PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (see an example) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below. Some articles will have been accepted based in part or entirely on reviews undertaken for other BMJ Group journals. These will be reproduced where possible.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Mindfulness Online: An Evaluation of the Feasibility of a Web-Based Mindfulness Course for Stress, Anxiety & Depression
AUTHORS	Krusche, Adele; Cyhlarova, Eva; Williams, J Mark

VERSION 1 - REVIEW

REVIEWER	Elisabeth H Bos Postdoctoral researcher. Interdisciplinary Center Psychopathology and Emotion regulation. University Medical Center Groningen, University of Groningen, The Netherlands. No conflict of interest.
REVIEW RETURNED	03-Jul-2013

THE STUDY	The information provided about the participants is very limited. I do understand that no more information on baseline characteristics is available as this was an anonymous online course. However, the authors could have provided information on the number of eligible subjects (i.e. what proportion of the people following the course was included in the study?) and the number of dropouts (i.e. how many subjects did not complete the course and/or did not complete the end-of-treatment measurement and follow-up measurement?). The lack of this information makes it hard to judge the representativeness of the sample, and also makes it difficult to judge the value of the observed changes in the outcome measures, given the likelihood of selection and attrition bias. It is not clear how inclusion in the study was established; were all people asked to fill out the self-report questionnaires and was this mandatory to start with the training? How was this asked? Did the participants give their written informed consent? The description of the online mindfulness course may also be a bit more elaborate. For example, what is globally the content of the instructional videos and how long do they last? What do these 'interactive sessions' look like? Can participants aks online questions to the instructors or can they chat with each other? The 'statistical analysis' paragraph in the Methods section is lacking. Further, the repeated measures ANOVAs do not seem adequately described and/or performed: only one F-test is given, while two different results are extracted form this test, namely the significance of the change from before to after the course and the significance of the change from after the course to one-month follow-up. Further, it is unclear how differential change between practice groups was tested. I guess this should be done by modeling the interaction between group and time, but the authors do not provide this information. Also no information is given about how effects sizes

	were calculated. Most of the English is proper, but there are a number of typos (e.g. 'treatement') and a number of sloppy formulations (e.g. ' It may be that with a different, and larger sample size using this course, more practice does incur a greater reduction in negative mood').
RESULTS & CONCLUSIONS	Some of the values in Figure 1 are different from the values reported in the text. Further, the figures lack a legend which informs the reader about what the error bars in the figure are representing and what the different values in the figure represent. Also, the Y-axes of the figures are highly misleading, because the actual range of the variables is much larger than what is shown in the figure. Further, presenting the values in the figures with 1 instead of 3 decimals will do. Finally, in general there is a lot of overlap and duplication in the wriitng-up of the results. I think they can be presented much shorter.
REPORTING & ETHICS	This item may not be applicable since I doubt whether there is a reporting checklist for uncontrolled studies such as these. Nevertheless, a number of things that should be reported where lacking (see above). No information about consent is provided.
GENERAL COMMENTS	In principle, this could be an interesting study, since an online mindfulness course showing such high effect sizes may be very useful in clinical practice. The authors should be praised for developing such a course. I do, however, think that more information is needed to judge the value of this study properly. I have mentioned a number of issues already above. Other issues are: 1. The study appears to be highly similar to an earlier report from the authors. Large parts of the texts are even identical. The authors are not very clear in their manuscript as to how the second study relates to the first. Almost at the very end of the manuscript I discovered that ‘these are two different samples, recruited at different time points and with different advertising’. In the introduction and the methods section, the fact that there has been an earlier investigation is not mentioned very explicitly and neither is explained why it is necessary to publish two separate but highly similar reports on apparently the same topic. I sincerely doubt whether this is what is meant with ‘replication’. 2. Given the uncontrolled study design, the authors should be cautious with words like ‘effectiveness’ (e.g. Abstract) and phrases like ‘appears to be helpful’ (e.g. end of Discussion). Most often they do a proper job, speaking of ‘changes’ and so on, but be consistent in this regard. 3. It is not entirely clear what the authors mean by their second hypothesis: ‘that the decrease will be maintained at follow-up’. Do they mean that a similar amount of change as has been observed from pre- to post-treatment will also be observed from post-treatment to the one-month follow-up? Or do they mean that the participants will stay at this decreased post-treatment level in the month thereafter? Please be clearer. 4. Psychometric properties (reliability and validity) are given for the GAD scale, but not for the other scales. 5. It is suboptimal to group participants according to average practice level, as this parameter was measured on a continuous scale. Similarly, it is suboptimal to group participants according to

	the time to complete the course. The authors could have used the continuous measures, or they should explain why they constructed these groups. 6. Related to the above point: The authors do seem to find a relationship between practice time and outcome, but only if they use correlational analyses. For me it is not clear why they nevertheless state that practice and outcome are not related. 7. Lower practice was related to higher baseline scores. This suggests that a proper analysis would adjust for baseline severity (ANCOVA). 8. People who took more time to complete the course had less practice. However, their absolute amount of practice may have been higher, given the fact that they spread their practice over a longer time period. 9. Given the fact that the study is uncontrolled and highly similar to the previous report, and given the large overlap and duplications in the writing-up of the results, I think a brief report may

REVIEWER	Steven D. Hickman, Psy.D. Associate Clinical Professor UC San Diego Departments of Psychiatry and Family & Preventive Medicine Executive Director, UC San Diego Center for Mindfulness
REVIEW RETURNED	15-Jul-2013

GENERAL COMMENTS	It would be helpful to clarify that you asked people to practice EVERY DAY, as that was not clear in the Methods section.

REVIEWER	Professor Chris Williams University of Glasgow, UK Competing interests: I have developed online treatment supports for anxiety and depression, and am author of books in the same area. I hold shares and am a Director of a company that markets online treatment resources that use a CBT based content
REVIEW RETURNED	19-Jul-2013

THE STUDY	There is much to be commended in this paper. It is innovative, but in my view needs re-writing. With a significant re-write I think it could be published and would be a really useful addition to the scientific literature The key point is that this is an initial feasibility study- and although large simply does not answer the question of whether mindfulness online is effective. We know little about the sample - just that they wanted to use it, paid for access and gender etc. This is accepted and identified by the authors and there is little at this stage can be done to address that - it is okay though as this is a pragmatic recruitment. There is little on the process of recruitment/consent though the

	consent form is included They are less depressed and less anxious than typical clinical samples and probably more motivated (they paid for access). The study reports results in detail however because of the natural history of recovery/regression to the mean in anxiety and depression and the lack of a control group, the title, abstract and main findings need to be couched in terms of a feasibility study. As such it is a really good study- but the main outcomes will be in terms of can you recruit (yes), can you deliver treatment (yes again), can you retain/follow-up people (yes), what clinical effect may be present- and how this will then help inform the power calculation for a future larger study. The graphs and other results showing large drops in mood scores really unfortunately at this stage are only suggestive the treatment may be effective The finding that self-reported practice didn't predict mood outcome/anxiety is interesting and again suggests the need for a control group Overall, I think the paper would benefit from a rewrite and relabel in terms of a feasibility study
RESULTS & CONCLUSIONS	The intervention shows strong promise. A larger RCT should occur The title needs updating as it suggests that a definitive answer about effectiveness is available through this paper. It is not as we don't know enough about what would have happened to their scores if they received nothing (ie no control) The future study should follow-up longer e.g. 6 months rather than one month which is short- but fine for a feasibility study The future substantive study might consider using either usual care (described) or a mindfulness group (described) and this could be added to the discussion The results cannot currently show anything about whether the package is effective. I would sugegst a resubmission to instead report the really good take-up, use and collection of data, and encouraging drops in mood etc. It is all very suggestive that a large study with a control group would be potentially positive Please reference the manual that informed the content of the online course. Is permission in place to allow use of that content? What was the training/experience of the online video-trainers? Is it recognised by experts that the course represents MBCBT? The sample were less anxious and less depressed than IAPT (p10), but then change in anxiety and depression are stated to be greater than in IAPT (page 11). As they are starting at a lower level, and are a different sample from IAPT, the cause of this change just cannot be linked causally with the use of the package
REPORTING & ETHICS	I'm not sure I saw any mention of ethics and couldn't find any regsitration number from an ethics cttee. This is a non NHS population so NHSethics is not required. I note an Information sheet was used with a tick box to obtain consent. This seems less than the information needed usually from a University ethics committee and I note there is no mention of the

	study going through such a committee. The consent form states: You give your consent for us to store your personal information about your participation in the online course. Information you enter during the course may be aggregated and used for research purposes. We will not make any personally identifiable information about you, available to any other party unless we are required to do so by law This is probably adequate to allow the reporting of the data Conflicts are well addressed
GENERAL COMMENTS	A really interesting paper

VERSION 1 – AUTHOR RESPONSE

Reviewer:Elisabeth H Bos
Postdoctoral researcher. Interdisciplinary Center Psychopathology and Emotion regulation. University Medical Center Groningen, University of Groningen, The Netherlands.
No conflict of interest.

• The information provided about the participants is very limited. I do understand that no more information on baseline characteristics is available as this was an anonymous online course. However, the authors could have provided information on the number of eligible subjects (i.e. what proportion of the people following the course was included in the study?) and the number of dropouts (i.e. how many subjects did not complete the course and/or did not complete the end-of-treatment measurement and follow-up measurement?).

We do not have this information because the sample under investigation consisted of 'completers' alone, however, we have added that the number of 'completers' including completion of the follow-up questions, is 1497 of 5094 people who have started the course, from November 2010 to August 2013, on page 11. This information is limited, however, because it does not report how many people are still taking the course, in large part due to the functionality that people can leave the course for a time and come back to it later.

• The lack of this information makes it hard to judge the representativeness of the sample, and also makes it difficult to judge the value of the observed changes in the outcome measures, given the likelihood of selection and attrition bias.

Thank you for this feedback. Although we only examine completers in this study, we have included people who did not complete the course but still logged on to complete their follow-up ratings and practice and course completion time were not shown to affect the change in the decrease in outcomes. We have added text to our discussion on page 16 to clarify this.

• It is not clear how inclusion in the study was established; were all people asked to fill out the self-report questionnaires and was this mandatory to start with the training? How was this asked? Did the participants give their written informed consent?

Everyone who takes the online course is asked to complete the PSS, GAD-7 and PHQ-9 before and after the course and then requested to log back in at follow-up to complete them once more. Practice questions are also issued to everyone taking part in the online course. Participants are required to read the terms and conditions of participation and required to agree to these before participating. The terms and conditions include information about how their anonymous data may be used for research purposes. We have clarified this in the Method on page 8 and have included the terms and conditions as an appendix.

• The description of the online mindfulness course may also be a bit more elaborate. For example, what is globally the content of the instructional videos and how long do they last? What do these 'interactive sessions' look like? Can participants ask online questions to the instructors or can they chat with each other?
We have added some information to the Method on page 9. Participants did not have contact with a mindfulness teacher or with other participants. They were guided in the meditations using video clips.

• The 'statistical analysis' paragraph in the Methods section is lacking. Further, the repeated measures ANOVAs do not seem adequately described and/or performed: only one F-test is given, while two different results are extracted from this test, namely the significance of the change from before to after the course and the significance of the change from after the course to one-month follow-up. Further, it is unclear how differential change between practice groups was tested. I guess this should be done by modeling the interaction between group and time, but the authors do not provide this information. Also no information is given about how effects sizes were calculated.

The ANOVAs were conducted using the mean scores pre, post and follow-up and reported in the paper. Post hoc tests were run, and these have now been described in the text. There is information about how participants were separated and analysed on pages 10 and 13 whereby participants were split into high, medium and low groups. Partial correlations and regressions were also run. These show an effect for practice accounting for baseline severity and have been included in the paper. Effect size was calculated by hand and checked using an effect size calculator.
The formula for Cohen’s D was:

√(n1-1)SD12 + (n2-1)SD22/n1+n2-2 = Spooled Estimate
m1-m2/spooled estimate = d

Omega Squared was:
[k-1/nk(MSm-MSr)]/
MSr+(MSb-MSr)/k + [k-1/nk(MSm-MSr)] = ω2

• Most of the English is proper, but there are a number of typos (e.g. 'treatement') and a number of sloppy formulations (e.g. ' It may be that with a different, and larger sample size using this course, more practice does incur a greater reduction in negative mood').

Thank you for bringing these to our attention. We believe we have corrected any typos and rephrased the sentence on page 7.

• Some of the values in Figure 1 are different from the values reported in the text. Further, the figures lack a legend which informs the reader about what the error bars in the figure are representing and what the different values in the figure represent. Also, the Y-axes of the figures are highly misleading, because the actual range of the variables is much larger than what is shown in the figure. Further, presenting the values in the figures with 1 instead of 3 decimals will do. Finally, in general there is a lot of overlap and duplication in the writing-up of the results. I think they can be presented much shorter.

Thank you for bringing this to our attention.The figures have been updated. The error bars represent the confidence interval and this has been added to the figures. Although the range of scores is large, it is simpler to display the mean scores with the ranges used in the graphs, otherwise the graphs are large and do not display the data well. We have edited them so that it is clearer that they do not start from 0. It is not our intention to mislead. Although the mean scores are discussed and presented in figures, this is done only where other information is also presented in the text.

• No information about consent is provided.

As above, the terms and conditions were included as an appendix but are now also referred to in the text (p.8).

• In principle, this could be an interesting study, since an online mindfulness course showing such high effect sizes may be very useful in clinical practice. The authors should be praised for developing such a course. I do, however, think that more information is needed to judge the value of this study properly. I have mentioned a number of issues already above. Other issues are:

• The study appears to be highly similar to an earlier report from the authors. Large parts of the texts are even identical. The authors are not very clear in their manuscript as to how the second study relates to the first. Almost at the very end of the manuscript I discovered that 'these are two different samples, recruited at different time points and with different advertising'. In the introduction and the methods section, the fact that there has been an earlier investigation is not mentioned very explicitly and neither is explained why it is necessary to publish two separate but highly similar reports on apparently the same topic. I sincerely doubt whether this is what is meant with 'replication'.

We have added text about this in the key messages, abstract and introduction (p2, p6, p7)

• Given the uncontrolled study design, the authors should be cautious with words like 'effectiveness' (e.g. Abstract) and phrases like 'appears to be helpful' (e.g. end of Discussion). Most often they do a proper job, speaking of 'changes' and so on, but be consistent in this regard.

Thank you, we have amended some of our text accordingly.

• It is not entirely clear what the authors mean by their second hypothesis: 'that the decrease will be maintained at follow-up'. Do they mean that a similar amount of change as has been observed from pre- to post-treatment will also be observed from post-treatment to the one-month follow-up? Or do they mean that the participants will stay at this decreased post-treatment level in the month thereafter? Please be clearer.

That the decrease is maintained at follow-up means that the decrease will remain stable, i.e., that participants will not increase significantly in stress, anxiety and depression at follow-up. We have clarified this in the text on page 7.

• Psychometric properties (reliability and validity) are given for the GAD scale, but not for the other scales.

The details of the PSS, GAD-7 and PHQ-9 are presented on page 9. These details contain references to publications outlining the reliability and validity of each. We have clarified this in the text.

• It is suboptimal to group participants according to average practice level, as this parameter was measured on a continuous scale. Similarly, it is suboptimal to group participants according to the time to complete the course. The authors could have used the continuous measures, or they should explain why they constructed these groups.

Thank you for your feedback. We ran regressions and ANCOVAs but did not find significant results; we have now described more of our analysis in the results section. Following your feedback we have now also run multiple regressions using the continuous variables, accounting for baseline severity and have found that, using the continuous variables as you suggested, the amount of practice does contribute to a significant change in outcome. We have amended our paper accordingly.

• Related to the above point: The authors do seem to find a relationship between practice time and outcome, but only if they use correlational analyses. For me it is not clear why they nevertheless state that practice and outcome are not related.

By this we mean that the amount of practice does not result in a larger reduction in outcome, we have amended this for clarification on page 13.

• Lower practice was related to higher baseline scores. This suggests that a proper analysis would adjust for baseline severity (ANCOVA).

Thank you for your suggestion. An ANCOVA was run but did not find a significant effect. We have added this to our results section.

• People who took more time to complete the course had less practice. However, their absolute amount of practice may have been higher, given the fact that they spread their practice over a longer time period.

Thank you for this comment. This is true and we have discussed that the answers relating to practice may not be representative because of this. We have clarified this in the text also in our discussion on page 15.

• Given the fact that the study is uncontrolled and highly similar to the previous report, and given the large overlap and duplications in the writing-up of the results, I think a brief report may

Reviewer: Steven D. Hickman, Psy.D.
Associate Clinical Professor
UC San Diego Departments of Psychiatry and Family & Preventive Medicine Executive Director, UC San Diego Center for Mindfulness

• It would be helpful to clarify that you asked people to practice EVERY DAY, as that was not clear in the Methods section.

Thank you, we have done this, page 8.

Reviewer: Professor Chris Williams
University of Glasgow, UK

Competing interests: I have developed online treatment supports for anxiety and depression, and am author of books in the same area. I hold shares and am a Director of a company that markets online treatment resources that use a CBT based content

There is much to be commended in this paper. It is innovative, but in my view needs re-writing. With a significant re-write I think it could be published and would be a really useful addition to the scientific literature

• The key point is that this is an initial feasibility study- and although large simply does not answer the question of whether mindfulness online is effective.

Thank you, we have amended our write-up accordingly and hope that the paper now reflects that the course is acceptable and can be feasibly used as an intervention with potential benefits.

• We know little about the sample - just that they wanted to use it, paid for access and gender etc. This is accepted and identified by the authors and there is little at this stage can be done to address that - it is okay though as this is a pragmatic recruitment.

Unfortunately, we do not have additional participant information but this is something we are intending to collect in future studies.

• There is little on the process of recruitment/consent though the consent form is included

We have clarified this on page 8.

• They are less depressed and less anxious than typical clinical samples and probably more motivated (they paid for access).

Thank you for this point. It is possible that the sample may be more motivated. We have discussed this on pages 15 & 16. In future, we are intending to examine the effectiveness of the course on samples recruited to participate in research as opposed to their seeking out the course themselves. It will be interesting to see if the results differ.

• The study reports results in detail however because of the natural history of recovery/regression to the mean in anxiety and depression and the lack of a control group, the title, abstract and main findings need to be couched in terms of a feasibility study. As such it is a really good study- but the main outcomes will be in terms of can you recruit (yes), can you deliver treatment (yes again), can you retain/follow-up people (yes), what clinical effect may be present- and how this will then help inform the power calculation for a future larger study. The graphs and other results showing large drops in mood scores really unfortunately at this stage are only suggestive the treatment may be effective

Thank you for the feedback. We have amended the paper accordingly and this hopefully now reflects your above points.

• The finding that self-reported practice didn't predict mood outcome/anxiety is interesting and again suggests the need for a control group

We plan to include a control group in future studies.

• Overall, I think the paper would benefit from a rewrite and relabel in terms of a feasibility study
This has been addressed in the revised paper.

• The intervention shows strong promise. A larger RCT should occur

This is something we are intending to do in future.

• The title needs updating as it suggests that a definitive answer about effectiveness is available through this paper. It is not as we don't know enough about what would have happened to their scores if they received nothing (ie no control)

Thank you, we have amended the title accordingly.

• The future study should follow-up longer e.g. 6 months rather than one month which is short- but fine for a feasibility study

This is something we are also planning to do. Thank you for your suggestion.

• The future substantive study might consider using either usual care (described) or a mindfulness group (described) and this could be added to the discussion

Thank you, we will consider this for future studies. We have added this to our discussion on page 17.

• The results cannot currently show anything about whether the package is effective. I would suggest a resubmission to instead report the really good take-up, use and collection of data, and encouraging drops in mood etc. It is all very suggestive that a large study with a control group would be potentially positive

We have revised the paper accordingly and we believe it reflect this point adequately now.

• Please reference the manual that informed the content of the online course. Is permission in place to allow use of that content?

There is no specific manual in place for the online course, however, Mark Williams and others (creators of MBCT and renowned mindfulness teachers) gave support and input in the programme creation.

• What was the training/experience of the online video-trainers? Is it recognised by experts that the course represents MBCBT?

The course instructors are leading UK mindfulness teachers as recognised by the mindfulness community and were hired because of their experience. Mark Williams co-developed MBCT, and checked this on-line course to ensure that it faithfully represented some of the core components of MBCT.

• The sample were less anxious and less depressed than IAPT (p10), but then change in anxiety and depression are stated to be greater than in IAPT (page 11). As they are starting at a lower level, and are a different sample from IAPT, the cause of this change just cannot be linked causally with the use of the package

Thank you, this is a valid point. We now clarify this in the text on page 13.

• I'm not sure I saw any mention of ethics and couldn't find any regsitration number from an ethics cttee. This is a non NHS population so NHSethics is not required.
I note an Information sheet was used with a tick box to obtain consent. This seems less than the information needed usually from a University ethics committee and I note there is no mention of the study going through such a committee.

We discussed what ethics application would be appropriate for this study with the University Research Department who deals with Ethics applications. They informed us that, so long as the data are anonymous, there was no need to go through a committee as this study represents an audit of the existing course.

• The consent form states: You give your consent for us to store your personal information about your participation in the online course. Information you enter during the course may be aggregated and used for research purposes. We will not make any personally identifiable information about you, available to any other party unless we are required to do so by law

• This is probably adequate to allow the reporting of the data

• Conflicts are well addressed

• A really interesting paper

Thank you for your very helpful feedback. We are glad you found the paper of interest.

VERSION 2 – REVIEW

REVIEWER	Elisabeth H Bos Postdoctoral researcher. Interdisciplinary Center Psychopathology and Emotion regulation. University Medical Center Groningen, University of Groningen, The Netherlands. No competing interests
REVIEW RETURNED	09-Sep-2013

GENERAL COMMENTS	Further issues: Key message 1 is not a key message but a whole story. It is still not entirely clear what the authors mean with their second hypothesis: 'that the decrease will be maintained at follow-up'. The authors have tried to make this more clear by adding the sentence: 'that the decrease will remain stable', but this is still not clear: stable with respect to what? With respect to the post-treatment level? The authors seem to test the pre- to follow-up difference; so this actually is about whether the decrease with respect to baseline is still significant at follow-up. The authors should make this more clear! Also, this does not justify phrases like 'and this further significantly decreases at follow-up', because then they should have tested the post- to follow-up difference. It is still not very clear that this study is an extension a the former study. Especially in Intro, 5th paragraph and 7th paragraph the writing-up is a bit misty in this regard. For example: the sentence "This study presents a follow-up study of an online mindfulness course examining change in perceived stress" could be rephrased into: "This study presents a follow-up of our earlier study on an online mindfulness course examining change in perceived stress [39]" The authors write: 'More practice may incur a greater reduction in negative mood in a different, larger sample'. But actually they mean: 'In a larger sample it is easier to find a significant effect…'

	Because I am an advocate of mindfulness myself, so I would have loved to be more positive about the manuscript. But I recognize some irritation coming up when reading a manuscript that a) does not tell much news, and b) does this in a lengthy and untidy way. In my first comments I suggested to rewrite the manuscript into a brief report. The authors did not do so. They merely seem to have taken a patchwork approach to revise the paper, which, to my opinion, has not been sufficient to answer all my questions. I still feel the scientific quality of the manuscript is too low to warrant publication.

REVIEWER	Professor Chris Williams University of Glasgow, UK Competing interests: I have developed online treatment supports for anxiety and depression, and am author of books in the same area. I hold shares and am a Director of a company that markets online treatment resources that use a CBT based content
REVIEW RETURNED	09-Sep-2013

GENERAL COMMENTS	I have one suggested change - that is under Key Messages bullet point three- to alter the text back to the original: The online mindfulness course can significantly reduce perceived stress, anxiety and depression and this further significantly decreases at one month follow-up. ie to read can not does.

VERSION 2 – AUTHOR RESPONSE

Reviewer Name Elisabeth H Bos

Further issues:
• Key message 1 is not a key message but a whole story.

Thank you for your feedback. We have amended our key message so that it is now shorter.

• It is still not entirely clear what the authors mean with their second hypothesis: 'that the decrease will be maintained at follow-up'. The authors have tried to make this more clear by adding the sentence: 'that the decrease will remain stable', but this is still not clear: stable with respect to what? With respect to the post-treatment level? The authors seem to test the pre- to follow-up difference; so this actually is about whether the decrease with respect to baseline is still significant at follow-up. The authors should make this more clear! Also, this does not justify phrases like 'and this further significantly decreases at follow-up', because then they should have tested the post- to follow-up difference.

Thank you for your comment. We have amended the phrasing of our hypothesis so that hopefully it is now clear what we mean. Our hypothesis was that the difference in outcome post-course would remain at a low level at follow-up, compared to the level of stress, anxiety or depression pre-course. Three Repeated Measures ANOVA were conducted; one for PSS, one for GAD-7 and one for PHQ-9. Each found that there was a significant decrease from pre to post-course. Post hoc tests (pairwise comparisons) also revealed that there was a further significant decrease from post to follow-up. We have amended the Results section to include further detail about our post hoc analysis and hope that this is now clear.

• It is still not very clear that this study is an extension a the former study. Especially in Intro, 5th paragraph and 7th paragraph the writing-up is a bit misty in this regard. For example: the sentence "This study presents a follow-up study of an online mindfulness course examining change in perceived stress" could be rephrased into: "This study presents a follow-up of our earlier study on an online mindfulness course examining change in perceived stress [39]"

We have amended this in the text as per your advice.

• The authors write: 'More practice may incur a greater reduction in negative mood in a different, larger sample'. But actually they mean: 'In a larger sample it is easier to find a significant effect…'

We have amended this also, thank you.

• Because I am an advocate of mindfulness myself, so I would have loved to be more positive about the manuscript. But I recognize some irritation coming up when reading a manuscript that a) does not tell much news, and b) does this in a lengthy and untidy way. In my first comments I suggested to rewrite the manuscript into a brief report. The authors did not do so. They merely seem to have taken a patchwork approach to revise the paper, which, to my opinion, has not been sufficient to answer all my questions. I still feel the scientific quality of the manuscript is too low to warrant publication.

We appreciate your feedback about the manuscript. We agree that this study represents a procedural replication of an earlier study so represents only a small incremental step forward in what we know about the delivery of a mindfulness intervention on-line.
It is always difficult to discern how small a step in research is worth disseminating. Replicating an effect with a larger sample in itself probably would not be sufficient, but we were concerned that relying only on a single measure (the Perceived Stress Scale) in our first study, was insufficiently robust to justify strong conclusions. This motivated us to use the measures of anxiety and depression that are now widely used across the UK in primary care practices and in the Increasing Access to Psychological Treatment (IAPT) initiative in England. The clear advantage of using such measures is the possibility of judging our effect size against good benchmarks.
Having said this, of course, this study can only be seen as preliminary for the reason we give in the article itself. Thank you again for providing us with your replies and helpful suggestions.
Reviewer Name Professor Chris Williams

• I have one suggested change - that is under Key Messages bullet point three- to alter the text back to the original: The online mindfulness course can significantly reduce perceived stress, anxiety and depression and this further significantly decreases at one month follow-up.

ie to read can not does.

Thank you for your feedback, we have amended the text.