Reviewer's report

Title: The feasibility and effectiveness of high-intensity boxing training versus moderate-intensity brisk walking in obese adults: A pilot study

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Reviewer: Mathieu Gourlan

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This article compare the impact of a high intensity boxing training with a brisk walking program on obesity (e.g., body mass index), cardiovascular (e.g., VO2 max) and quality of life outcomes (e.g., vitality) among obese adults. I would like to highlight that this article is well written and clear. In addition, I found the author were (very) rigorous with regard to the protocol they have implemented. That said, I have some important concerns with the actual version of the manuscript. Those concerns are both related to the introduction (i.e., the concept of quality of life is not presented in this part of the manuscript), to the statistical analyses (i.e., there are no data available concerning the skewness and the kurtosis of the variables), and to the discussion (i.e., some paragraphs should be reworked to better highlight some interesting perspectives related to the study).

Major compulsory revisions

1. Introduction: The authors adequately present the pertinence of their work concerning the impact of high-intensity interval training on obesity and cardiovascular outcomes. However in the current version of the introduction, we do not understand very well why they have also explored the impact of their intervention on quality of life. For sure I am convinced that it was pertinent to test the impact of the intervention on such a variable. That said, I think they should insert a short paragraph notably recalling that obese population suffer from a low quality of life (e.g., Jia & Lubetkin, 2005) and highlighting (here more than in the discussion section) that some studies have reported the beneficial impact of high-intensity interval training on quality of life (i.e., Molmen-Hansen et al., 2012; Wisloff et al., 2007).

2. Statistical analyses: I believe the authors when they say they have inspected visually and (more importantly) statistically for the normality of the data. I nonetheless find it is a little bit surprising to have a normal distribution on 12 individuals (!). Just like some previous research (e.g., Ng, Ntoumanis, Thøgersen-Ntoumani, Stott, & Hindle, 2013), I think the authors should insert the data concerning the skewness and the kurtosis of the different variables in the complementary files of their manuscript.

3. Discussion (First paragraph): I think the authors make an error here to repeat (to not say to “copy/past”) what they have already presented before in their manuscript. A reader of BMC Sport Science and Rehabilitation is legitimately in right to read more interesting conclusions and perspectives in this paragraph. For
example, the authors announce that “recruitment was slower than expected”, but what do they concretely propose or recommend to improve recruitment if they want to implement a randomized controlled trial? To what extent such a result tend to confirm that boxing is perceived as a “risky” sport (Hermand, Mullet, & Rompteaux, 1999), and what advices should be given to other research teams or health practitioners that would be interested in recruiting obese patients to implement a boxing program? The authors could be inspired by the existing literature concerning the barriers and facilitators to recruitment in randomized controlled trials (e.g., Kaur, Smyth, & Williamson, 2012). Similarly, they report that two participants in the boxing group experienced some adverse events. According to them, to what extent such a result confirm (or infirm) the data available in the literature reporting that risk of injury is not necessary high among boxers (e.g., Loosemore, Knowles, & Whyte, 2007)? To what extent other research team or health practitioners should consider boxing as a “sure” activity? I think this is this kind of elements one can expect to read in the discussion section of such an interesting (and innovating) study (!).

4. Discussion (third paragraph): In this paragraph, the authors notably present in details the inconsistency in the literature concerning the impact of high intensity interval training on obesity outcomes. From this they (interestingly) present the result of a power analysis for future research to detect statistical significant on obesity variables. However, I think they miss an important perspective here, at least as important as to recruit 50 participants to detect an impact for their intervention. If it is true that inconsistency of high intensity interval training interventions is at least in part due to heterogeneity of those interventions in terms of duration of the intervals of high intensity training, or age or level of obesity of the population (as they announce in this paragraph), I think an important (and urgent) perspective is to implement research that explore the impact of the different duration of intervals of high intensity training (from 6 s to 4 min) in function of the age and/or level of obesity of individuals. I think this kind of perspective should more clearly appear in this paragraph.

5. Discussion (last paragraph): I think the authors are note enough audacious in this paragraph. In their study, they not only suggest that high intensity interval training may have an impact on quality of life (as I said in my first commentary I think this kind of information should be instead presented in the introduction section), but they suggest that high intensity interval training may have an higher impact on quality of life than moderate intensity exercise. If their result is confirmed, two kinds of perspectives appear appealing. First, it would be useful to explore the explicative mechanisms associated with the higher impact of high intensity interval training on quality of life. More precisely, it would be interesting to explore to what extent such a kind of exercise may have an higher impact on variables such as emotion, motivation, well-being or satisfaction with life (among others) in comparison to moderate intensity of exercise. Second, I think it would be also useful to explore to what extent both kind of training have a differential impact on outcomes that are associated with quality of life such as productivity at work or health expenditure (Cash et al., 2011; Dixon, 2010). I think this kind of perspectives could enhance the interest and the contribution of the paper to the literature.
Minor essential revisions

1. Statistical analysis: Reporting effect size (in the current study the Cohen’d) is very interesting. However, in the current form of the manuscript the reader “discover” in the results section that the authors report such an indicator. I think they should precise in the statistical analysis section that they have calculated the effect size related to the statistical test they have performed and recall the different criteria associated with the value of the d. This could be done with one (or two) sentence(s) like this: “Cohen’s d was calculated to explore the effectiveness of the intervention and provide data to inform sample size calculations in future studies. Cohen indicated criteria for evaluating d such that 0.20 is a “small effect”, 0.50 is a “medium effect”, and 0.80 is a “large effect” (Cohen, 1992).”

2. Outcomes (1st commentary): This comment deal with the sentence “Waist circumference, body mass and BMI was also reduced in the boxing group with small to medium effect (Cohen’s d = 0.23-0.48); however, these effects did not achieve statistical significance”. On what I understand the Cohen’s d announced in this sentence are not always the same than those presented in the Table 2 of the supplementary files. The Cohen’s d presented in the table 2 range from 0.29 to 0.48 but the authors announce in the section outcomes that the Cohen’s d range from 0.23 to 0.48. This is not a grave error but it is important to correct it.

3. Outcomes (2nd commentary): I think the authors should standardize the way they present the Cohen’s d and the p value in the outcomes section. For example they sometimes present the value of the cohen’d and the p value when the p is significant (e.g., “obesity outcomes” section) and sometimes they only present the Cohen’s d value and not the p value even if the p is significant (e.g., last sentence of the “HRQoL outcomes” section). Similarly, most of the times the authors present in bracket the value of the cohen’d and then the p value, but in the “sensitivity and post hoc analyses” paragraph they present the p value and then the cohen’s d.

4. Discussion: The authors tend to present a lot of results through the discussion. If it is natural to recall the most important results in the discussion I think some paragraphs here more look very much like an “outcomes” section, with some percentages, p values or Cohen’s d in every sentences, than a “true” discussion section where the authors situate their study among the existing literature and propose some perspectives for future works. The results presented in this section of the manuscript are not wrong, but most of them have to be placed in the “outcomes” section (as this is its function).

Discretionary revisions

1. Methods (participants paragraph): The authors announce “a desire to lose body fat” as an eligibility criterion. However, as the other eligibility criteria were measurable (e.g., BMI > 25 kg/m², waist circumference >94 cm for men), how did they measure the “desire” of participants to loose body fat? Did they use a questionnaire available in the literature? If yes what is the reference? Is it possible to present an item of this questionnaire in the manuscript? If they did not
actually measure “desire to loose body fat”, I would advise to authors to suppress this eligibility criterion as it tends to create confusion.

2. Clinical outcomes (Obesity outcomes paragraph): The authors present the brand name of most of the devices they use to measure obesity outcomes, except for the height and weight. Is it possible to precise the name of the band for the digital balance scale and the stadiometer?

References


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

Quality of written English: Acceptable

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

Declaration of competing interests:

I declare that I have no competing interests