a limitation of the study is the fact that Physical Activity was assessed by means of questionnaires and not with more objective techniques like accelerometers (e.g. Actigraph system). Moreover, the final sample size is relatively small to reach strong conclusions.

There are some major concerns regarding the manuscript in its current form which I feel need addressing.

Major Points:

1. The prevalence of MS is extremely lower when compare to other samples from other countries (this aspect should be addressed by the authors). This condition make also that the comparisons between the re-categorized groups by MS precise a Effect Size estimation (i.e. Cohen’s d [95% exact confidence interval]).
2. The sample is relatively small, especially in the regression (please explain the lack of subjects because they do not fill the supposed total sample size of 158 women). In this case, please address it as limitation of the study as well as the total simple size.

Minor points:

1. I think the authors abuse of abbreviations (e.g. FBG, SBP, DBP)
2. Please, define Waist circumference (WC) in the method section
3. Why did the authors decide to use the Baecke questionnaire instead of the IPAQ?
4. Did the authors analyze the Normality of the data? Why did the authors employ T-student test to compare women with MS vs. healthy women? Why the authors did not adjust the model for age, and therefore performed an ANCOVA to contrast the main groups?

5. Could you include the P derived of the regression analysis in the result sections?

6. I miss a reference to the new concept of “metabolically healthy but obese phenotype”, especially when talking about cardiorespiratory fitness (Ortega et al., 2012). To further analyze and categorize by this aspect could enrich the manuscript and the discussion section.


7. I did not consider measuring clinical data on cardiovascular risk factors as a strength of this study due to the fact that MS can just be defined through these measurements.

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

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests