Author's response to reviews

Title: OPTIMAL, an Occupational Therapy Led Self-management Support Programme for People with Multimorbidity in Primary Care: A Randomized Controlled Trial

Authors:

Jess Garvey (garveyjm@tcd.ie)
Deirdre Connolly (connoldm@tcd.ie)
Fiona Boland (fionaboland@rcsi.ie)
Susan M Smith (susansmith@rcsi.ie)

Version:5 Date:3 February 2015

Author's response to reviews: see over
Dear Dr Richards,

Thank you for re-considering our paper which has been substantially revised based on reviewer feedback. We have provided a point-by-point description of the revisions made below as well as uploading a track change version of the original document.

Yours sincerely,

Prof Susan M Smith
On behalf of co-authors

Author responses to peer review comments.
3 February 2015

Editor's comment:
Please review the enclosed reviewers comments and make amendments, as appropriate, to the manuscript. It is essential you provide additional methodological detail to satisfy CONSORT reporting of the trial, addressing any omissions noted by the reviewers. Two reviewers have asked you to critically reflect on whether this study might be best described as a pilot trial rather than an RCT. It is important that whatever decision you make describing the study design, that you justify this decision. One reviewer has argued that you are not evaluating a 'complex' intervention - please provide a clearer description of your intervention in the manuscript to assist the reader in appreciating the complexity of the intervention.

Author response
We have added the term feasibility to the trial description. We would argue that it is more than a pilot study in that we had a pre-determined sample size calculation and the study was undertaken as a randomised controlled trial and we had already undertaken a pilot study of the intervention (O’Toole et al, ref 11). One of the authors (S Smith) is the lead author on the Cochrane Review of Interventions for Multimorbidity and the current study would be eligible for inclusion in an update of this review based on study design criteria.

We have also addressed the reviewer comment about ‘complex interventions’ below and justified our use of this term and added additional detail on intervention elements. We have developed this intervention over a period of 4 years using the MRC Framework and have secured additional funding for a large scale definitive RCT based on the current description. The international grant reviewers did not address this as a concern so we would be reluctant to change our focus at this stage. We have added a comment to the discussion to address this. The same applies to our use of the term multimorbidity.
Reviewer 1

Minor Essential revisions
Figure 1 - needs to be amended to satisfy consort guidance. The figure should start with information on patients referred to the study, then deemed eligible (or otherwise), and include the % of patients offered study entry who eventually took part. Please check consort guidance on how to present this diagram and amend as appropriate.

Author response
There is no clear denominator for patients as recruitment was based across three community care areas. We have added the following explanation to the Methods: Setting section to clarify this issue and provided a new CONSORT flow sheet using the template from CONSORT:
“Given the pragmatic nature of this feasibility trial there was no clear denominator of all eligible patients. Patients were recruited across three community care areas in which participating occupational therapists were based and these areas cover a population of approximately 67,000 people. Clinicians (family practitioners or any other primary care clinicians in the areas) were emailed with information and study inclusion criteria and encouraged to refer any eligible patients over a three-month period (December 2012 to February 2013). This replicates how an intervention such as OPTIMAL would be offered and delivered in clinical practice but means it is not possible to calculate the numbers of potentially eligible patients in the population. However, given the prevalence of multimorbidity this would be far higher than the numbers that were needed for this feasibility trial.”

Abstract, Line 47. Please describe the number/timing of follow-up assessments and the time point at which the primary outcome measure was assessed,

Author response
We have added the following to the abstract:
“Outcomes were collected within two weeks of intervention completion.”

Abstract, Line 54-57. Would be helpful to present the effect size and 95% CI whenever you report a p-value in the results.

Author response
We have presented the adjusted mean difference and 95% CI for the primary outcome in the abstract and removed the p values for secondary outcomes as we do not report all secondary outcome data in the abstract.

Methods, line 103. Consort guidance for complex interventions - it would be helpful if the authors included a box summarising the 'active ingredients' of their intervention to assist the reader. At present there is not enough description of the intervention. Although line 103 has a cross-reference to a academic paper (reference 11) describing its development/content, there is simply not enough description here to assist the reader. I would put 'see box X' before reference 11 on line 103.

Author response
We have now provided more detail on the intervention development, underlying theory and intervention elements as follows in the background and have added a box summarizing intervention elements as suggested:

“The programme was based on the Stanford Chronic Disease Self-Management Programme (CDSMP) with the key adaptations being an occupational therapy focus, groups professionally led and a clear focus on multimorbidity. We adapted this programme based on the need to develop effective interventions for patients with multimorbidity as the CDSMP has modest effects, particularly when delivered in settings outside the USA. The intervention was also designed to be professionally led, with the aim of harnessing the effective elements of other successful professional-led interventions such as cardiac and pulmonary rehabilitation. The theoretical underpinning for the OPTIMAL Intervention is Bandura’s Theory of Self-Efficacy. Bandura defined self-efficacy as “…people’s judgment of their capacity to organise and execute courses of action required to attain designated types of performances” In the context of the OPTIMAL intervention, the proposed improvement in self-efficacy would be expected to enhance self-management and confidence which would in turn enable patients to manage their symptoms and have improved performance of daily activities and improved well-being. Patients with multimorbidity may particularly benefit from such programmes as they have been shown to have low levels of self-efficacy and poor quality of life, both of which worsens with increasing numbers of conditions. This likely relates to the increasing complexity of managing additional conditions and the increasing burden of symptoms and of treatment. (24) These considerations have driven intervention design and development.”

Methods, Line 112. Can you please put a study reference number against the Research Ethics Committee approval by the university? If someone wanted to review the approval they would need it.

Author response
The Faculty Ethics Committee do not provide a reference number, instead they use the date that ethical permission has been given as a method for tracing ethical permission. This date has been added.

Randomisation, Line 136. You report that randomisation was independent of the trial management team. It’s a bit picky, but the point is that randomisation is conducted by a statistician who is independent of the people recruiting into the trial. Suggest you put 'trial management team responsible for patient recruitment' in Methods, around line 138.

Author response
Added as suggested

You describe the intervention group reasonably well, but you don’t describe the control group. You need a sub-heading with 'control group' and some description on what happened to them.

Author response
We have added the following to the Methods section:

“Control
All 50 participants underwent assessment and baseline data collection prior to randomisation. Patients allocated to control were placed on a waiting list and were invited to attend an OPTIMAL course following trial completion in their local Occupational Therapy Department.”

Outcomes, line 166. FAI - you describe three subcategories of activities, each scoring 0-15, but you don't actually say what these categories are. Please describe here. They are first mentioned in the results section.
Author response
These have now been added to the text – (domestic chores, leisure/work and outdoor activities)

Secondary outcomes, lines 168-175. A little more information is required on how each of the measures are scored (e.g. the range of possible scores for the measure/any sub-scores, and how to interpret it). The results tables do not have a foot note explaining the scores - so without any extra information - it is difficult to know whether, for example, a mean HADs anxiety score of 9.77 in the intervention group is a good or bad thing.
Author response
The following has been added as footnotes to the Table as requested:
“FAI: range 0 – 45 (total), 0 – 15 (subscales). Higher scores = greater occupational performance
COPM: range 1 – 10. Higher scores = improved perceptions of and satisfaction with ability levels
NEADL: range 0 – 66 (total) 0 – 15/18 (subscales). Higher scores = higher levels of independence
HADS: range 0 – 21 (both scales). Higher scores = higher levels of anxiety/depression
SSE: range 0 – 10. Higher scores = higher levels of self-efficacy
EQ-VAS: range 0 – 100. Higher scores = higher levels of perceived QoL
HeiQ: 4 point likert scale. Higher scores = higher performance levels in each domain”

Methods, Line 210: you applied the bonferroni correction for multiple comparisons. Please spell out what level of the p-value was deemed significant after this correction was applied, and whether or not this level was applied to the primary analysis, or just the secondary analyses.
Author response
The Bonferroni correction was used to adjust for multiple comparisons for the secondary outcome measures. Including all secondary outcomes measures and subscales, 23 tests were run. The p-value was adjusted by dividing alpha by the number of tests being run, thus 0.05 was divided by 23 to give 0.00217.

Results, lines 214-221: Would appear that all the patients referred to the study were eligible, but not all were recruited. For completeness, you might say that all 63 patients referred to the study met eligibility criteria. It is quite unusual for all
patients who are referred to be eligible, so this might need careful description.

Author response
We have clarified this in relation to the comment above about the CONSORT Flow sheet and added additional information to the Methods: setting and patients

Results, lines 225-27. Please put denominators next to the proportions throughout the patient flow section (e.g. XX/X; 12%).

Author response
The text has been amended to include this information.

Figure 1 presents an overview of participant follow up during the trial. Of the fifty participants recruited into the study, 44/50 had complete baseline and follow-up data sets (88%). In total, 6/50 participants were lost to follow-up (12%); four from the intervention group and two from the control group. The majority of the intervention group (20/26: 69%) attended three or more of the six sessions but 6/26 (13%) never attended any session.

Primary outcome, line 235. You header describes your primary outcome as 'occupational participation' whereas the methods describes it as either the FAI or 'activities participation'. Think you need to be consistent - to me occupational participation means ability to take part in paid work (which is I suspect not what you mean). I would also provide the effect sizes and 95% CIs for the between group differences tested within the text and not just the table. This is your primary outcome, and it would help the reader to see the effects in the text. You probably don't have to do this for your secondary outcomes, mainly as there are a lot of them.

Author response
We have used the term ‘activity participation’ throughout as this most accurately reflects the Frenchay Activity Index

Results, line 256. You refer to a number of EQ-5D domains with a trend to improvement, and cross refer to a table. Might be helpful to put the domains in brackets to assist the reader).

Author response
We have added these to the EQ5D result section as follows:
“The results relating to health related QoL showed a trend towards improvement in a number of EQ-5D domains (mobility, usual activities, anxiety/depression) in the intervention group (see Table 4).”

Discussion, throughout. I would avoid the use of abbreviations for the names of outcome measures (e.g. HCU, HeiQ, QoL) in the discussion and write them in full to aid clarity.

Author response
This has been changed to avoid abbreviations throughout the discussion

Discussion, line 335. You state it is a pragmatic trial. I agree, but you should also use the word 'pragmatic' in your methods section when describing study design.
Author response
We have added the word pragmatic to the study design throughout.

Box 1, line 531. There is a typo, with 'Priamry'. Also lines 534-535. The sentence starting 'the model of primary care' does not make sense. Please re-write to improve the clarity, possibly splitting into two.
Author response
The typo has been corrected and the sentence clarified as follows:
“The model of primary care supported by government but not yet fully implemented. The proposed model emphasises a change from secondary care to more appropriate primary care services, to provide a single point of entry for all health and personal social services.”

Table 2. The text does not fit onto one page, with the p-value column not visible. Please review.
Author response
Apologies, we did not spot this formatting error – all tables now fit the window contents.

Table 3. The foot note ‘***’ is potentially misleading. When next to a p-value, most people would erroneously interpret this as highly statistically significant due to convention of using * =0.05. ** =0.01 etc. I am not sure on the journal guidance, but it might be better to select a different symbol to aid interpretation.
Author response
We have replaced this symbol for clarity as suggested with the symbol #

Reviewer 2
I have some comments that might improve the reporting and discussion of this trial:
1. It is a small trial that comes off the back of some pilot work and there is a view that now they want to move to a definitive trial so is this trial better described as a pilot or feasibility trial?
Author response
We have added the term feasibility to the study design throughout. However, we would argue that the current study is more than a pilot study in that we had a pre-determined sample size calculation and the study was undertaken as a randomised controlled trial and we had already undertaken a pilot study of the intervention (O'Toole et al, ref 11). We would be reluctant to describe it simply as a pilot or feasibility study alone.

2. Can the authors say something more about what they mean by activity engagement? In the abstract I wasn’t sure what frequency of activity engagement was and initially thought it was about healthcare activity as opposed to just a measure of patients’ engagement with domestic, non-domestic activity, ADLs etc.
Author response
Activity engagement refers to the range of daily activities, be they self-care,
domestic and leisure related activities that people carry out over the course of a day/week/month. The FAI measures how often people participate in these activities and was chosen as the primary outcome as previous research has indicated that those with multimorbidity engage less frequently in productive and leisure activities despite having the ability to do so (Lin Y.C, Huang L.H., Yeh M.C., and Tai J.J. (2010). Leisure-time physical activities for community older people with chronic diseases. *Journal of Clinical Nursing*, 20 (7-8), 940-949). One of the primary objectives of the OPTIMAL programme is to increase frequency of activity engagement.

3. The abstract also alludes to the idea that the intervention was 'manageable' but this is not discussed - this implies some level of acceptability to patients which is important given what we know about the need to balance interventions with the need to take into account patient's desire for minimally disruptive medicine (see for example http://www.bmj.com/content/339/bmj.b2803). Also, how was this measured?

*Author response*

We have removed the term manageable from the abstract as it does lead to confusion. We have further addressed this comment under the related comments on feasibility and group attendance.

4. The introduction and discussion might also benefit by contextualising their findings in relation to what we know about patient and professional attitudes to self-management. This trial shows that self-management can be achieved despite the back drop of data on the limited scope of GPs and nurses to activate patients to self-care and the idea that multimorbid patients' attitudes to self-care vary between those who see it as a priority and those who don't (e.g. http://www.ncbi.nlm.nih.gov/pubmed/25367263 and http://smo.sagepub.com/content/1/2050312113510001.abstract)

*Author response*

Thank you, we have reviewed these papers and considered them and other papers to add contextual information to the background and discussion around chronic disease self-management programmes.

5. Can they say a bit more about the rationale about the choice of FAI as the primary outcome as opposed to measuring physical activity per se?

*Author response*

The qualitative literature on patients with multimorbidity highlighted difficulties with daily functioning and the Cochrane review suggested that interventions addressing functional difficulty might be more effective across multiple conditions so this led to a focus on activity participation. We have addressed the rationale for choosing the FAI in our comment above.

6. There are lots of secondary outcomes which the trial would not have been powered to detect and what was the theory about including all these in a piece of pilot work?

*Author response*

All the outcomes were chosen based on previous pilot work and using the MRC
Framework to guide evaluation of the theoretical underpinning and potential mechanisms of action of intervention elements. We did not regard the current study as a pilot study but as a small feasibility or exploratory RCT. We regarded the secondary outcomes as important in relation to the theoretical underpinning and we also wanted to collect data on the feasibility of measures for use in the planned definitive RCT.

7. The analysis is described as ITT but they say they only used data from complete cases which sounds like a complete case analysis. Can the authors be clear if missing data were imputed.

Author response
The analysis was undertaken according to ITT principles. There are different approaches to this including complete case analysis, which is what we have presented. We did not impute missing values and this is now clearly stated in the Methods: analysis section. We did undertake a sensitivity analysis using one of the approaches to missing data – Last Observation Carried Forward but this made no difference to overall results.

8. How does the data on drop out and completion in this OT intervention compare with OT interventions in other populations?

Author response
The group participation rate is very similar to other group based interventions. This is addressed in the discussion and highlights the importance of being able to offer one-to-one type interventions for patients who do not want to attend groups. The following text was added to the discussion:

“As outlined in the results, low attendance was an issue and a number of participants missed sessions. In other self-management programme studies the number of participants per group ranged from 8 to 15. Initially the aim in this study was to recruit a minimum of 10 participants for each group, however due to lower than anticipated recruitment rates and low attendance ≤six participants consistently attended each of the three separate programmes. Of the 26 participants in the intervention group, 20 (77%) participants attended one session and 16 (62%) attended 3 or more sessions. A Cochrane review found similar results, finding that between 51% and 87% lay-led self-management programme participants attended at least half the self-management programme sessions, and between 8% and 29% never attended any sessions”.

9. Can the mean difference in outcomes with 95% CIs be presented so we can get a sense of the clinical as well as statistical relevance of the outcomes.

Author response
These have now been added to the text in the results section as well

10. What does the outcome mean for patients? The idea that they are more active, e.g. by doing housework, is a marker that they are more engaged in daily activities and not being sedentary etc. but does this suggest a shift in health behaviours? Are the outcome likely to be linked to health gains down stream?

Author response
We have added the following sentence to the discussion opening summary: “These improvements have the potential to improve outcomes for patients through more effective management of long term conditions.”

**Reviewer 3**

**Major compulsory revisions:**

There are a number of design issues that need to be addressed. The study is described as a randomised controlled trial using the UK MRC design for complex interventions. There is no evidence in the methodology or results and discussion that the trial does match these guidelines. There is for example no description of background theory which matches the intervention and which corresponds with outcome measures designed to measure the theoretical application of the intervention. The intervention could in fact be argued as not being complex. It was a standard 6 week group intervention conducted by occupational therapists. The time frame supports this as a unitary intervention because the measures were taken at baseline then immediately after the group program.

**Author response**

We have now added a detailed description of the intervention development and underlying theoretical framework. The MRC Framework states that complex interventions are ‘conventionally defined as interventions with several interacting components’ and we have highlighted the elements of OPTIMAL in a separate Box as suggested by Reviewer 1. We have spent four years developing this intervention using the MRC Framework and do not feel we can adjust our reporting of this process.

The second issue is related to the title and aim of the study. It is described as an efficacy study with a non specific ie convenience selection of patients, and randomisation which is briefly described. Whilst there are some elements of an efficacy study ie the selection of patients in a real world situation, the intervention is delivered by specially selected and trained occupational therapists identified for the study. Under the Complex intervention guidelines this study is more like a phase I pilot study and should be described like that, although given that it is unlikely to be a complex intervention it could simply be described as a pilot study which would inform the design of a larger RCT. The reference to being a complex intervention should be removed.

**Author response**

We explicitly stated that we were examining the effectiveness of OPTIMAL rather than the efficacy and we have outlined above why we regard this study as a randomised controlled trial but have added the term feasibility to acknowledge that it was a small scale RCT of short duration and that a definitive RCT is needed to test effectiveness in a wider setting and over longer periods of time. The aim has been amended as follows:

“We aimed to undertake a pragmatic feasibility trial to determine the effectiveness of OPTIMAL for increasing activity participation in individuals with multimorbidity”

**Background**
a key issue here relates to a lack of theory and reference to previous research. 1. Multi-morbidity: the authors claim this is a first study in multi-morbidity of a group self-management intervention. The problem with putting this study in this framework is that multi-morbidity is a vaguely defined concept. It is confused with co-morbidity and it is unclear whether it includes mental disorders, drug and alcohol as well as physical disorders or disabilities eg post stroke. Any definition which includes nearly 60% of the adult population is probably unhelpful scientifically as a definition and to inform the efficacy of interventions. The authors use the criteria of 2 or more chronic conditions (‘any two chronic conditions’ is probably incorrect) which does not fit with the general use of the term multi-morbidity to refer to those with 5 or more chronic diseases/conditions to imply a more complex severe group of people. I suggest removing reference to multi- multi-morbidity in the criteria although referring to it in the discussion might have relevance.

Author response
We are very aware of the extensive literature and discussion regarding the definition of multimorbidity, which is most commonly defined as being two or more chronic conditions. We would not agree that there is consensus that it refers to five or more conditions. Because of these differences in definition, which also relate to whether authors use restricted condition lists, there is general acceptance that authors need to very clearly define what they mean when they use the term multimorbidity. We have added further clarification to the section on patients and settings to reflect this.

We have also added comment in the discussion on how an intervention that specifically targets multimorbidity differs to previous CDSMPs that have generally targeted single conditions but have also reported results from sub-groups of patients with multimorbidity attending the same programmes.

2. Self-management; similar to multi-morbidity, self-management is referred to without a definition – there are several international definitions to refer to but again it is a broadly used term but there is a large body of literature which debates concepts like self-management, self-care and self-efficacy and similarly there is a body of literature discussing measurement of self-management based on definitions or where there are no definitions.

Author response
We have added the following definition to the introduction:
“Self-management, sometimes referred to as self-care has been defined as the actions taken by individuals to lead a healthy lifestyle, to meet their needs and to care for their long-term conditions to prevent further future illness.”

3. The intervention; this is a 6 week group intervention delivered by occupational therapists. In the background there is no reference to a large body of research from Kate Lorig and colleagues on the Stanford chronic disease self-management course which is a 6 week course which can be delivered by lay (peers) leaders or health professionals or both. In fact a glaring omission in this paper is the lack of reference to Lorig’s work with only on reference which is to the Stanford outcome measurement tools. The Stanford literature is referred to briefly in the discussion, but it appears that the content and skills being taught to the patients are very similar
to the Stanford course. There needs to be a table and description of the similarities and differences between the two programs where the intervention is being described. On the face of it the only significant difference between the two programs is that it is being delivered by occupational therapists. Both are generic not disease specific programs and the literature differentiating the disease specific and generic programs needs to be acknowledged.

Author response

Apologies that this was not clear but we had written a short introduction to keep within the word count. We have now specifically addressed in the Background and Discussion sections how OPTIMAL was developed and adapted from the CDSMP and how they differ.

The claim in the discussion that this is the first to use health professionals in such a program is incorrect – others have tested physiotherapists and other disciplines conducting these Stanford like programs. Indeed Lorig herself has tested a combined model of health professional and peer compared to peers alone. The findings can be summarised that when health professionals conduct these programs the participants know more, when peers conduct the program the participants do more.

Author response

We are not aware of any other version of the CDSMP that has been completely professionally led and evaluated using RCT methodology. We chose this approach in an effort to harness the success of Cardiac and Pulmonary Rehabilitation models as published in Cochrane Reviews of these interventions and we were influences by the paper by Griffiths et al on this topic. (ref: Griffiths C, Foster G, Ramsay J, Eldridge S, Taylor S. How effective are expert patient (lay led) education programmes for chronic disease? British Medical Journal. 2007;334(7606):1254-6). This detail and reference is now included in the Background section.

Methodology

There needs to be more detail on the randomisation process. Who conducted it, when and how independent was the person conveying the allocated group to the patient. What was the computer allocation method used? What was the procedure in how the patient allocated to the intervention group was then provided with the intervention. How long did it take to fill a group and commence it? Did this impact on the non attendance at the beginning?

Author response

We have added the following details on the randomization process to the methods section:

“Participants were randomised when baseline data collection was complete. Randomisation was carried out remotely by a statistician independent of the trial management team, using a computer generated sequence. The researcher informed participants of their group assignment a week prior to intervention commencement by telephone. It took between 4 to 8 weeks to fill a group. (Six participants never attended any session. The time between baseline data collection and group commencement may have been a contributing factor).”
Sample size – this is based on the FAI difference of 4 points using a 90% power and even including 20% drop out the total is 34 – presumably in each group – this is not clear in the text – and then ultimately each group had 26 and 24, so it is unclear whether this is higher than required or under the desired sample size. This will influence whether there could have been type 1 or 2 errors in the results. Analysis – my understanding is that intention to treat analysis includes all subjects with baseline data whether they have follow up data or not and whether they received the intervention or not.

Author response
Apologies, we have clarified this as follows:
“a total sample of 34 participants was required and we aimed to recruit 60 participants in total, 30 in each arm with three groups running for intervention group participants in the three primary care centres involved, and 10-12 patients per group as we anticipated some variation in group attendance over the programme anticipated.”

Results
Whilst 22 in each group were part of the pre and post analysis, the text refers to 23% not attending any session. This breakdown should be included in the consort flow diagram. Of those who did start, 69% attended three or more sessions – what is this n? it is easy to work out but should be shown.

Author response
This is now included in the new CONSORT Flow diagram

A key issue here is feasibility and this should be a focus of this pilot study. It is a major finding and highly consistent with other group self-management trials that few people attend for significant periods and they are mostly women. Again we do not know the breakdown of those who started and those who attended three or more sessions relating to gender. Whilst the overall outcomes are positive it is critical to know who attends or does not and preferably why. The inference is that other forms of self-management delivery are needed and they may have the advantage of being more accessible and cost effective eg individual one to one interventions or on-line interventions and those targeted specifically at men or disadvantaged groups who do not attend group programs in working hours.

Author response
We have added comment on attendance to the discussion section as outlined in our response above. We did not do a detailed analysis of outcomes based on attendance or other sub-groups due to the small numbers in the study. We have added an additional comment on this as follows:
“A larger study is needed to assess the effect of attendance on outcomes, and examine differences based on age, gender and whether participants are engaged in paid employment.”

The HEI-Q results are not discussed in the results section.

Author response
We have added the following to the results text for the HEI_Q scores:
“The positive and active engagement in life domain were significantly higher
for the intervention group than for the control group (p=0.04) in the HeiQ scores. No significant findings were identified for the seven other domains.”

Discussion
Much of the discussion is relevant but would need to be rewritten in the light of the above comments.

Author response
We have substantially re-written the discussion based on the helpful comments of all three reviewers and hope we have addressed the main issues and concerns