Reviewer's report

Title: Singing classes for chronic obstructive pulmonary disease: a randomized control trial

Version: 1 Date: 30 June 2012

Reviewer: Ellen Freiberger

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Major Compulsory Revisions:
1.) The authors do not report if patient are taking singing as an adjunct therapy or a stand alone as it is stated in the introduction part and of first paragraph with reference.
2.) In contrast to the other interventions (Reference Bonilha et al. 2009;) this study took less singing sessions (only 8 weeks), and no longitudinal data is presented.
3.) In addition the intervention is not clearly explained how often which aspects were trained (e.g. as in the Bonhila paper) and no information is available how often participants did practice “their home assignment in the booklet (first paragraph in the Intervention section). One of the major concerns regarding the intervention is the difference in class attendance between CG (once per week) and IG (twice per week) which could create a bias, and no usual care group is involved (even so in the previous work no difference were found).
4.) No reason is given for the differences in class session offered to the CG and the IG which could have lead to a bias in increasing the PA more by just coming to the group session in the IG.
5.) Objectives of the study are not clearly stated (see comment under questioned posed).

1. Is the question posed by the authors well defined?
The study objectives are not clearly stated but arguments for the modifications are stated. No clear objective is stated e.g. why physical activity was assessed, and the meaning of the last sentence of this paragraph is not fully clear to the reviewer: is no breathing control one of the investigated variables or self-reported (?) health status? Please clarify. Only later in the methods sections, the reader can see the investigated variables e.g. shuttle walk.

2. Are the methods appropriate and well described?
The authors do not report if patient are taking singing as an adjunct therapy or a stand alone as it is stated in the introduction part and of first paragraph with reference.
In contrast to the other interventions (Reference Bonilha et al. 2009;) this study took less singing sessions (only 8 weeks).
In addition the intervention is not clearly explained how often which aspects were trained (e.g. as in the Bonhila paper) and no information is available how often participants did practice “their home assignment in the booklet (first paragraph in the Intervention section). One of the major concerns regarding the intervention is the difference in class attendance between CG (once per week) and IG (twice per week) which could create a bias.

3. Are the data sound?
Although the reported data seems sound again –as in the first study by this week- no quantitative effects were found “There were no significant differences between groups in the response of measures of breathing control, functional exercise capacity or daily physical activity (Table 2)”.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
Yes to a certain amount. A table for the components of the IG sessions as well as titles of movies (given the remarks by the participants) would be good.

5. Are the discussion and conclusions well balanced and adequately supported by the data?
The third paragraph of significant findings section (page 16) seem to be too optimistic given that the significant effects were only on some parts of the SF 36 and no effects in the quantitative data. Therefore this part of the discussion does not seem appropriate, and well supported by the data. Instead the CG improved in the control of breathing but no information about amount and intensity of practicing either the booklet or the singing at home has been given in the result section.

The reviewer did not understand why the information about the structure for the singing group is not displayed earlier or in a table but stated in the discussion section [“It is also worth noting that over half the workshop-time was normally spent on physical warm-ups, breathing exercises and singing exercises; under half singing songs: this proportion is perhaps different to a ‘normal’ community singing group in its emphasis on technique over song]. Not discussed are the possible effects of the physical warm-ups which is a major concern given the literature of positive effects of Tai- Chi e.g.

6. Are limitations of the work clearly stated?
No limitations are stated or given for this study.

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?
Yes they do state former work.

8. Do the title and abstract accurately convey what has been found?
The title is misleading. It should state that the study investigates about qualitative and quantitative effects of singing on COPD.
In the Abstract the conclusion seem to be too optimistic given the results and should be rephrased. Otherwise the paper could lead to wrong conclusions.

9. Is the writing acceptable?
The writing is acceptable and the paper well written.

**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.