

## PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (<http://bmjopen.bmj.com/site/about/resources/checklist.pdf>) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	What interventions are used to improve exercise adherence in older people? And what behavioural techniques are they based on? A Systematic Review
<b>AUTHORS</b>	Room, Jonathan; Hannink, Erin; Dawes, Helen; Barker, Karen L.

### VERSION 1 – REVIEW

<b>REVIEWER</b>	Sarah Dean University of Exeter Medical School, UK
<b>REVIEW RETURNED</b>	01-Sep-2017

<b>GENERAL COMMENTS</b>	<p>Thank you for asking me to review this interesting and useful paper. It has been well written and presented and helps address a gap in the literature. The work is based on an important clinical uncertainty – how to improve exercise adherence and is targeted at a specific population – older people, for whom there is increasing need to find ways to help them manage their often multiple long term conditions. The authors have followed some excellent and robust methods, for example the use of the Cochrane risk of bias assessment criteria. I also welcomed the use of the Behaviour Change Taxonomy as a way of identifying intervention techniques and strategies which can then be tracked back to underlying behavioural theories, what a sensible idea and useful undertaking since seldom, in my experience, does one theory explain all exercise adherence behaviour. Furthermore I agree that a meta-analysis would be impossible and suggest more comment should be made about the problem of accurately measuring adherence in a consistent manner (see later for more on this).</p> <p>My more detailed comments are firstly some suggestions for refinements to your manuscript and secondly some minor corrections for improving clarity or consistency.</p> <p>1) Page 4, 4th bullet in list of criteria. Consider adding some of the other terms commonly used with ‘adherence’ e.g. compliance, concordance or engagement. I see you have some of these in your search example however I suggest putting some comment about the range of possible terms in your text. I suggest adding somewhere a sentence that explains what you mean by ‘exercise’. In the introduction you mention exercises prescribed by physiotherapists, so is this also your definition of exercise for the criteria? Your example search list also suggests you did not search for ‘physical activity’ interventions so I think it is worth explaining</p>
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somewhere (perhaps in the introduction?) what the difference is between prescription of physical activity versus the type of therapeutic exercise prescription that might be made by a physiotherapist. These last two points could be covered together. Clarify if your exclusion of pilot studies also included the exclusion of pilot RCTs.

2) Page 5 Risk of bias section / Figure 3 / Table 1. Please comment more about the risk of bias results. I am particularly interested in seeing rehabilitation studies being more careful about reporting what, if any, blinding has occurred for outcome assessment and data analysis as these become so much more important when other blinding is impossible (as for most exercise or rehabilitation studies) as they can go some way to mitigate the lack of blinding for performance bias. Thus I was not sure how you made judgements regarding outcome assessment blinding. For example adherence outcome was 'considered adherent if they had performed exercise 3 or more times a week' for the Yates et al 2005 study, but you do not say who made this assessment and whether they were by blinded assessors, but the study gets a positive score. Similarly who assessed adherence in the Schneider et al 2011 study, who measured the time spent exercising in the past month? Ditto for studies by Ridgel et al 2016 and Boshuizen et al 2005. Conversely why did Gardner et al 2011 not get a positive score? One solution would be to put more detail in the table regarding who performed the outcome assessment and whether they were blind. If the authors do not report this detail then you may wish to re-consider your rating. Thank you.

3) Please consider re-crafting the conclusion. I have four issues here. Firstly you make a very strong and general statement regarding there being a 'lack of theoretically derived approaches to exercise adherence'. This is not technically true as a stand-alone statement, I suggest adding 'in physiotherapy interventions for older people' or words to that effect. Or, secondly, re-consider the statement entirely as it is possible that it is a problem of poor reporting rather than an actual lack of theoretically derived approaches. This was the reason Michie and colleagues produced their taxonomy – to help people report clearly and consistently what behavioural theories (or techniques based on theory) they were using in interventions. What you have done is show how poor the reporting has been in your specific area and you could now call for a need for (a) better reporting as well as (b) better use or (c) better development of theory in this area. Thirdly, I suggest you re-visit the problem of measuring this type of exercise adherence, you have already noted this problem and I suggest research in this area is always going to be limited unless we address the lack of robust, validated measurement, so this could be mentioned in your limitations and or your conclusion (as a need for future research – as no point developing the new interventions if you cannot measure their effects!). Finally, pretty much all adherence research needs to consider the relationship between adherence and health outcome, in your study you have not mentioned this and whilst I do not think you need to add this in terms of what you are covering in your review, it is worth an acknowledgment - either in the limitations section and or your conclusion.

This is because there is no point in promoting adherence if there is no beneficial health outcome or if the outcome is actually detrimental to health (which can happen in exercise interventions, for example increased risk of falls).

	<p>4) Adjust Table 1 so that studies are listed alphabetically - this is what you have done for the Risk of Bias Figure and having both the same makes it easier to compare.</p> <p>5) I suggest writing in full all words abbreviated 'didn't', 'couldn't' etc throughout manuscript.</p> <p>6) Page 7, 3rd line: replace 'subjects' with 'participants'.</p> <p>7) Page 8, 1st line. Sentence ending: 'evidence from other populations' would benefit from a supporting reference.</p> <p>8) Page 9, 2nd line of conclusion. Delete 'with'?</p> <p>Finally, this is a really useful, focused piece of work and I wish you well with your future research endeavours.</p>
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<b>REVIEWER</b>	<p>Philippa Nicolson Centre for Health, Exercise and Sports Medicine University of Melbourne Australia</p>
<b>REVIEW RETURNED</b>	04-Sep-2017

<b>GENERAL COMMENTS</b>	<p><b>Title</b> The title is rather "clunky" in 3 parts, and does not accurately reflect the paper - the behaviour change techniques that interventions are based on is more of a focus than the theoretical background (as per abstract).</p> <p><b>Abstract</b> The objectives again mention the behavioural theories and not the behaviour change techniques, then the methods mention only the BCTs, and the results focuses on the BCTs - this is not clear. "utilized behavioural approaches" would be clearer to say "explicitly utilized behavioural theory"</p> <p><b>Introduction</b> The first paragraph jumps straight to physiotherapists prescribing exercise - what about other health professionals who prescribe exercise? This opening paragraph does not "grab" the reader. 2nd paragraph "it is known that exercise adherence can affect treatment outcomes" - would be good to elaborate on this - how? Paragraph about predictors of adherence is not directly relevant to this review. The work of Michie et al in developing the Behaviour change technique taxonomy is not introduced. The final paragraph mentions the Cochrane review of interventions to improve exercise adherence but does not include more recent published reviews that are specific to older populations, such as: Ezzat AM, MacPherson K, Leese J, Li LC. The Effects of Interventions to Increase Exercise Adherence in People with Arthritis: A Systematic Review. Musculoskeletal Care 2015: 1. Nicolson PJA, Bennell KL, Dobson FL, Van Ginckel A, Holden MA, Hinman RS. Interventions to increase adherence to therapeutic exercise in older adults with low back pain and/or hip/knee osteoarthritis: a systematic review and meta-analysis. Br J Sports</p>
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	<p>Med 2017; 51. Williamson W, Kluzek S, Roberts N, Richards J, Arden N, Leeson P, et al. Behavioural physical activity interventions in participants with lower-limb osteoarthritis: a systematic review with meta-analysis. BMJ Open 2015; 5: e007642.</p> <p><b>Methods</b> Eligibility criteria - was the comparator group required to also be completing exercise - this is not clear? Data synthesis - a little more detail about the behaviour change technique taxonomy / how interventions/intervention components were categorised is needed.</p> <p><b>Results</b> Risk of bias - assessment completed but not discussed - were there common areas of bias across the studies? Behavioural theories - were the interventions based on theory more effective? It would be good to include intervention content rather than just the authors names in relation to each study.</p> <p><b>Discussion</b> More recent relevant reviews not included as mentioned in introduction comment.</p> <p>When discussing the benefits of feedback and monitoring in context no link is made to published work identifying barriers and facilitators to exercise adherence - this would help to justify the suggestion that these interventions are worthy of further exploration.</p> <p>Issue of contextual equivalence would be good to raise and discuss - given that some included studies compared two groups completing totally different interventions.</p> <p>Strengths and limitations - measurement of adherence is first mentioned here - this is certainly an issue but should be raised earlier.</p> <p>Figure 2 - error in box "Records after removal duplicates removed = 47" should be "Duplicates removed = 47"</p>
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**Reviewer: 1**

Reviewer Name: Sarah Dean

Institution and Country: University of Exeter Medical School, UK

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

Comment: What interventions are used to improve exercise adherence in older people? And what behavioural theories are they based on? A Systematic Review.

BMJ-Open – 2017 – 019221

Response: Thank you for asking me to review this interesting and useful paper. It has been well written and presented and helps address a gap in the literature. The work is based on an important clinical uncertainty – how to improve exercise adherence and is targeted at a specific population – older people, for whom there is increasing need to find ways to help them manage their often multiple long term conditions. The authors have followed some excellent and robust methods, for example the use of the Cochrane risk of bias assessment criteria. I also welcomed the use of the Behaviour Change Taxonomy as a way of identifying intervention techniques and strategies which can then be tracked back to underlying behavioural theories, what a sensible idea and useful undertaking since seldom, in my experience, does one theory explain all exercise adherence behaviour. Furthermore I agree that a meta-analysis would be impossible and suggest more comment should be made about the problem of accurately measuring adherence in a consistent manner (see later for more on this).

My more detailed comments are firstly some suggestions for refinements to your manuscript and secondly some minor corrections for improving clarity or consistency.

1) Page 4, 4th bullet in list of criteria.

Consider adding some of the other terms commonly used with 'adherence' e.g. compliance, concordance or engagement. I see you have some of these in your search example however I suggest putting some comment about the range of possible terms in your text.

I suggest adding somewhere a sentence that explains what you mean by 'exercise'. In the introduction you mention exercises prescribed by physiotherapists, so is this also your definition of exercise for the criteria?

Your example search list also suggests you did not search for 'physical activity' interventions so I think it is worth explaining somewhere (perhaps in the introduction?) what the difference is between prescription of physical activity versus the type of therapeutic exercise prescription that might be made by a physiotherapist. These last two points could be covered together.

Clarify if your exclusion of pilot studies also included the exclusion of pilot RCTs.

Comment: We agree with the reviews comments here and have added the terms compliance, concordance and engagement to the 4th bullet in the criteria list on page 4.

Exercise has been defined and differentiated from physical activity in the introduction – see opening paragraph in the introduction.

We have clarified the exclusion criteria regarding exclusion of pilot RCTs – see eligibility criteria

2) Page 5 Risk of bias section / Figure 3 / Table 1. Please comment more about the risk of bias results. I am particularly interested in seeing rehabilitation studies being more careful about reporting what, if any, blinding has occurred for outcome assessment and data analysis as these become so much more important when other blinding is impossible (as for most exercise or rehabilitation studies)

as they can go some way to mitigate the lack of blinding for performance bias. Thus I was not sure how you made judgements regarding outcome assessment blinding. For example adherence outcome was 'considered adherent if they had performed exercise 3 or more times a week' for the Yates et al 2005 study, but you do not say who made this assessment and whether they were by blinded assessors, but the study gets a positive score. Similarly who assessed adherence in the Schneider et al 2011 study, who measured the time spent exercising in the past month? Ditto for studies by Ridgel et al 2016 and Boshuizen et al 2005. Conversely why did Gardner et al 2011 not get a positive score? One solution would be to put more detail in the table regarding who performed the outcome assessment and whether they were blind. If the authors do not report this detail then you may wish to re-consider your rating. Thank you.

Response: The section on quality assessment in the methods, has been expanded to include a description of how judgements were made for each of the domains in the risk of bias assessment.

Thank you for raising this important point around blinded outcome assessors/data analysis. We have reviewed our score for the risk of bias table and have only scored 'low risk of bias', for studies that specifically mention blinding of outcome assessors or those handling the data if outcomes were self-report measures filled in at home. The difference in our initial score was perhaps related to how to score self-report questionnaires, or surveys filled out at home, with no research staff around. However we have now scored these as unclear, if no mention is made of blinding of personnel, e.g those handling the data. The table has been revised to reflect this.

3) Please consider re-crafting the conclusion. I have four issues here. Firstly you make a very strong and general statement regarding there being a 'lack of theoretically derived approaches to exercise adherence'. This is not technically true as a stand-alone statement, I suggest adding 'in physiotherapy interventions for older people' or words to that effect. Or, secondly, re-consider the statement entirely as it is possible that it is a problem of poor reporting rather than an actual lack of theoretically derived approaches. This was the reason Michie and colleagues produced their taxonomy – to help people report clearly and consistently what behavioural theories (or techniques based on theory) they were using in interventions. What you have done is show how poor the reporting has been in your specific area and you could now call for a need for (a) better reporting as well as (b) better use or (c) better development of theory in this area. Thirdly, I suggest you re-visit the problem of measuring this type of exercise adherence, you have already noted this problem and I suggest research in this area is always going to be limited unless we address the lack of robust, validated measurement, so this could be mentioned in your limitations and or your conclusion (as a need for future research – as no point developing the new interventions if you cannot measure their effects!). Finally, pretty much all adherence research needs to consider the relationship between adherence and health outcome, in your study you have not mentioned this and whilst I do not think you need to add this in terms of what you are covering in your review, it is worth an acknowledgment - either in the limitations section and or your conclusion. This is because there is no point in promoting adherence if there is no beneficial health outcome or if the outcome is actually detrimental to health (which can happen in exercise interventions, for example increased risk of falls).

Response: Point 1 and Point 2 We agree that the conclusion need to be rephrased, we have changed the conclusion so that instead of 'lack of theoretically derived approaches to exercise adherence' It reads 'There is a need for better reporting, use and development of theoretically derived interventions in this area, in addition to the need for the development of robust measures of adherence '

Point 3 – We agree that the challenge of measuring adherence is of importance, we have amended both the limitations, future research and conclusion sections so that they consider measurement of adherence, in addition to referencing the following review – Bollen et al (2014)

Point 4 This is another important point and we have address this in the limitations sections as recommended

4) Adjust Table 1 so that studies are listed alphabetically - this is what you have done for the Risk of Bias Figure and having both the same makes it easier to compare.

Response: The table has been adjusted so that it is now listed alphabetically

5) I suggest writing in full all words abbreviated 'didn't', 'couldn't' etc throughout manuscript.

Response: Didn't, couldn't, it's etc have been replaced with full words throughout the manuscript.

6) Page 7, 3rd line: replace 'subjects' with 'participants'.

Response: Subjects has been replaced with participants

7) Page 8, 1st line. Sentence ending: 'evidence from other populations' would benefit from a supporting reference.

Response: References added

8) Page 9, 2nd line of conclusion. Delete 'with'?

Response: With has been deleted

Finally, this is a really useful, focused piece of work and I wish you well with your future research endeavours.

**Reviewer: 2**

Reviewer Name: Philippa Nicolson

Institution and Country: Centre for Health, Exercise and Sports Medicine, University of Melbourne, Australia

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

**Title**

The title is rather "clunky" in 3 parts, and does not accurately reflect the paper - the behaviour change techniques that interventions are based on is more of a focus than the theoretical background (as per abstract).

Response: We agree with the points made regarding the title and abstract and have changed the title from 'behavioural theories to techniques as suggested.

## Abstract

The objectives again mention the behavioural theories and not the behaviour change techniques, then the methods mention only the BCTs, and the results focuses on the BCTs - this is not clear.

"utilized behavioural approaches" would be clearer to say "explicitly utilized behavioural theory"

Response: As above, theory has been changed to techniques

## Introduction

The first paragraph jumps straight to physiotherapists prescribing exercise - what about other health professionals who prescribe exercise? This opening paragraph does not "grab" the reader

Response: The opening paragraph has been amended in line with the suggestions from yourself and reviewer Dean.

2nd paragraph "it is known that exercise adherence can affect treatment outcomes" - would be good to elaborate on this - how?

Response: This section has been made clearer – to reflect that better exercise adherence has been linked to pain, physical function, physical performance and self-perceived effect.

Comment: Paragraph about predictors of adherence is not directly relevant to this review.

Response: We felt it useful to outline the predictors of adherence, particularly with regard to the predictors of adherence for older people, in order to build a line of argument that there is need for adherence interventions, and for those interventions to be reviewed.

Comment: The work of Michie et al in developing the Behaviour change technique taxonomy is not introduced.

Response: Thank you for pointing this out, the introduction has been edited so that the taxonomy is introduced.

Comment: The final paragraph mentions the Cochrane review of interventions to improve exercise adherence but does not include more recent published reviews that are specific to older populations, such as:

Ezzat AM, MacPherson K, Leese J, Li LC. The Effects of Interventions to Increase Exercise Adherence in People with Arthritis: A Systematic Review. *Musculoskeletal Care* 2015; 1.

Nicolson PJA, Bennell KL, Dobson FL, Van Ginckel A, Holden MA, Hinman RS. Interventions to increase adherence to therapeutic exercise in older adults with low back pain and/or hip/knee osteoarthritis: a systematic review and meta-analysis. *Br J Sports Med* 2017; 51.

Williamson W, Kluzek S, Roberts N, Richards J, Arden N, Leeson P, et al. Behavioural physical activity interventions in participants with lower-limb osteoarthritis: a systematic review with meta-analysis. *BMJ Open* 2015; 5: e007642.

Response: We agree that these reviews add to the introduction. The papers list above have been included in the paragraph mentioned.



## Methods

Eligibility criteria - was the comparator group required to also be completing exercise - this is not clear?

Response: Yes, this has been clarified in the methods

## Data synthesis –

a little more detail about the behaviour change technique taxonomy / how interventions/intervention components were categorised is needed.

Response: We agree further detail could be given. This has been added to the data synthesis section

## Results

Risk of bias - assessment completed but not discussed - were there common areas of bias across the studies?

Response: Yes, particularly that the included studies had relatively small sample sizes/lack of sample size justification, this has been added to the risk of bias section.

## Behavioural theories –

were the interventions based on theory more effective? It would be good to include intervention content rather than just the authors names in relation to each study.

Response: This section has been updated in line with the suggestions given. Clarification over the effectiveness (significant results) of the studies that used behavioural theories is given in the behavioural theories section of the results . Also the intervention content has been added in addition to the authors names to this section.

## Discussion

More recent relevant reviews not included as mentioned in introduction comment.

Response: As with the introduction reference has been made to the reviews by Ezzat et al, and Nicolson et al in the discussion.

Comment: When discussing the benefits of feedback and monitoring in context no link is made to published work identifying barriers and facilitators to exercise adherence - this would help to justify the suggestion that these interventions are worthy of further exploration.

Response: Thanks for this great suggestion .A possible link between feedback and monitoring and barriers and facilitators has been added to the relevant paragraph in the discussion.

Comment: Issue of contextual equivalence would be good to raise and discuss - given that some included studies compared two groups completing totally different interventions.

Response: This is a great point, we agree that contextual equivalence is important in this areas, and we have now amended the conclusion so that it is included.

## Strengths and limitations –

measurement of adherence is first mentioned here - this is certainly an issue but should be raised earlier.

Response: The issue of measurement has been given more attention in line with your comments, and also comments from reviewer Dean. It is now discussed in the discussion, future research and strengths and limitations sections.

Comment: Figure 2 - error in box "Records after removal duplicates removed = 47" should be "Duplicates removed = 47"

Response: Thank you for pointing this out Figure 2 has been amended

### VERSION 2 – REVIEW

<b>REVIEWER</b>	Sarah Dean University of Exeter Medical School, UK.
<b>REVIEW RETURNED</b>	23-Oct-2017

<b>GENERAL COMMENTS</b>	<p>Thank you for asking me to re-review this paper. Nearly all of the requested revisions have either been made or addressed in the author's response document. I have only a few very minor further suggestions to make:</p> <p>1) I suggested that when clarifying the difference between physical activity and exercise / exercise as prescribed by physiotherapists that the term 'therapeutic' would be worthwhile using (so as to distinguish exercise that might be part of a physical activity / health promotion type programme for people not needing 'treatment' for a health condition); the authors do use the term 'therapeutic' on page 4 of the revised manuscript but not in the opening paragraph of the Introduction (page 2) when exercise is being defined. I would recommend inserting the term here as well e.g. line two could start: 'This type of therapeutic exercise is defined as a subset.....' etc.</p> <p>2) Paragraph two of the Introduction, line 5. I suggest clarifying that you mean 'low levels' of adherence may limit the effectiveness of prescribed exercise.</p> <p>3) Page 5, Quality assessment section, line 11. I personally do not agree that 'opaque sealed envelopes' are a 'low risk' method of allocation concealment (see Clark et al 2016) but accept that the Cochrane handbook (2017) does indicate that 'sequentially numbered non-opaque envelopes' are considered low risk.</p> <p>4) Data synthesis section, page 6. BCTT needs to be written in full first time it is mentioned.</p> <p>5) I note that you have explained in the Quality Assessment section (page 5) how you judged risk regarding the blinding of outcome assessment (in this case adherence measures), this is useful, however I think it is a shame you have not inserted the information about who measured adherence in Table 1 so we can see more easily how you made this quality assessment.</p> <p>6) Social support section, page 11. Second and third sentences do not make full sense.</p> <p>7) Discussion, page 13 line 4. Do you mean: 'The inconclusive results mirror similar results to adherence 'promoting interventions'</p>
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in other populations'?

8) Discussion, page 13, second paragraph, line 9. The same issue as for point 2 above, when mentioning self-efficacy and motivation it needs to be clear that you mean 'low levels' of self-efficacy and motivation, thank you.

9) Discussion, page 13, third paragraph, line 5. This sentence could be phrased a little better: 'adherence is essentially measuring behaviour change' as 'adherence' cannot measure anything; did you mean: 'Measurement of adherence is essentially very similar to the measurement of behaviour change'?

10) Discussion page 14, line 16-17. Did you mean robust adherence outcome measures? It just gets confusing if you use just the term 'outcome' and may be better to clarify that you mean outcome measures of interventions designed to promote adherence and not outcome measures of a health condition. Final sentence of this paragraph also has a typo – 'considered' not 'consider' and needs a full stop.

11) Discussion, strengths and limitations section, page 14 lines 9-10. Suggest enhance the clarity of these sentences: a) by inserting 'promoting interventions' and 'health outcome' so that line 9 reads: 'to analyse health outcomes, adherence promoting interventions do need to be considered in the context of these health outcome results' and b) in line 10 explaining more clearly what you mean (I agree it is an important consideration but be better to say exactly what this consideration is) – 'for it is only appropriate to intervene to promote adherence if there is also concomitant improvement in health outcome, or at least no harm caused by the adherence promoting intervention' – or words to that effect.

12) Excellent revisions to the conclusion, thank you.

#### References

Clark, L., Fairhurst, C., Torgerson, D.J. (2016). Allocation concealment in randomised controlled trials: are we getting better? *BMJ*;355:i5663 doi: 10.1136/bmj.i5663.  
Higgins JPT, Altman DG, Sterne JAC (editors). Chapter 8: Assessing risk of bias in included studies. In: Higgins JPT, Churchill R, Chandler J, Cumpston MS (editors), *Cochrane Handbook for Systematic Reviews of Interventions* version 5.2.0 (updated June 2017), Cochrane, 2017.

<b>REVIEWER</b>	Philippa Nicolson Centre for Health, Exercise and Sports Medicine University of Melbourne Australia
<b>REVIEW RETURNED</b>	08-Oct-2017
<b>GENERAL COMMENTS</b>	The authors have done a thorough job of addressing the Reviewers comments. The paper reads very well and I look forward to seeing it published.

## VERSION 2 – AUTHOR RESPONSE

### Reviewer: 2

Reviewer Name: Philippa Nicolson

Institution and Country: Centre for Health, Exercise and Sports Medicine, University of Melbourne, Australia

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

Comment: The authors have done a thorough job of addressing the Reviewers comments. The paper reads very well and I look forward to seeing it published.

Response: Many thanks to Philippa Nicolson for reviewing the original and revision of the manuscript. Thank you for your comments which have improved the paper.

### Reviewer: 1

Reviewer Name: Sarah Dean

Institution and Country: University of Exeter Medical School, UK.

Please state any competing interests or state 'None declared': 'None declared'

Please leave your comments for the authors below

Comment: What interventions are used to improve exercise adherence in older people? And what behavioural theories are they based on? A Systematic Review.  
BMJ-Open – 2017 – 019221

Response: Thank you for asking me to re-review this paper. Nearly all of the requested revisions have either been made or addressed in the author's response document. I have only a few very minor further suggestions to make:

Many thanks to Sarah Dean for reviewing both the original and revised version of this paper, your comments on the original and revised version have improved the paper. I have responded to each comment below.

1) I suggested that when clarifying the difference between physical activity and exercise / exercise as prescribed by physiotherapists that the term 'therapeutic' would be worthwhile using (so as to distinguish exercise that might be part of a physical activity / health promotion type programme for people not needing "treatment" for a health condition); the authors do use the term 'therapeutic' on page 4 of the revised manuscript but not in the opening paragraph of the Introduction (page 2) when exercise is being defined. I would recommend inserting the term here as well e.g. line two could start: 'This type of therapeutic exercise is defined as a subset.....' etc.

Response: Thank you for making this point, we agree that using the term therapeutic will help to add clarity. The term therapeutic has been added to the introduction on page 2, the line in question now begins - 'This type of therapeutic exercise is defined as a...'

2) Paragraph two of the Introduction, line 5. I suggest clarifying that you mean 'low levels' of adherence may limit the effectiveness of prescribed exercise.

Response: We agree that this clarification would be helpful. The line in question has been changed so that it now reads - 'Therefore low levels of adherence may limit the effectiveness of prescribed exercise'

3) Page 5, Quality assessment section, line 11. I personally do not agree that 'opaque sealed envelopes' are a 'low risk' method of allocation concealment (see Clark et al 2016) but accept that the Cochrane handbook (2017) does indicate that 'sequentially numbered non-opaque envelopes' are considered low risk.

Response: Thank you for making this interesting and important point around allocation concealment, and also for forwarding the Clark et al reference. When assessing risk of bias, we followed the Cochrane handbook, which lists sequentially numbered, opaque, sealed envelopes as low risk of bias. That being said, no studies included in this review record this method in their paper.

4) Data synthesis section, page 6. BCTT needs to be written in full first time it is mentioned.

Response: Behaviour Change Technique Taxonomy is mentioned in the introduction page 3 paragraph 2, it is written in full before the BCTT abbreviation is given.

5) I note that you have explained in the Quality Assessment section (page 5) how you judged risk regarding the blinding of outcome assessment (in this case adherence measures), this is useful, however I think it is a shame you have not inserted the information about who measured adherence in Table 1 so we can see more easily how you made this quality assessment.

Response: Thank you for this helpful suggestion. Table 1 has been amended, in the 'measure of adherence' column we have added who measured/collected data about adherence, if this information was given in the original paper. Where no information was provided, nothing was added for that particular study in this column. We hope this gives clarity around how judgement was made as part of the quality assessment.

6) Social support section, page 11. Second and third sentences do not make full sense.

Response: Thank you for noting this. The sentences have been edited, they now read – 'The intervention included weekly phone calls and one home visit over a 3 month period. The phone calls and visit included dealing with queries about exercise adherence and exercise maintenance, problem solving, discussion and recommendations about health problems and encouragement.'

7) Discussion, page 13 line 4. Do you mean: 'The inconclusive results mirror similar results to adherence 'promoting interventions' in other populations'?

Response: Yes, this is what was meant. The sentence has been edited to read – 'The inconclusive results mirror similar results to adherence prompting interventions in other populations'

8) Discussion, page 13, second paragraph, line 9. The same issue as for point 2 above, when mentioning self-efficacy and motivation it needs to be clear that you mean 'low levels' of self-efficacy and motivation, thank you.

Response: Thank you for pointing this out. This line has been edited to read - 'such as low self-efficacy and motivation'

9) Discussion, page 13, third paragraph, line 5. This sentence could be phrased a little better: 'adherence is essentially measuring behaviour change' as 'adherence' cannot measure anything; did you mean: 'Measurement of adherence is essentially very similar to the measurement of behaviour change'?

Response: This is a good point. This sentence has been edited to read - 'Measuring adherence is essentially measuring behaviour change in participants, i.e. the participant's behaviour corresponding to recommendation from a health care provider'

10) Discussion page 14, line 16-17. Did you mean robust adherence outcome measures? It just gets confusing if you use just the term 'outcome' and may be better to clarify that you mean outcome measures of interventions designed to promote adherence and not outcome measures of a health condition. Final sentence of this paragraph also has a typo – 'considered' not 'consider' and needs a full stop.

Response: This sentence has been edited to read - 'The papers included utilised a diverse range of adherence outcomes measures.'

The typo has been corrected

A full stop has been added

11) Discussion, strengths and limitations section, page 14 lines 9-10. Suggest enhance the clarity of these sentences: a) by inserting 'promoting interventions' and 'health outcome' so that line 9 reads: 'to analyse health outcomes, adherence promoting interventions do need to be considered in the context of these health outcome results' and b) in line 10 explaining more clearly what you mean (I agree it is an important consideration but be better to say exactly what this consideration is) – 'for it is only appropriate to intervene to promote adherence if there is also concomitant improvement in health outcome, or at least no harm caused by the adherence promoting intervention' – or words to that effect.

Response: Thank you for these comments -

a) The sentence has been amended to read -

'Another consideration is that although it was beyond the scope of this review to analyse health outcomes, adherence promoting interventions do need to be considered in the context of these health outcome results'

b) The following has been added

'For it is appropriate to ensure that intervening to promote adherence also offers an improvement in health outcome, or at least causes no harm'

12) Excellent revisions to the conclusion, thank you.

Response: Thank you

#### References

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### VERSION 3 – REVIEW

<b>REVIEWER</b>	Sarah Dean University of Exeter Medical School, UK
<b>REVIEW RETURNED</b>	30-Oct-2017
<b>GENERAL COMMENTS</b>	All revisions completed, thank you. This is a really interesting paper and I wish you good luck with your future research in this area.