Reviewer's report

Title: Physical activity, cardiorespiratory- and muscular fitness among pregnant women from urban Ethiopia

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Reviewer: Frida Renström

Reviewer's report:

Discretionary Revisions;
1. Is the measure used to infer cardiorespiratory fitness informative and reliable enough to be given such emphasis in the title?

2. As described in ref 2, are big seasonal changes in physical activity (PA) to be expected in this population?

3. Discussion, Physical activity: the observed 30% lower AEE observed in this population compared to others is supported by the ‘fact that energy intake in these pregnant Ethiopian women was also found to be low (data not shown)’. I think the paper would greatly benefit from combining these two sets of data, preferably being able to investigate their respective associations with birth outcomes if available. This would give a more comprehensive picture of the determinants of low birth weight in this particular population of women, if that is one of the end aims (see also major compulsory revisions, point 1).

4. Is this technically to be considered a high altitude population (>1500m above sea level)? If so, does this have any implications for the current work?

5. Results, second para, last sentence: a large part of the women appear to be underweight, which add an interesting dimension when assessing the relationship between physical activity during pregnancy and metabolic health in mother and offspring. One could, as touched upon in the discussion, hypothesize that when in a negative energy balance, it might be more beneficial to reduce energy expenditure. The characteristics of the population under study again suggest that a combined analysis investigating the associations between physical activity, energy intake and maternal/offspring health would be most interesting and informative from a public health point of view in this population.

Minor Essential Revisions;

6. The study is made with women from urban Ethiopia, is it likely that another result with regard to level of physically demanding activities during pregnancy would be obtained in rural areas, i.e. how generalizable are the study findings to women in general in Ethiopia?

7. Materials and method, Study area and population, row 7: Would avoid referring to the Actiheart output as ‘TEE measurement’, the Actiheart measures
heart-rate and physical activity from which activity energy expenditure is calculated/estimated.

8. Materials and method, assessment of habitual physical activity and cardiorespiratory fitness, para 5: how would walk speed over ratio of HR adjust AEE for possible pregnancy related changes when no knowledge of pre-pregnancy status is known? Assume there is a great inter-individual variation in walk speed and HR in general among people, does the authors know how this estimate associates with energy metabolism/AEE?

9. Materials and method, Anthropometry, body composition and muscular fitness: how much will handedness influence the measures of MUAC, AMA and TSF?

10. Might be appropriate to clarify that the energy cost associated with specific activities change continuously during pregnancy along with activity patterns (along with an increase in basal metabolic rate), influencing estimates and interpretation of AEE and PAL (PMID:17465854), particularly when comparing these estimates between women ranging between 7-40 weeks of gestation.

11. Discussion, physical activity, last paragraph + Discussion, cardiorespiratory fitness, first paragraph: In general pregnancy is associated with weight gain, partly due to water retention. One would assume this could potentially affect measures like MUAC? Or is this a population where weight gain during pregnancy is very modest and mostly confined to the growing fetus, or potentially even negative (considering that a large fraction of the women appear to be underweight/malnourished?).

**Major Compulsory Revisions;**

12. It is not entirely clear what the authors aim with the paper is. In the introduction the authors explain the potential impact of the generally considered high physical workload in pregnant women and birth weight and consequently offspring health in this population. Since no associations between physical activity and pregnancy outcomes are made, the aim appear to be to validated a method to assess physical activity behavior in this population to be used in future studies, but it does not appear to be a validation of the Actiheart, nor any subjective measures of physical activity behavior, rather being an attempt to objectively assess level of physical activity to investigate coherence with the general consideration of women in low-income countries having a high physical workload. But then the study is performed in an urban area with more than 50% of the women being housewives. The question that then arises is the proportion of women living in urban vs rural areas in Ethiopia, and how representative these findings are for Ethiopian women in general and in rural areas in specific (if the results in the end are to infer lifestyle guidelines during pregnancy in Ethiopia). I think the paper would greatly benefit from the authors being more focused with the aim of the paper. Although it relates to a very important topic, being studied in a population that due to its demographic and general characteristics might be most informative it is unclear what this particular paper will/want to contribute with to the field.
13. In line with the previous comment, the conclusion in the abstract is rather general ‘The level and intensity of physical activity among pregnant women from urban Ethiopia was low’, low compared to what? To other pregnant populations? To the general level and intensity of physical activity in urban Ethiopian women? What is the health impact of physical activity being ‘low’ in this population?

14. Table 4 and 5: I’m not sure I follow the results presented for the regression models on AEE. From the way the results are reported it appears that the effect estimates for the independent variables with regard to AEE have been back transformed before reported in the tables. Considering that log transformation will change the scale for the regression model, I don’t think this is an appropriate approach. I strongly suggest the authors report the log transformed beta coefficients. (To make the results more informative, the results could be illustrated with a specific example where the results could be back transformed.) If I interpret the output correctly this would correspond to a beta coefficient for Gestational age of -0.0044, from which it also becomes clear that gestational age is negatively associated with AEE? As the results are currently presented, there seem to be a positive association.

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