

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Investigating Self-Perceived Health and Quality of Life; -a longitudinal prospective study among beginner recreational exercisers in a fitness club setting
AUTHORS	Heiestad, Hege; Gjestvang, Christina; Haakstad, Lene

VERSION 1 – REVIEW

REVIEWER	Xavier C. C. Fung Department of Rehabilitation Sciences, The Hong Kong Polytechnic University, Hong Kong
REVIEW RETURNED	10-Feb-2020

GENERAL COMMENTS	<p>General comment: The manuscript entitled “Self-Perceived Health and Quality of Life; - a one year follow-up study among new beginner exercisers in a fitness club setting” investigated the effects of fitness club attendance on self-perceived health and quality of life. The strength of the manuscript is the use of longitudinal design. However, more details should be provided in order to improve the clarity throughout the manuscript.</p> <p>Specific comments: INTRODUCTION: • In the first paragraph, “low SPH may be a valid and robust predictor of morbidity and mortality of various diseases (2-5)”. The authors may provide some example to highlight the importance of SPH. • In the second paragraph, it will be better to explain further, or provide definition and example. The authors may explain what is “exercise involvement”. What is the difference between active/inactive and involvement? How does the exercise involvement relate to your study? Is it about the usage of fitness club? If yes, please states it. In addition, any example to illustrate activity context would affect SPH? • The last paragraph, “This shows that maintaining regular exercise can be challenging even for motivated individuals. Thus, the primary aim of the present study was to investigate SPH and QoL at onset and after three, six and 12 months of fitness club membership.”, the linkage between these two sentences was poor. Please modify.</p> <p>MATERIALS AND METHODS • “Moreover, only physically inactive individuals (exercising <60 min/week at moderate or vigorous intensity or brisk walking <150 min/week, the last six months)” It would be better to mention any reference or standard to support your criteria of physically inactive. • I think there were typos, e.g. “35 points Likert-scale for QoL”, “QoL is measured on a 7-item scale”.</p>
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	<ul style="list-style-type: none"> • Can the authors provide the Cronbach's α of the scale from the current study? <p>RESULTS</p> <ul style="list-style-type: none"> • The authors used the term "frequent use". Please change it to "regular use" for consistency. <p>DISCUSSION</p> <ul style="list-style-type: none"> • In the first paragraph, "Main findings were an increase in mean scores for all five statements in QoL and an improvement in QoL sum score throughout the follow-up period." The authors should make it clear that only two items of QoL were significantly different. • In the third paragraph, the authors should discuss the non-significant results of QoL as well. <p>CONCLUSION</p> <ul style="list-style-type: none"> • Again, the authors should make it clear that only two items of QoL were significantly different.
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REVIEWER	António C. Rodrigues Sampaio Instituto Politécnico da Maia - IPMAIA, Portugal
REVIEW RETURNED	12-Feb-2020

GENERAL COMMENTS	<p>The manuscript entitled, "Self-Perceived Health and Quality of Life; - a one year follow-up study among new beginner exercisers in a fitness club setting", is about a current topic, given the exponential demand for fitness activities and the need to characterize the population that practice fitness activities. It is al very important to understand their motivations and their perceptions of health and Quality of Life, when exposed to fitness programs. Without a doubt, I believe this topics can be a valuable tool for all those involved in the Fitness industry. However, I have a few major and some minor points I feel that require further attention, before this paper could be considered for publication.</p> <p>General comments</p> <p>Firstly, I enjoyed reading the introduction, but I feel the structure could be improved to build a clearer and stronger rationale for your study. In the introduction, they could indicate which factors motivate the practice and which lead to abandonment, to justify the variables used and referred to in the methods (I have made some more specific suggestions below in relation to this).</p> <p>Secondly, my main concern with the manuscript is the QoL questionnaire, which should be better justified as to its importance and applicability. Also, there should be literature references concerning this questionnaire. Is it validated? Why was this questionnaire selected for this study?</p> <p>Third, the methods must be better clarified and supported in the bibliography (example from the 1st paragraph on page 7. There are some references in the discussion, but not in the methods.</p> <p>Fourth, the authors reported that they used other variables, and they should justify why these and not others. For example, why the use "cohabitation" or "children"?</p> <p>Lastly, why are the participants and outcomes mentioned in the</p>
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	<p>discussion section, and not in the the methods. Furthermore, the discussion is written in a rushed way and without sub-themes. The authors refer about drop-out, but was it the same for both sexes and at different ages? They included many variables and did not include these in the discussion. This needs to be improved to make the manuscript more robust.</p> <p>In addition to the generic points identified above, I provide several specific suggestions below.</p> <p>Specific comments</p> <ul style="list-style-type: none"> - p. 4, l. 28/38 – I suggest that the authors revise this sentence. Review English. - p. 6, l. 10 – withdraw the word “that”. <ul style="list-style-type: none"> l. 40 – I would suggest to better explain why the inclusion of these variables. - p. 7, l. 5 – Refer studies concerning the QoL questionnaire. - p. 9, l. 29/31 – Why were not all responses mandatory? <ul style="list-style-type: none"> l. 45/49 – Indicate studies about the impact of these variables. - p. 10, l. 19 – Refer to studies about the impact of these variables. <ul style="list-style-type: none"> - Table 1 – Why the results does not consider both sex? - Suggest dividing results by gender - Why did the authors include “Norwegian descent“ in table 1? - p. 13, Table 5 – Why the value of 80500\$ per year is considered high?
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

INTRODUCTION:

1. In the first paragraph, “low SPH may be a valid and robust predictor of morbidity and mortality of various diseases (2-5)”. The authors may provide some examples to highlight the importance of SPH.

Authors response:

We agree and to highlight the importance of SPH, we have somewhat rephrased the text and added some examples: “Up to date, several studies have shown that low SPH may be a valid and robust predictor of morbidity and mortality of various diseases, such as cancer, cardiovascular disease, stress, diabetes, and other chronic health condition. (2-5)” (page 4, paragraph 1).

2. In the second paragraph, it will be better to explain further or provide a definition and examples. The authors may explain what is “exercise involvement”. What is the difference between active/inactive and involvement? How does exercise involvement relate to your study? Is it about the usage of the fitness club? If yes, please states it. In addition, any example to illustrate activity context

would affect SPH?

Authors response:

We have tried to further explain exercise involvement, active/inactive, and activity contexts: "It is growing interest in the assessment of physical activity on modification of SPH, and studies have indicated a strong association between insufficient physical activity and lower SPH in adults, especially in older individuals (10,11). However, it is important to investigate SPH not only between individuals that are active or inactive according to current physical activity recommendations (12) but also if this differs between activity contexts (organized sports clubs, public spaces and fitness clubs) and exercise involvement (frequency, duration, intensity, and modes) (13)" (page 4, paragraph 2).

3. The last paragraph, "This shows that maintaining regular exercise can be challenging even for motivated individuals. Thus, the primary aim of the present study was to investigate SPH and QoL at onset and after three, six and 12 months of fitness club membership.", the linkage between these two sentences was poor. Please modify.

Authors response:

We thank you for the comment and have as suggested, restructured the two last paragraphs into one, as well as rephrased the main study aim: Those who join a fitness club may be initially motivated to exercise, still previous studies have shown a high dropout-rate after only three months (21,22). This shows that maintaining regular exercise can be challenging even for motivated individuals. Even though this arena has become a large and growing venue for activity, the scientific knowledge of those that choose to be a member is scant. Research has not yet investigated how fitness club membership relates to SPH and QoL-status among beginner recreational exercisers. Hence, the primary aim of the present study was to report longitudinal data of SPH and QoL in an age-diverse group of men and women across the first year of fitness club membership" (page 5, paragraph 2).

MATERIALS AND METHODS

4. "Moreover, only physically inactive individuals (exercising <60 min/week at a moderate or vigorous intensity or brisk walking <150 min/week, the last six months)" It would be better to mention any reference or standard to support your criteria of physically inactive.

Authors response:

We have now inserted two references to support our criteria of physically inactive: Garber, C.E., et al. (2011) and Hawley-Hague, H.H., M; Skelton, D.A; Todd, C (2016) (page 6, paragraph 3).

5. I think there were typos, e.g. "35 points Likert-scale for QoL", "QoL is measured on a 7-item scale".

Authors response:

Thank you for pointing this out. We have tried to clarify and have rephrased and added some text in the revised version: "...it may be that small changes occur in QoL, such as going from 23 (low QoL) to 26 (high QoL) on the total sum score for QoL (35 points)" (page 6, paragraph 4).

6. Can the authors provide the Cronbach's α of the scale from the current study?

Authors response:

As requested, we have conducted an analysis regarding Chronbach's α and have added the following text under the statistical section: "Chronbach's α for the SWLS was 0.87, 0.91, 0.90 and 0.91 at baseline, and after three, six and 12 months, respectively" (page 9, paragraph 2).

RESULTS

7. The authors used the term “frequent use”. Please change it to “regular use” for consistency.

Authors response:

The term “frequent use” is changed to “regular use” throughout the manuscript.

DISCUSSION

8. In the first paragraph, “Main findings were an increase in mean scores for all five statements in QoL and an improvement in QoL sum score throughout the follow-up period.” The authors should make it clear that only two items of QoL were significantly different.

Authors response:

We agree that this was not clear and have added to the manuscript: “The main findings were an increase in mean scores for all five statements in QoL, even if only two of the statements reached statistical significance. We also found an improvement in QoL sum score throughout the one-year follow-up period.” (page 16, paragraph 1).

9. In the third paragraph, the authors should discuss the non-significant results of QoL as well.

Authors response:

We are not sure that we understand what the reviewers` are asking regarding the third paragraph in the discussion. Firstly in this paragraph, we discuss the secondary aim with respect to regular use of the fitness club (defined as ≥ 2 sessions/week) and QoL. At the end of the same paragraph, we discuss our main aim concerning QoL throughout the first year of membership. However, we have tried to emphasize (with respect to the reviewers` comment) the non-significant results of QoL: “Throughout the follow-up period, there was an increase in two out of five statements of QoL and an improvement in total sum score. The three non-significant statements could be explained by a higher rating already at onset, and a possible ceiling effect, which we also discussed earlier regarding SPH. In addition, the SWLS focuses to a large extent on how the participants feel and think with respect to several important aspects from a life-long perspective (from birth and up to date). Hence, it can be difficult to rationalize our results and the influence of joining a fitness club, whatever the findings” (page 17, paragraph 1).

CONCLUSION

10. Again, the authors should make it clear that only two items of QoL were significantly different.

Authors response:

We have now rephrased the conclusion with respect to the reviewer`s comment: “We found an increase in all five statements in QoL, while only two of the items reached statistical significance. We also found an improvement in QoL sum score, whereas no changes were observed in SPH the first year of fitness club membership” (page 19, paragraph 3).

Reviewer: 2

11. Figure 1 in PDF format should be on a single page only.

Authors response:

Figure 1 is now in one single page.

12. Patient and Public Involvement: Authors must include a statement in the Methods section of the manuscript under the sub-heading 'Patient and Public Involvement'.

Authors response:

We have added the sub-heading “Patient and Public involvement” after the sub-heading “Study design and participants” in Materials and Methods, and also the following text: “Four volunteers completed a pilot test of the whole electronic questionnaire, which led to minor changes in wording and format. Otherwise, participants and public have not been involved in the development of research questions, study design or recruitment” (page 7, paragraph 2)

General comments

13. Firstly, I enjoyed reading the introduction, but I feel the structure could be improved to build a clearer and stronger rationale for your study. In the introduction, they could indicate which factors motivate the practice and which lead to abandonment, to justify the variables used and referred to in the methods (I have made some more specific suggestions below in relation to this).

Authors response:

We thank you for the comment regarding the structure and rationale for the present study. However, we are unsure what the reviewer is particularly asking for with respect to “...which factors motivate the practice and which lead to abandonment”. We have, however, in response to the first reviewer rephrased and added some text in the introduction such as: highlighted the importance of SPH, explained exercise involvement, active/inactive, and activity contexts, and restructured the two last paragraphs into one, as well as rephrased the main study aim. More details can be found below the 1st, 2nd and 3rd comments from reviewer 1.

14. Secondly, my main concern with the manuscript is the QoL questionnaire, which should be better justified as to its importance and applicability. Also, there should be literature references concerning this questionnaire. Is it validated? Why was this questionnaire selected for this study?

Authors response:

The QoL questionnaire was selected for the present study instead of other scales (e.g. the World Health Organization Quality of Life (WHOQoL) or Short Form Health Survey (SF-36)), because of multiple assessment-points and a comprehensive questionnaire, covering several possible factors influencing exercise adherence. After several discussions in the project group, we, therefore, decided to use SWLS, being shorter and including five statements only. Several studies have also supported the validity and reliability of the scale (33-36).

The text in the method section now reads: This is a secondary analysis of data collected as part of the research project “Fitness clubs - a venue for public health?” (22-25), a longitudinal prospective study, aiming to investigate which factors that influence exercise adherence in beginner recreational exercisers” (page 6, paragraph 1) and added: “Because of multiple assessment-points, and a comprehensive questionnaire covering several factors influencing exercise adherence (22-25), we decided after discussion in the project group to use SWLS. The SWLS is shorter and includes five statements only. Several studies have also supported the validity and reliability of the scale (33-36)” (page 8, paragraph 2).

We have also added two references to this paragraph; William Pavot et al. (1991) and Jovanović et al. (2020) (page 8, paragraph 2).

15. Third, the methods must be better clarified and supported in the bibliography (example from the 1st paragraph on page 7). There are some references in the discussion, but not in the methods.

Authors response:

We thank you for the comment and have corrected the 1st paragraph on page 7 concerning power calculations, where the numbers for the sample and the terms “SPH” and “QoL” were commingled. We also agree that some references in the discussion should also be quoted in the methods, hence, suggested references are added:

Gjestvang et al (2017) (page 6, paragraph 1), Gjestvang et al (2019) (page 6, paragraph 1), Garber (2011) (page 6, paragraph 3), Hawley (2016) (page 6, paragraph 3), Pavot, W., & Diener, E. (2009) (page 6, paragraph 4). Springer, Dordrecht et al. (2013) (page 6, paragraph 4), Pavot, W., Diener, E. D., Colvin, C. R., & Sandvik, E. (1991) (page 8, paragraph 2) and Jovanović, V., Lazić, M., & Gavrilov-Jerković, V. (2020) (page 8, paragraph 2).

16. Fourth, the authors reported that they used other variables, and they should justify why these and not others. For example, why the use “cohabitation” or “children”?

Authors response:

Based on crude analysis comparing demographic and health factors between high and low SPH, and high and low QoL, all seven variables with p-values ≤ 0.05 (Table 1) were entered into the binary logistics regression analysis (Table 5). This is now somewhat rewritten under statistics: “Based on crude analysis comparing demographic and health factors between high and low SPH, and high and low QoL, all seven variables (exercise, age, cohabitation, total household income, Body Mass Index (BMI), children and gender) with p-values ≤ 0.05 (Table 1), were all entered in the above order in the adjusted model (Table 5) (40-42)” (page 10, paragraph 1).

17. Lastly, why are the participants and outcomes mentioned in the discussion section, and not in the methods? Furthermore, the discussion is written in a rushed way and without sub-themes. The authors refer to drop-out, but was it the same for both sexes and at different ages? They included many variables and did not include these in the discussion. This needs to be improved to make the manuscript more robust.

Authors response:

We thank you for pointing this out and believe that the reviewer is asking about participants' outcomes (attendance and dropout) presented in the discussion and not under results. This was an error in the former manuscript and the result text now reads: “There was a large drop in participants visiting the fitness club twice weekly or more from baseline to three (54%), six (67%) and 12 (72%) months. More details of exercise behavior at the gym are previously reported (22-25)” (page 13, paragraph 1). The discussion is also to some extent rewritten: “More than half of the participants did not manage to visit the fitness club on a regular basis throughout the initial year of membership” (page 17, paragraph 2).

Specific comments

18. - p. 4, l. 28/38 – I suggest that the authors revise this sentence. Review English.

Authors response:

We agree and have already responded and revised this paragraph based on the first reviewer's comments (please see point 2).

19. - p. 6, l. 10 – withdraw the word “that”.

Authors response:

As suggested, we have withdrawn the word “that” from page 6, line 8.

20. l. 40 – I would suggest to better explain why the inclusion of these variables.

Authors response:

We agree that this could be written clearer and have now deleted and restructured this paragraph. The text now reads: “Enrollment was limited to adults (≥ 18 years),

21. - p. 7, l. 5 – Refer to studies concerning the QoL questionnaire.

Authors response:

As suggested, concerning sample size considerations and the QoL questionnaire, we have now added two references: Pavot, W., & Diener, E. (2009) and Springer et al (2013) (page 6, paragraph 4).

22. - p. 9, l. 29/31 – Why were not all responses mandatory?

Authors response:

The ethics of mandatory questionnaire responses were discussed in the project group, and after a thorough debate, we decided that the participants should not commit to an actual answer (for example “yes” or “no”), and response options such as “I do not want to answer” and “Not relevant” were therefore included in the questionnaire.

Hence, we have added some text under the statistics: “Also, due to ethics of mandatory questionnaire responses, we included “I do not want to answer” or “Not relevant” as response options, which in the SPSS data set were treated as missing values (page 9, paragraph 2).

23. -l. 45/49 – Indicate studies about the impact of these variables.

Authors response:

We believe that we have answered this question in point 16, but have also in response to the current comment, added three references supporting the impact of these variables on SPH and QoL:

Ogbeide, S. A., Neumann, C. A., Sandoval, B. E., & Rudebock, C. D. (2010), Pino-Domínguez, et al, (2016) and https://ec.europa.eu/eurostat/statistics-explained/index.php/Self-perceived_health_statistics (page 10, paragraph 1).

24. - p. 10, l. 19 – Refer to studies about the impact of these variables.

Authors response:

To the best of our knowledge, the object of a result section is to accurately present key results without interpretation or using references. Hence, considering the line you are referring to is presented in the result section, we hope that it is ok by the reviewer to not include studies about the impact of these variables. However, we have added several references in the method section (please also see point 23 above).

25. - Table 1 – Why the results do not consider both sexes?

Authors response:

We are uncertain if the comment is related to gender differences regarding general characteristics or gender differences concerning SPH or QoL. In the results, we have however in the revised version added the following text and hope this is satisfactory: “At onset, more men than women had a household income \geq 87 500 US dollar (52.0% versus 39.2%), worked outside the home (86.4% versus 61.6%), were overweight or obese (BMI \geq 25 kg/m², 58.4% versus 38.7%) and older (38.5 years versus 34.3 years)” (page 12, paragraph 1).

26. - Suggest dividing results by gender

Authors response:

Please see point 25.

27. - Why did the authors include “Norwegian descent” in table 1?

Authors response:

We included “Norwegian descent” to show the readers the proportion of participants coming from our cultural background and origin. We will, of course, change this to “Caucasian” or “Predominantly white” if the reviewer prefers one of these terms instead.

28. - p. 13, Table 5 – Why the value of 80500\$ per year is considered high?

Authors response:

Thank you for this question. The cut off for high household income was based on another large Norwegian intervention study (Haakstad et al, 2018), plus several publications from the present research project (ref nr 22-25). However, 80 500\$ is incorrect and should have been 87 500\$. We are sorry about this error and have changed from 80 500\$ to 87 500\$ throughout the manuscript and Tables 1 and 5.

References

Below is a reference used in the response letter only:

Haakstad, L. A., Vistad, I., Sagedal, L. R., Lohne-Seiler, H., & Torstveit, M. K. (2018). How does a lifestyle intervention during pregnancy influence perceived barriers to leisure-time physical activity? The Norwegian fit for delivery study, a randomized controlled trial. BMC pregnancy and childbirth, 18(1), 127.

VERSION 2 – REVIEW

REVIEWER	Xavier C. C. Fung Department of Rehabilitation Sciences, Faculty of Health and Social Sciences, The Hong Kong Polytechnic University, Hong Kong
REVIEW RETURNED	02-Apr-2020

GENERAL COMMENTS	Thank you for the opportunity to review a revised version of this manuscript. The authors responded to all my concerns, thus the paper can be accepted for publication.
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REVIEWER	António C. Rodrigues Sampaio Instituto Politécnico da Maia - IPMAIA, Portugal
REVIEW RETURNED	06-Apr-2020

GENERAL COMMENTS	The manuscript entitled, “Investigating Self-Perceived Health and Quality of Life; -a longitudinal prospective study among new beginner exercisers in a fitness club setting”, is about a current topic, given the exponential demand for fitness activities and the need to characterize the population that practice fitness activities. It is all very important to understand their motivations and their perceptions of health and Quality of Life, when exposed to fitness programs. Without a doubt, I believe this topic can be a valuable tool for all those involved in the Fitness industry. I am grateful for the fact that the authors have gladly accepted the suggestions sent previously, contributing to improve the submitted manuscript. However, I have a few points I feel that require further attention, before this paper could be considered for publication. I will answer the authors' responses to the suggestions made previously about the manuscript entitled - Self-Perceived Health and Quality of Life; - a one year follow-up study among new beginner exercisers in a fitness club setting. These suggestions should be
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VERSION 2 – AUTHOR RESPONSE

Reviewer 1

We are pleased that you are happy with our responses regarding the first revision and that you find our manuscript acceptable for publication in BMJ Open.

Reviewer 2

1. 13. Firstly, I enjoyed reading the introduction, but I feel the structure could be improved to build a clearer and stronger rationale for your study. In the introduction, they could indicate which factors motivate the practice and which lead to abandonment, to justify the variables used and referred to in the methods (I have made some more specific suggestions below in relation to this). It was my intention to ask what are the main factors that lead to physical exercise and the main factors for drop-out. The presentation of these factors would lead readers to understand the importance of the study in question and lead them to understand the loss of participants in the sample. So, I suggest that you mention these factors.

Authors response:

We thank you for the comment and have added some text regarding the main factors that may lead to exercise or dropout. The text now reads: “Physical activity is a complex behavior influenced by several different determinants (1). Much research has focused on the main factors that may lead to regular exercise or dropout. There is consensus that enjoyment (intrinsic motives), social support, and access to exercise facilities (environmental factors) may positively influence exercise behavior. On the other side, lack of time and motivation (internal barriers) may inhibit exercise adherence (2)” (1. paragraph, page 4).

2. 14. Secondly, my main concern with the manuscript is the QoL questionnaire, which should be better justified as to its importance and applicability. Also, there should be literature references concerning this questionnaire. Is it validated? Why was this questionnaire selected for this study? On this question, I inform you that I was completely clarified with the answer.

Authors response:

We thank you for letting us know.

3. 15. Third, the methods must be better clarified and supported in the bibliography (example from the 1st paragraph on page 7). There are some references in the discussion, but not in the methods. I was completely clarified by the answer.

Authors response:

We thank you for letting us know.

4. 16. Fourth, the authors reported that they used other variables, and they should justify why these and not others. For example, why the use “cohabitation” or “children”? In answering this question, I verified that the justification for the use of these variables is based on the references, right?

Authors response:

We thank you for pointing this out and have tried to justify that the use of these variables (exercise, age, cohabitation, total household income, Body Mass Index (BMI), children and gender) are based on the references Ogbeide, S. A. et al (2010), Pino-Domínguez, L. et al (2016) and https://ec.europa.eu/eurostat/statistics-explained/index.php/Self-perceived_health_statistics, in

addition to univariate analysis. The text now reads: “Based on previous literature (42-44) and crude analysis comparing demographic and health factors between high and low SPH, and high and low QoL, seven variables (exercise, age, cohabitation, total household income, Body Mass Index (BMI), children and gender) with p-values ≤ 0.05 (Table 1), were all entered in the above order in the adjusted model (Table 5)” (1. paragraph, page 10).

5. 17. Lastly, why are the participants and outcomes mentioned in the discussion section, and not in the methods? Furthermore, the discussion is written in a rushed way and without sub-themes. The authors refer to drop-out, but was it the same for both sexes and at different ages? They included many variables and did not include these in the discussion. This needs to be improved to make the manuscript more robust. In the authors' response, one of the parties was answered, however, I inform you, that I have not yet been clarified as to the fact why do you include the participants and outcomes mentioned in the discussion section, and not in the methods?

Authors response:

Based on review, we have somewhat rephrased the results regarding exercise dropout-rates, as well as included data concerning gender and age: “There was a large drop in participants reporting regular use of the fitness club (≥ 2 times a week) from three (51.8%) to six (37.6%) and 12 (37.4%) months ($p = 0.003$), with no gender or age differences” (1. Paragraph, page 13). We are not sure that we understand what the reviewer is asking about but have highlighted and added a sentence in the method section concerning participants and dropouts used in the discussion. The text now reads: “In line with Garber et al. (14), non-regular use was defined as exercising one session/week, or no exercise the last month, whereas regular use of the fitness club was defined as exercising ≥ 2 times a week. Hence, membership dropouts were counted in the non-regular users of the gym” (1. paragraph, page 9).

6. 27. - Why did the authors include “Norwegian descent” in table 1?

I accept the authors' justification and agree that this will be the best term to be used.

Authors response:

We appreciate your comment and have no further input.

Please note that we have done some minor changes in the manuscript due to minor typos/errors. On page 9 and in respective tables, the response rate at three months is changed from 225 to 224. In Tables 1 and 2, we have changed the number of participants at onset from 249 to 250, and in Table 4, the number of regular gym users at six months are changed from 82 to 80 participants.

We have acknowledged the reviewers' comments and questions and adjusted the manuscript accordingly and hope that the new version of our manuscript is acceptable for publication in BMJ Open. We look forward to your response.