Peer Review History

BMJ Open-2010-000035

Tai chi for the treatment of osteoarthritis: a systematic review and meta-analysis

Date Submitted: 15-Dec-2010

Jung Won Kang, Myeong Soo Lee, Paul Posadzki, Edzard Ernst

Reviewer 1: Maher, Chris

Reviewer Affiliation: The George Institute for International Health, Musculoskeletal Division

The Study	Yes	No
Is the research question clearly defined?	✓	
Is the overall study design appropriate and adequate to answer the research question?	✓	
Are the participants adequately described, their conditions defined, and the inclusion and exclusion criteria described?	✓	
Are the patients representative of actual patients the evidence might affect?	✓	
Are the methods adequately described?		✓
Is the main outcome measure clear?	✓	
Are the abstract/summary/key messages/limitations accurate?		✓
Are the statistical methods described?	✓	
Are they appropriate?		✓
Is the standard of written English acceptable for publication?	✓	
Are the references up to date and relevant? (If not, please provide details of significant omissions below.)	✓	
Do any supplemental documents e.g. a CONSORT checklist, contain information that should be better reported in the manuscript, or raise questions about the work?	✓	

If you answered No to any of the above, please supply details below.
I enjoyed reading this review and offer some comments that I trust will assist the authors to improve their manuscript. I look forward to seeing it published. Please refer to the PRISMA checklist to ensure that you report all aspects of the review. For example you do not say if you followed a registered protocol (item 5) or present the full electronic search (item 8). The manuscript is seemingly contradictory with regard to trial quality. In the second paragraph of the discussion you say that the studies had a low risk of bias but abstract and conclusion refers to the often poor quality of trials. Please explain how you inferred variances for change scores. In trials with multiple end points please explain how you chose the end point to include in the study. In the forest plots can you make clear what time point you are referring to.

RESULTS AND CONCLUSION (For articles reporting research findings only)	Yes	No
Do the results answer the research question?		✓
Are they credible?		✓
Are they well presented?	✓	
Are the interpretation and conclusions warranted by and sufficiently derived from/focused on the data?		✓
Are they discussed in the light of previous evidence?		✓
Is the message clear?	✓	

If you answered No to any of the above, please supply details below.
I am aware of two studies that have evaluated the risk of bias tool and they both conclude that it has problems (see J Eval Clin Pract 2010 Aug 3 [Epub ahead of print] and BMJ 2009;339:b4012). The authors need to make clear that the tool they used to assess risk of bias provides unreliable measurements and acknowledge the limitations of these measurements in the manuscript. I was surprised to see that the SDs for the Wang study were exactly the same in the Tai Chi and Control groups for the pain, stiffness and function outcomes. I checked the original paper and this was what was reported. I also checked the pain scores and found that the SDs were also the same, to two decimal places, for each time point. I cannot believe that this is correct and I think the authors need to contact the authors to find out what has happened here or at least discuss this issue in the manuscript. The review ratings of blinding appear wrong and/or inconsistent. As the outcomes are self-report and the patients are not blinded then the assessments cannot be blinded. However the blinding ratings include yes & unclear. I think they should all be no. In the introduction the authors harshly dismiss both previous reviews on this topic and also fail to discuss their own findings in relation to these two reviews. The dismissal of the Hall review is harsh because the authors describe the work as flawed in several respects but only cite one issue: missing studies. Missing studies is a curious justification for a new review because you can never be sure of the extent of this problem until you actually undertake the new search. In this case the authors of the current review have missed studies from the Hall review and the Hall review has missed studies from the current review. Rather than describing one or the other review as flawed it would be more instructive to discuss the differences in included studies and also compare the conclusions of the different reviews. This would provide the current study with more depth and so value. As the second author of the Hall review I need to declare an interest here. I think presenting p values for intergroup differences is of limited value and should be dropped from the table. Readers want to know about the precision of the estimate of the treatment effect and an isolated p value does not help in this regard. Similarly I am not particularly interested in the authors conclusions because it has been well documented that author conclusions are typically more optimistic than the data justify. I would drop this. I would encourage the authors to adopt the BMJ structured discussion style. Two issues from the BMJ style that could be added, and would greatly improve the manuscript are: (i) strengths and weaknesses in relation to other studies, discussing important differences in results (ii) meaning of the study: possible explanations and implications for clinicians and policymakers.

REPORTING AND ETHICS	Yes	No
Is the article reported in line with the appropriate reporting statement or checklist (e.g. CONSORT)?		✓
Are research ethics (e.g. consent, ethical approval) addressed appropriately?	✓	
Is the article free from any concerns about publication ethics (e.g. plagiarism, fabrication, redundant publication, undeclared conflicts of interest)?	✓	

If you answered No to any of the above, please supply details below or contact the editorial office.

The authors need to provide some additional information to conform to PRISMA

req **BMJ Open uses compulsory open peer review. Your name and institution will be returned to the authors and will be published with this review if the article is accepted. Therefore please sign your review in the box below. Include your name, position, institution and country. Please also include a statement of competing interests. If you have filled out an ICMJE Conflicts of Interests form - please attach this using the box beneath instead.**

I am second author on the Hall Tai Chi review that was referred to by the authors in the introduction.

Professor Chris Maher | BAppSc(Phty), PhD
Director, Musculoskeletal Division
Professor Sydney Medical School, University of Sydney
NHMRC Senior Research Fellow

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E cmaher@georgeinstitute.org.au | W www.georgeinstitute.org.au

Reviewer 2: Chen, Kevin

Reviewer Affiliation: University of Maryland School of Medicine, Center for Integrative Medicine and Dept of Psychiatry

The Study	Yes	No
Is the research question clearly defined?	✓	
Is the overall study design appropriate and adequate to answer the research question?	✓	
Are the participants adequately described, their conditions defined, and the inclusion and exclusion criteria described?	✓	
Are the patients representative of actual patients the evidence might affect?		✓
Are the methods adequately described?	✓	
Is the main outcome measure clear?	✓	
Are the abstract/summary/key messages/limitations accurate?	✓	
Are the statistical methods described?	✓	
Are they appropriate?	✓	
Is the standard of written English acceptable for publication?	✓	
Are the references up to date and relevant? (If not, please provide details of significant omissions below.)	✓	
Do any supplemental documents e.g. a CONSORT checklist, contain information that should be better reported in the manuscript, or raise questions about the work?	✓	

If you answered No to any of the above, please supply details below.
Min-body intervention like Taiji is a self-applied therapy requesting time commitment and quality practice, it is a voluntary wellbeing activity that add the benefits of general health both physically and psychologically. Most practitioners of Taiji hardly will experience any OA, while most of OA patients may not want to commit their time doing Taiji. Conventional RCT may not be the best tool to evaluate something like Taiji or other self applied mind-body exercises....

RESULTS AND CONCLUSION (For articles reporting research findings only)	Yes	No
Do the results answer the research question?	✓	
Are they credible?	✓	
Are they well presented?	✓	
Are the interpretation and conclusions warranted by and sufficiently derived from/focused on the data?	✓	
Are they discussed in the light of previous evidence?	✓	
Is the message clear?	✓	

If you answered No to any of the above, please supply details below.

REPORTING AND ETHICS	Yes	No
Is the article reported in line with the appropriate reporting statement or checklist (e.g. CONSORT)?	✓	
Are research ethics (e.g. consent, ethical approval) addressed appropriately?	✓	
Is the article free from any concerns about publication ethics (e.g. plagiarism, fabrication, redundant publication, undeclared conflicts of interest)?		✓

If you answered No to any of the above, please supply details below or contact the editorial office.
I am not sure this my comment is approrpiate here -- I have read many reviews by this group . They are generally good quality itself, but the quality of reviewed studies themselves was not very high, really not worth the systematic review yet. I am especially concerned about this one since the same authors just published the same review 2 years ago, although there are a few more new studies in the field, the conclusions are exactly same -- general positive trend, small sample, low quality of study. They published many similar reviews with the same conclusions in this area, and the quality itself was affected since they are so similar to each other... If we encourage this kind repeated reviews to be published too frequently, it really did not bring anything new to the research field except telling clinicians that the evidence is not there yet....

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No conflict of interests, here

Comments

If you have any further comments for the authors please enter them below.

This is a well conducted systematic review by a group with a lot of experience in this area. My main concerns are the following:

1) What is new to the field compared to the review published by the same author in 2008? Although there are some new publications included in the review, but their weakness are almost the same -- small sample, methodoligcal faults, lack of comatible control and lack of details in instruction of exerices.... My feeling is we are going to say the same things for next 10 year in review of mind-body medicine research since these problems are buildt in the field itself -- it is a self-applied wellbeing exercise, not a clinician applied therapy. We need change our perspective and method of review for "therapy" like these.

2) Related to my first concern, I am not sure if the quality assessment certieria by Cochrane are really best tool for assessing something like Taiji, a self-applied exercise that is really part of a wellbeing lifestyle, instead of a clinical therapy. Some of the criteria in RCT may not be applied for study of benefits of Taiji. For example, the blindness, if the main outcome is WOMAC, which is self-reported measure, how can you expect any researcher to design a blinded study to assess the outcome? I hope the reviewers are more practical and realistic on the interventino itself, and develop more practical and useful tool to assess clinical studies of mind-body medicine intervention.

developed by

Authors Response to Decision Letter for (BMJ Open-2010-000035)

Tai chi for the treatment of osteoarthritis: a systematic review and meta-analysis

Dear Mr. Richard Sands,

RE: Tai chi for the treatment of osteoarthritis: a systematic review and meta-analysis (BMJ Open-2010-000035)

On the behalf of my co-author, I would like to thank you for arranging peer-review of our manuscript and for your invitation to submit a further revised version. We appreciate the effort of the reviewers, and believe that their constructive suggestions have resulted in a stronger manuscript for BMJ Open's readers.

Yours sincerely

Myeong Soo Lee, PhD

Dear Reviewer,

We appreciate the constructive criticism by the reviewers on our manuscripts. According to the suggestions, my colleagues and I made the appropriate changes, point by point. The responses are detailed as followings.

Reviewer(s)' Comments to Author:
[Reviewer 1]
I enjoyed reading this review and offer some comments that I trust will assist the authors to improve their manuscript. I look forward to seeing it published.

Comment 1) Please refer to the PRISMA checklist to ensure that you report all aspects of the review. For example you do not say if you followed a registered protocol (item 5) or present the full electronic search (item 8).
Revised> We did not publish the protocol for this review. We have noted it in the text (page 6, 1st paragraph, last line) and added the search strategies as supplement 1.
Comment 2) The manuscript is seemingly contradictory with regard to trial quality. In the second paragraph of the discussion you say that the studies had a low risk of bias but abstract and conclusion refers to the often poor quality of trials.
Revised> We have extended the items of risk of bias. We have re-evaluated them (page 9, 2nd paragraph and page 15, 2nd paragraph) and added the details in table 2.
Comment 3) Please explain how you inferred variances for change scores.
Revised> We have added the reference for it (reference12. Follmann D, Elliott P, Suh I, Cutler J. Variance imputation for overviews of clinical trials with continuous response. J Clin Epidemiol 1992;45(7):769-73.).

Comment 4) In trials with multiple end points please explain how you chose the end point to include in the study. In the forest plots can you make clear what time point you are referring to.
Revised> We have added the details in page 7, 2nd paragraph, line1-3.
Comment 5) I am aware of two studies that have evaluated the risk of bias tool and they both conclude that it has problems (see J Eval Clin Pract 2010 Aug 3 [Epub ahead of print] and BMJ 2009;339:b4012). The authors need to make clear that the tool they used to assess risk of bias provides unreliable measurements and acknowledge the limitations of these measurements in the manuscript.
Revised> We have now added the limitations of Cochrane risk of bias tool (page 15, 1st paragraph, lines 1-5 from the bottoms).
Comment 6) I was surprised to see that the SDs for the Wang study were exactly the same in the Tai Chi and Control groups for the pain, stiffness and function outcomes. I checked the original paper and this was what was reported. I also checked the pain scores and found that the SDs were also the same, to two decimal places, for each time point. I cannot believe that this is correct and

I think the authors need to contact the authors to find out what has happened here or at least discuss this issue in the manuscript.
Revised> We contacted the authors and received the reply. The following is the reply from the authors.
The basic explanation is that the standard errors that are identical are the standard errors of the combinations that comprise the control and treatment means. Since the model is a mixed model with AR(1) error structure and random intercepts, there is a symmetry in the estimated covariance matrix of the regression coefficients such that the covariances between two terms are always 1/2 of the ratio of the corresponding standard errors. Because of cancellation in the computations, the standard errors are the same. This is in essence a constant variance assumption in the regression model.
I received the details of data for the outcomes. If you need the further information, please contact the original authors or me.
Comment 7) The review ratings of blinding appear wrong and/or inconsistent. As the outcomes are self-report and the patients are not blinded then the assessments cannot be blinded. However the blinding ratings include yes & unclear. I think they should all be no.
Revised> We assumed the row risk of bias for assessor blinding if specified in the text regardless of the type of outcome measures (even for self-reported outcome measures) (page 7, 1st paragraph). We've also added the limitation of this in the discussion (page 15, 2nd paragraph, lines 1-3 from the bottoms).
Comment 8) In the introduction the authors harshly dismiss both previous reviews on this topic and also fail to discuss their own findings in relation to these two reviews. The dismissal of the Hall review is harsh because the authors describe the work as flawed in several respects but only cite one issue: missing studies. Missing studies is a curious justification for a new review because you can never be sure of the extent of this problem until you actually undertake the new search. In this case the authors of the current review have missed studies from the Hall review and the Hall review has missed studies from the current review. Rather than describing one or the other review as flawed it would be more instructive to discuss the differences in included studies and also compare the conclusions of the different reviews. This would provide the current study with more depth and so value. As the second author of the Hall review I need to declare an interest here.
Revised> We have the details in the introduction (page 4, 2nd paragraph).
Comment 9) I think presenting p values for intergroup differences is of limited value and should be dropped from the table. Readers want to know about the precision of the estimate of the treatment effect and an isolated p value does not help in this regard. Similarly I am not particularly interested in the authors conclusions because it has been well documented that author conclusions are typically more optimistic than the data justify. I would drop this.
Revised> We have now delete them.
Comment 10) I would encourage the authors to adopt the BMJ structured discussion style. Two issues from the BMJ style that could be added, and would greatly improve the manuscript are: (i) strengths and weaknesses in relation to other studies, discussing important differences in results (ii) meaning of the study: possible explanations and implications for clinicians and policymakers.
Revised> We tried to follow this comment in the discussion (page 14, 2nd and 3rd paragraph)

Comment 11) The authors need to provide some additional information to conform to PRISMA
Revised> We have followed the PRISMA guideline.

[Reviewer 2]

Min-body intervention like Taiji is a self-applied therapy requesting time commitment and quality practice, it is a voluntary wellbeing activity that add the benefits of general health both physically and psychologically. Most practitioners of Taiji hardly will experience any OA, while most of OA patients may not want to commit their time doing Taiji. Conventional RCT may not be the best tool to evaluate something like Taiji or other self applied mind-body exercises....

I am not sure this my comment is approrpiate here -- I have read many reviews by this group . They are generally good quality itself, but the quality of reviewed studies themselves was not very high, really not worth the systematic review yet. I am especially concerned about this one since the same authors just published the same review 2 years ago, although there are a few more new studies in the field, the conclusions are exactly same -- general positive trend, small sample, low quality of study. They published many similar reviews with the same conclusions in this area, and the quality itself was affected since they are so similar to each other... If we encourage this kind repeated reviews to be published too frequently, it really did not bring anything new to the research field except telling clinicians that the evidence is not there yet....

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Comment 1) What is new to the field compared to the review published by the same author in 2008? Although there are some new publications included in the review, but their weakness are almost the same -- small sample, methodoligcal faults, lack of comatible control and lack of details in instruction of exerices.... My feeling is we are going to say the same things for next 10 year in review of mind-body medicine research since these problems are buildt in the field itself -- it is a self-applied wellbeing exercise, not a clinician applied therapy. We need change our perspective and method of review for "therapy" like these.
Revised> We have now added the this points in the introduction (page 4, 2nd paragraph).
Comment 2) Related to my first concern, I am not sure if the quality assessment certieria by Cochrane are really best tool for assessing something like Taiji, a self-applied exercise that is really part of a wellbeing lifestyle, instead of a clinical therapy. Some of the criteria in RCT may not be applied for study of benefits of Taiji. For example, the blindness, if the main outcome is WOMAC, which is self-reported measure, how can you expect any researcher to design a blinded study to assess the outcome? I hope the reviewers are more practical and realistic on the interventino itself, and develop more practical and useful tool to assess clinical studies of mind-body medicine intervention.
Revised> We have now added the this points in the discussion (page 7, 1st paragraph, lines 4-7 and page 15, 2nd paragraph, lines 1-3 from the bottoms).

Thank you for your valuable comments.

Best wishes,

Myeong Soo Lee, PhD

BMJ Open-2010-000035.R1

Tai chi for the treatment of osteoarthritis: a systematic review and meta-analysis

Date Submitted: 31-Jan-2011

Jung Won Kang, Myeong Soo Lee, Paul Posadzki, Edzard Ernst

Reviewer 1: Maher, Chris

Reviewer Affiliation: The George Institute for International Health, Musculoskeletal Division

The Study	Yes	No
Is the research question clearly defined?	✓	
Is the overall study design appropriate and adequate to answer the research question?	✓	
Are the participants adequately described, their conditions defined, and the inclusion and exclusion criteria described?	✓	
Are the patients representative of actual patients the evidence might affect?	✓	
Are the methods adequately described?		✓
Is the main outcome measure clear?		✓
Are the abstract/summary/key messages/limitations accurate?		✓
Are the statistical methods described?		✓
Are they appropriate?		✓
Is the standard of written English acceptable for publication?		✓
Are the references up to date and relevant? (If not, please provide details of significant omissions below.)	✓	
Do any supplemental documents e.g. a CONSORT checklist, contain information that should be better reported in the manuscript, or raise questions about the work?		✓

If you answered No to any of the above, please supply details below.
I still think the authors are unfairly harsh in their dismissal of the Hall review. The criticism that it missed all available primary studies is unfair because the Hall review missed one available trial (Song 2003) and the current review also missed one available trial (Song 2007). The criticism that the Hall review pooled all 7 RCTs is incorrect. This did not occur. In the discussion the authors are critical of the Hall review (and another) because they included trials of poor methodological quality but so did the authors. It would be far more constructive if the authors took a balanced approach and truly considered the strengths and weaknesses of their review and the other two reviews rather than selectively citing perceived flaws in the other two reviews. For example an important issue would be to explore why the Hall and current reviews each both missed one trial. If the problem in the search strategy that led to the missed trials could be identified then something important would have been learnt for future reviews. The authors have not provided sufficient information on the variance estimation method they used. Please include the equation not simply one parameter for the equation. The authors have not provided sufficient information on the reliability of the risk of bias scale that they used. The single sentence is ungrammatical and completely unclear. The information the authors provided on the standard deviations for the Wang study makes little sense. The response from Wang relates to how he modelled the data (where he forced the model to specify the same variance) but what was required here is the standard deviations of the original raw data. I think it would be preferable for the authors to go back to the author and ask for the original data. If this is not available they would need to be more explicit about the issue and address it as a limitation of the review. It remains unclear what time points the outcomes pertain to. The authors response to my previous question on this was that this information is provided on page 7 paragraph 2 but there is only one paragraph on page 7. On a related issue on page 9 the authors state that the data were validated and abstracted according to pre-defined criteria but never specify what these criteria are.

RESULTS AND CONCLUSION (For articles reporting research findings only)	Yes	No
Do the results answer the research question?		
Are they credible?		
Are they well presented?		
Are the interpretation and conclusions warranted by and sufficiently derived from/focused on the data?		
Are they discussed in the light of previous evidence?		✓
Is the message clear?		✓

If you answered No to any of the above, please supply details below.
The authors state that they are confident that they located all data but they did not include the Song et al 2007 trial included in the Hall review so this statement seems hollow. The authors need to say why they are confident they did not miss trials and also why the Song et al 2007 trial was not included in this review. The authors assessed risk of bias with a tool of low reliability, but this issue is completely ignored in the discussion and conclusion.

REPORTING AND ETHICS	Yes	No
Is the article reported in line with the appropriate reporting statement or checklist (e.g. CONSORT)?	✓	
Are research ethics (e.g. consent, ethical approval) addressed appropriately?	✓	
Is the article free from any concerns about publication ethics (e.g. plagiarism, fabrication, redundant publication, undeclared conflicts of interest)?	✓	

If you answered No to any of the above, please supply details below or contact the editorial office.

req BMJ Open uses compulsory open peer review. Your name and institution will be returned to the authors and will be published with this review if the article is accepted. Therefore please sign your review in the box below. Include your name, position, institution and country. Please also include a statement of competing interests. If you have filled out an ICMJE Conflicts of Interests form - please attach this using the box beneath instead.
Professor Chris Maher | BAppSc(Phty), PhD Director, Musculoskeletal Division Professor Sydney Medical School, University of Sydney I was an author on the Hall Tai Chi review

Authors Response to Decision Letter for (BMJ Open-2010-000035.R1)

Tai chi for the treatment of osteoarthritis: a systematic review and meta-analysis

Dear Mr. Richard Sands,

RE: Tai chi for the treatment of osteoarthritis: a systematic review and meta-analysis (BMJ Open-2010-000035R1)

On the behalf of my co-author, I would like to thank you for arranging peer-review of our manuscript and for your invitation to submit a further revised version. We appreciate the effort of the reviewers, and believe that their constructive suggestions have resulted in a stronger manuscript for BMJ Open's readers.

Yours sincerely

Myeong Soo Lee, PhD

Dear Reviewer,

We appreciate the constructive criticism by the reviewers on our manuscripts. According to the suggestions, my colleagues and I made the appropriate changes, point by point. The responses are detailed as followings.

Reviewer(s)' Comments to Author:

[Reviewer 1]

Comment 1) I still think the authors are unfairly harsh in their dismissal of the Hall review. The criticism that it missed all available primary studies is unfair because the Hall review missed one available trial (Song 2003) and the current review also missed one available trial (Song 2007). The criticism that the Hall review pooled all 7 RCTs is incorrect. This did not occur. In the discussion the authors are critical of the Hall review (and another) because they included trials of poor methodological quality but so did the authors. It would be far more constructive if the authors took a balanced approach and truly considered the strengths and weaknesses of their review and the other two reviews rather than selectively citing perceived flaws in the other two reviews. For example an important issue would be to explore why the Hall and current reviews each both missed one trial. If the problem in the search strategy that led to the missed trials could be identified then something important would have been learnt for future reviews.

--Revised > We've now delete the raised issue in the introduction. We did not discount the reviewer's work. The two papers referred by reviewer were from one trial by the same authors (Song 2003 and Song 2007). The authors reported different outcome measures for already published trial and data are exactly the same. In the discussion, the sentence was wrongly modified during the English proof reading stage by Professional English proof reading company. We have now changed the sentence (page 14, 2nd paragraph, last sentence).

Comment 2) The authors have not provided sufficient information on the variance estimation method they used. Please include the equation not simply one parameter for the equation.

--Revised > We have now added more details in the methods section (page 9, lines 2-6). However, we explained the details of procedure with reference instead of including the equation. The reference includes the equation, example and etc.

Comment 3) The authors have not provided sufficient information on the reliability of the risk of bias scale that they used. The single sentence is ungrammatical and completely unclear.

--Revised > We have now added the paragraph for this comment (page 16, 1st paragraph). We also showed the reliability of our assessment.

Comment 4) The information the authors provided on the standard deviations for the Wang study makes little sense. The response from Wang relates to how he modelled the data (where he forced the model to specify the same variance) but what was required here is the standard deviations of the original raw data. I think it would be preferable for the authors to go back to the author and ask for the original data. If this is not available they would need to be more explicit about the issue and address it as a limitation of the review.

--Revised > We received the raw data from the author. It is the same as they reported. We have now added this point in the methods section (page 8, lines 2-5 from the bottom). I also discuss this matter with the statistician and found this situation is possible. However, the explanation of their statistical methods is beyond our work. The followings are the data from the original authors.

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womac.pain
Weeks Tai Chi Control
Estimate Lower Upper Estimate Lower Upper
0 209.30 169.60 249.00 220.35 180.65 260.05
12 52.05 12.35 91.75 181.90 142.20 221.60
24 77.75 38.05 117.45 155.75 116.05 195.45
48 93.95 54.25 133.65 151.15 111.45 190.85

Time Tai Chi Control
Difference
Estimate Lower Upper Pvalue Estimate Lower Upper Pvalue
Estimate Lower Upper Pvalue
0-12 -157.25 -203.11 -111.39 2e-11 -38.45 -84.31 7.41 0.100
-118.80 -183.66 -53.94 5e-04
0-24 -131.55 -177.41 -85.69 2e-08 -64.60 -110.46 -18.74 0.007
-66.95 -131.81 -2.09 5e-02
0-48 -115.35 -161.21 -69.49 8e-07 -69.20 -115.06 -23.34 0.004
-46.15 -111.01 18.71 2e-01

womac.phys.func
Weeks Tai Chi Control
Estimate Lower Upper Estimate Lower Upper
0 707.60 572.28 842.92 827.00 691.68 962.32
12 200.85 65.53 336.17 644.85 509.53 780.17
24 267.10 131.78 402.42 569.70 434.38 705.02
48 301.75 166.43 437.07 526.45 391.13 661.77

Time Tai Chi Control
Difference
Estimate Lower Upper Pvalue Estimate Lower Upper Pvalue
Estimate Lower Upper
0-12 -506.75 -640.66 -372.84 1e-13 -182.15 -316.06 -48.24 9e-03
-324.6 -513.98 -135.22
0-24 -440.50 -574.41 -306.59 1e-10 -257.30 -391.21 -123.39 3e-04
-183.2 -372.58 6.18
0-48 -405.85 -539.76 -271.94 3e-09 -300.55 -434.46 -166.64 2e-05
-105.3 -294.68 84.08
Pvalue
0-12 0.001
0-24 0.060
0-48 0.300

Comment 5) It remains unclear what time points the outcomes pertain to. The authors response to my previous question on this was that this information is provided on page 7 paragraph 2 but there is only one paragraph on page 7. On a related issue on page 9 the authors state that the data were validated and abstracted according to pre-defined criteria but never specify what these criteria are.

--Revised > We have now tried to add more clear sentence for this (page 8, lines 2-3 in the Quantitative data synthesis). We have also added the details of pre-defined criteria.

Comment 6) The authors state that they are confident that they located all data but they did not include the Song et al 2007 trial included in the Hall review so this statement seems hollow. The authors need to say why they are confident they did not miss trials and also why the Song et al 2007 trial was not included in this review.

--Answer> This was already explained in the reply to comment 1.

Comment 7) The authors assessed risk of bias with a tool of low reliability, but this issue is completely ignored in the discussion and conclusion.

--Revised > The answer is the same as the reply to comment 3.

Thank you for your valuable comments.

Best wishes,

Myeong Soo Lee, PhD

BMJ Open-2010-000035.R2

Tai chi for the treatment of osteoarthritis: a systematic review and meta-analysis

Date Submitted: 14-Feb-2011

Jung Won Kang, Myeong Soo Lee, Paul Posadzki, Edzard Ernst

Reviewer 1: Maher, Chris

Reviewer Affiliation: The George Institute for International Health, Musculoskeletal Division

The Study	Yes	No
Is the research question clearly defined?	✓	
Is the overall study design appropriate and adequate to answer the research question?	✓	
Are the participants adequately described, their conditions defined, and the inclusion and exclusion criteria described?	✓	
Are the patients representative of actual patients the evidence might affect?	✓	
Are the methods adequately described?	✓	
Is the main outcome measure clear?	✓	
Are the abstract/summary/key messages/limitations accurate?	✓	
Are the statistical methods described?	✓	
Are they appropriate?	✓	
Is the standard of written English acceptable for publication?		✓
Are the references up to date and relevant? (If not, please provide details of significant omissions below.)	✓	
Do any supplemental documents e.g. a CONSORT checklist, contain information that should be better reported in the manuscript, or raise questions about the work?	✓	

If you answered No to any of the above, please supply details below.
The additions to the text include some ungrammatical sections. The sense is clear but it looks a bit awkward. Some copyediting would help.

RESULTS AND CONCLUSION (For articles reporting research findings only)	Yes	No
Do the results answer the research question?	✓	
Are they credible?	✓	
Are they well presented?	✓	
Are the interpretation and conclusions warranted by and sufficiently derived from/focused on the data?	✓	
Are they discussed in the light of previous evidence?	✓	
Is the message clear?	✓	

REPORTING AND ETHICS	Yes	No
Is the article reported in line with the appropriate reporting statement or checklist (e.g. CONSORT)?	✓	
Are research ethics (e.g. consent, ethical approval) addressed appropriately?	✓	
Is the article free from any concerns about publication ethics (e.g. plagiarism, fabrication, redundant publication, undeclared conflicts of interest)?	✓	

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Comments

If you have any further comments for the authors please enter them below.

I look forward to reading the published paper

Authors Response to Decision Letter for (BMJ Open-2010-000035.R2)

Tai chi for the treatment of osteoarthritis: a systematic review and meta-analysis

Dear Mr. Richard Sands,

RE: Tai chi for the treatment of osteoarthritis: a systematic review and meta-analysis (BMJ Open-2010-000035R2)

On the behalf of my co-author, I would like to thank you for arranging chance to improve our manuscript. We appreciate the effort of the reviewers and editorial staffs of BMJ Open's readers.

Yours sincerely

Myeong Soo Lee, PhD

Revised points
1. We have now English proof-read our manuscript by American Journal Experts. We've added the certification as supplement not to review.
2. We have now added the sentence 'All authors read and approved the final version of the manuscript' at the ends of Author Contributions.
3. We have now changed the sentence as 'Limitations of this study include the potential incompleteness ...'.
4. We have changed the word 'evidence' to 'result' in page 4, line 4 from the bottoms and page 17, line 2 from the bottoms.
5. We have changed the key message to 'This systematic review offers limited evidence suggesting that tai chi may be effective for controlling pain and improving physical function in patients with OA in the knee.".