Author's response to reviews

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

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Author's response to reviews: see over
Singing classes for chronic obstructive pulmonary disease: a randomized control trial- response to reviewers

We thank the reviewers for their comments which we hope have been satisfactorily addressed in the revised manuscript and below.

Nick Hopkinson.

**Reviewer's report**

**Title:** Singing classes for chronic obstructive pulmonary disease: a randomized control trial  
**Version:** 1  
**Date:** 30 June 2012  
**Reviewer:** Ellen Freiberger

**Reviewer's report:**

**Major Compulsory Revisions:**

1.) The authors do not report if patients are taking singing as an adjunct therapy or a standalone as it is stated in the introduction part and of first paragraph with reference.

Adjunctive in this context means in addition to conventional pharmacological therapy. Patients continued on their usual medications throughout. This has been clarified in the text (intro and methods page 8)

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.

As described in the introduction, the rational for the study length was based on our previous work so the duration was increased from 6 to 8 weeks in the second study. A longer intervention might have had additional benefits but would have had resource implications and further studies may be warranted. We wished to study the effect of a reasonably short intervention that could be introduced into clinical practice.

Longitudinal data is provided as the response to treatment in the two study arms.

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).

The intervention is described in much more detail in our previous (open access) paper (in particular the online supplement to that) and this is now signposted more clearly in the methods. As described in the introduction, the rational for the study length was based on our previous work so the duration was increased from 6 to 8 weeks in the second study. A longer intervention might have had additional benefits but would have had resource implications and further studies may be warranted. We
wished to study the effect of a reasonably short intervention that could be introduced into clinical practice. Study duration is discussed in the penultimate paragraph.

Participants were asked to practice at home, ideally for 30 minutes each day, either in two 15-minute sessions, or one longer session. Data on compliance with this were not collected.

4.) No reason is given for the differences in class session offered to the CG and the IG which could have led to a bias in increasing the PA more by just coming to the group session in the IG.

Physical activity was measured after the final visit so would not have been influences by the number of attendances during the intervention period (page 9 para 3)

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.

Text now states “The primary hypothesis of the present study was therefore that singing lessons would lead to a greater improvement in health status than a film studies group. In addition to health status measures of exercise capacity, physical activity and breathing control were also measured.”

2. Are the methods appropriate and well described?
The authors do not report if patient are taking singing as a adjunct therapy or a stand alone as it is stated in the introduction part and of first paragraph with reference.

This has been clarified as above

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.

This has been addressed above – the detail is available in our previous publication.

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)”. 
The heath status measures are quantitative – qualitative in this context means interview based responses as opposed to validated health status measures.

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.

The list of films is now in the text: Hable Con Ella, Local Hero, Vicky Cristina Barcelona, Point Blank, Punch Drunk Love, The Fog of War, Fargo, The Truman Show, Sunset Boulevard, Goodbye Lenin, Pan's Labyrinth, Double Indemnity, The Fog of War, The Truman Show, Sunset Boulevard, Goodbye Lenin, Pan's Labyrinth, Double Indemnity, Shadow of A Doubt, The L Shaped Room, I Heart Huckabees, Little Miss Sunshine, The Fall, Vertigo

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 main finding of the study is that singing has effects distinct from those of a “non-physical” intervention. We don’t think that this paragraph is “optimistic” but rather that it places the findings in a possible mechanistic context so would prefer to leave it unchanged.

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.

We have drawn attention to the fact that this program might differ from a “normal” community program but feel that further discussion of this is speculative. A detailed description of the singing groups is contained in the previous publication. The home singing exercises were to be done daily – we did not systematically obtain group were asked to practice daily.

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.
If editorially a longer title is preferred we are happy to change it. “Qualitative” is one of the key words.

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.

We disagree – the abstract conclusion “The present data suggest that singing has specific effects of physical wellbeing and taken together with other small studies support the concept that participation in singing lessons may be a useful activity for patients with COPD” seems to us to be cautious and to match the data.

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’
**2 Reviewer’s report**

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

**Version:** 1  **Date:** 8 June 2012

**Reviewer:** Stephen M Clift

**Reviewer’s report:**

**Major compulsory revisions**

1. **Fuller justification of design – and in particular the decision not to undertake an ‘intention to treat analysis’**

   Data were only available for those patients who returned for the follow up assessments. The number of sessions attended for each group is now included in the results.

2. **Fuller justification of the suitability of the statistical approach adopted.**

   Covariate analysis taking account of baseline assessments should be considered. Comment required on the extent to which the data meet the assumptions for parametric analysis

   As suggested we have undertaken covariate analysis of response including baseline values. Methods and table 2. The statistical significance of the improvement in SF36-PCS has improved slightly from a p value of 0.03 to 0.02. Other values are essentially unchanged.

3. **Means, standard deviations and confidence intervals for measures at baseline and follow-up required**

   Table 1 has been expanded

4. **Ensure accuracy and consistency between description of measures in text and reports in Tables 1 and 2 (there are some inconsistencies and omissions)**

5. **Data on activity measurement needed at baseline and explanation and checking required of data on activity in Table 2 (the differences between the groups are puzzling and seem very wide)**

**Minor essential revisions**

6. **Ensure accuracy of results reported in Tables 1 and 2 and consistency in number of decimal places (1 or 2 in all cases)**

   This has been standardised

7. **Abstract highlights FEV1 – consider stating that groups did not differ on the primary outcome measure or on the range of baseline measures employed**

   Abstract states that the groups did not differ significantly

8. **Fuller description of the two conditions – what did the singing programme and the film programme consist of and was there an element of choice for participants?**
The conduct of the singing intervention is described in detail in the online repository in our previous paper.

*Did the film group last for two hours each week?*

The participants watched the film and then discussed for about an hour afterwards (now clarified in methods),

*Did all participants in the two conditions attend one group together during the study?*

The study used a rolling program.

*There appeared to be three singing group facilitators. How they share the work of facilitation?*

There was one facilitator per session. PC did all the facilitation in the previous study but left during the present study. Subsequently JS and MW led groups at Royal Brompton or Harefield Hospital sites.

*In the qualitative results section one of the participants in the singing group mentions an ‘open mic’ session in a pub. Was this suggested/supported by the facilitator?*

This is not documented

9. *Why did some participants drop out of the groups they had been assigned to? Was there are option for participants to continue as participants in the trial even though they did not wish to continue with the groups?*

Specific reasons were not documented but participants no longer wished to take part at all including the end assessments

10. *Did all participants attend all sessions?*

Median of 7 for film and 14.5 for singers – now included in results

11. *Clearer discussion of potential confounding factors between the two conditions – for example the singing group met twice a week over eight weeks, but the film group once a week. This must mean that the singing group was more active simply because they had twice as much travelling to do. Singing group also had more opportunity to socialise and get to know one another and would probably be standing some of the time during the activity whereas the film group would be sitting for most of the time.*

This is discussed in penultimate paragraph. Physical activity measured after the end of the intervention period so attendance at groups was not being measured.

12. *Inconsistency in references to heart rate vs. heart rate recovery in text and tables. Subjective recovery mentioned in Table 1 but not text. Subjective recovery is the time for breathlessness (Borg dyspnoea score) to recover.*
13. Stated in Discussion that singing group showed ‘worse performance’ on the control of breathing measures, but reference to Table 2 shows only a very slight reduction in breath hold time, and an improvement in singing breath counting.

The discussion states that this is non-significant and it is mentioned in the discussion to illustrate a point about the suitability of outcome measures rather than the effect of the singing so we would prefer to leave it as it stands.

Discretionary revisions

14. Consistency in reference to the singing intervention? Lessons, classes or workshops?

The word workshop has been replaced for the singing sessions – terms classes/lessons/sessions were used interchangeably so have been kept.

15. Increase of length of the singing intervention compared with previous study – did two further weeks make a difference in the opinion of the facilitators?

Results now state “Singing group facilitator commented that sixteen sessions seemed about the right duration but that mixed ability classes were challenging and that this could be more difficult with a rolling program where new people were joining the group.”

16. References to the issue of social isolation – sources? What about heightened risks of depression as a factor that the singing intervention could address?

Social isolation is referenced in last para page 5 ref 20 21


Level of interest: An article of importance in its field
Quality of written English: Acceptable
Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.
Declaration of competing interests:
I declare that I have no competing interests