PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Caregiver's readiness for change as a predictor of outcome and attendance in an intervention programme for children and adolescents with obesity: a secondary data analysis.
AUTHORS	Anderson, Yvonne; Dolan, Gerard; Wynter, Lisa; Treves, Katharine; Wouldes, Trecia; Grant, Cameron; Cave, Tami; Smiley, Anna; Derraik, José; Cutfield, Wayne; Hofman, Paul

VERSION 1 – REVIEW

REVIEWER	Emma Mead University of Nottingham, UK
REVIEW RETURNED	18-Apr-2018

GENERAL COMMENTS	I enjoyed reading this piece of work, and it highlighted to me that caregiver's readiness is probably just one small piece in the puzzle, and there are many other factors that are likely to influence change in weight status and attendance in the intervention. Hence, why the results from this study were not significant. However, I don't think this message was portrayed enough. I think there are a few improvements which could be made in the abstract. The first point is that you have a heading called "Primary and secondary outcomes measures" but only discuss one primary outcome of the trial and don't mention any secondary outcomes. Later in the results section you refer to 'secondary outcomes' but the reader has no idea what they are. Also, you are referring to primary outcomes of the trial but not primary outcomes of the review. I think there could be some confusion here. I know you are limited by the word count but I felt there needs to be a least a short description/few examples of what the secondary outcomes were. In addition, you provide full numerical data for the primary outcomes but only give a p-value for overall attendance which is one of your objectives - I believe full numerical results should be given here. You refer to Cronbach's alpha expecting the reader to know what this is and what it measures. I think it should be explained in further detail. Under 'strength and limitations of this study' I would advise splitting the bullet points into 'strengths' and 'limitations' as it wasn't always clear which one it is. I thought the background was very comprehensive. When you refer to parents/children not believing their own weight, I would reference the following study: Jones, A.R., Parkinson, K.N., Drewett, R.F., Hyland, R.M., Pearce, M.S. and Adamson, A.J., 2011. Parental perceptions of weight status in children: the Gateshead Millennium Study. International journal of obesity, 35(7), p.953.

	I don't think the abbreviations RFC and SOC are very helpful when reading the review. I would prefer if you spelt these out in all cases. Under methods>participants: when you refer to 'UK Cole Normative data' - isn't it usually referred to as the 'UK 1990 growth reference'? Also, under this section I felt that the last two sentences contradicted each other - you first say that the eligibility was to be pre-contemplative or above - then you say you set the bar low (below pre-contemplative level). Which one did you use? Also, when reading the description of the RCT on pg 9, I would have benefited from some further information on what exactly the 'intensive intervention' was. I know I can go and read the trial report but I felt it would be useful to reader to at least give a brief sentence explaining some of the components (i.e. were both diet and physical activity interventions included?) I found the 'RFC as a dichotomous measure' quite difficult to follow, and still not 100% sure I understand how you dichotomized this outcome. Results>reliability: as said above, I think you should explain the Cronbach's alpha values - are these values showing good reliability? Your subheading 'outcome' - I would replace with a description of what you are actually providing results for in this section. In addition, attendance is listed as the second objective in this review so I would provide the results for attendance after the SOC outcome. Table 2 - I think you need to summarize these results in the text, highlighting what is shown. I also think it would be useful to give the difference between the groups and the associated 95% CI, alongside the p value. Under 'Readiness for change as a continuous measure' - you have put 'data not shown' - could you not include these results in a supplementary file? Discussion - I enjoyed reading this section - but I have three comments. The first is in relation to the second paragraph. You say it's 'not surprising that the adult's stage of readiness to make lifestyle changes was not a good predictor of child/adolescent outcome' - but then say their acknowledgement of child obesity is a problem - but I thought this was taken into account in the 'family member's stage of change' - i.e. if they didn't perceive their child to be overweight then they would be in 'pre-contemplation' stage? I think in this section you do need to highlight the other contributing factors which are likely to also influence change in child/adolescent weight status and attendance. i.e. that adult's stage of readiness may be factor but there are likely to be many others factors influencing these outcomes. Secondly, you say on pg 19 that it is possible that the study was underpowered to detect an association between the two groups. Could you not perform a power calculation to see if this is true? Finally, you also say on pg 19 that age may have had an impact but you did not present results by age because adults accompanied both children and adolescents. I still think an analyses split by age

	would be interesting to see as even though children and adolescents had adults present at the intervention, the level of involvement and influence of the adult I think could be different in these age groups. For example, in young children, adults are more likely to be able to control their diet and encourage physical activity. However, an adolescent is probably more likely to be independent and to start making their own decisions in relation to their diet and physical activity. Hence, I would expect 'caregiver's readiness for change' would be more likely to influence a child's weight status, as opposed to an adolescent.

REVIEWER	Louise Cominato ICR-HC FMUSP
REVIEW RETURNED	13-Jun-2018

GENERAL COMMENTS	The authors should consider review the MAIN GOALS of the paper, regarding the objectives. I am fully concerned that two different approachs does not present the results properly. Also, the statistical analyses should be separately performed, embroading children bellow 11y old and above this age.

VERSION 1 – AUTHOR RESPONSE

REVIEWER #1

Comments to the Author
I enjoyed reading this piece of work, and it highlighted to me that caregiver's readiness is probably just one small piece in the puzzle, and there are many other factors that are likely to influence change in weight status and attendance in the intervention. Hence, why the results from this study were not significant. However, I don't think this message was portrayed enough.

Reply: We thank the Reviewer for the insightful comments, which we believe have improved the quality of this manuscript.

We agree that we could have emphasised that caregiver readiness is one factor in a complex multitude of factors, so have subsequently amended the discussion, which now reads:

“Indeed, caregiver readiness is one factor in a complex multitude of factors predicting success in achieving reductions in weight status, such as perception of child weight status, and recognition of weight as a problem.[11, 12] Environmental factors, such as access to transport to sessions, food security, and availability of a caregiver to attend sessions also will affect outcome. Such factors are likely to be why the results from this study were not significant.”

I think there are a few improvements which could be made in the abstract. The first point is that you have a heading called "Primary and secondary outcomes measures" but only discuss one primary outcome of the trial and don't mention any secondary outcomes.

Reply: We have subsequently changed this to outline some secondary outcome measures.

Later in the results section you refer to 'secondary outcomes' but the reader has no idea what they are.

Reply: We have amended the abstract as above, so hope this now resolves this issue.

Also, you are referring to primary outcomes of the trial but not primary outcomes of the review. I think there could be some confusion here. I know you are limited by the word count but I felt there needs to be a least a short description/few examples of what the secondary outcomes were.

Reply: The abstract now includes examples of secondary outcomes, and we have also included a summary of secondary outcomes in the objectives section of the introduction:

"…and/or the secondary outcomes in the Whānau Pakari 12-month intervention (such as waist circumference, number of breakfasts eaten, servings of fruit and vegetables, sweet drink consumption, 550-m walk/run time, steps per day, indices on quality of life and behaviour checklists, and biochemical markers)"

In addition, you provide full numerical data for the primary outcomes but only give a p-value for overall attendance which is one of your objectives - I believe full numerical results should be given here.

Reply: The Reviewer makes a valid comment, and we have since added these data into the Abstract and Results section.

You refer to Cronbach's alpha expecting the reader to know what this is and what it measures. I think it should be explained in further detail.

Reply: The Reviewer highlights a valid point, and we have subsequently removed reference to Cronbach's alpha in the abstract. We have however, added an amendment in the methods, which now reads:
"Cronbach's alpha (a numerical measure of internal consistency) was used…"

Under 'strength and limitations of this study' I would advise splitting the bullet points into 'strengths' and 'limitations' as it wasn't always clear which one it is.

Reply: This section is now amended, and should be easier to read.

I thought the background was very comprehensive. When you refer to parents/children not believing their own weight, I would reference the following study: Jones, A.R., Parkinson, K.N., Drewett, R.F., Hyland, R.M., Pearce, M.S. and Adamson, A.J., 2011. Parental perceptions of weight status in children: the Gateshead Millennium Study. International journal of obesity, 35(7), p.953.

Reply: This has been included in the introduction, which now reads:

"Parental perception of their child's weight status is also an important consideration; parents' ability to identify when their child was overweight has been found to be limited."

I don't think the abbreviations RFC and SOC are very helpful when reading the review. I would prefer if you spelt these out in all cases.

Reply: These have been amended throughout the manuscript.

Under methods: participants. When you refer to 'UK Cole Normative data' - isn't it usually referred to as the 'UK 1990 growth reference'?

Reply: We have altered to UK 1990 growth reference data.

Also, under this section I felt that the last two sentences contradicted each other - you first say that

the eligibility was to be pre-contemplative or above - then you say you set the bar low (below pre-contemplative level). Which one did you use?

Reply: We have altered the section for clarification:

“We purposely set the bar low for readiness to change (i.e. below the pre-contemplative level) to assess whether degree of readiness for change predicts outcome;[17] therefore, only those classed as not ready for change on stage of change assessment were excluded from this study.”

Also, when reading the description of the RCT on pg 9, I would have benefited from some further information on what exactly the 'intensive intervention' was. I know I can go and read the trial report but I felt it would be useful to reader to at least give a brief sentence explaining some of the components (i.e. were both diet and physical activity interventions included?)

Reply: We have subsequently amended the methods, which now read:

“In brief, the RCT compared a 12-month intensive intervention with home-based comprehensive assessments (medical, dietary, physical activity and psychology screening) and weekly activity sessions (group sessions for 12 months, including physical activity, nutrition and psychology content) with a minimal intensity control with home-based assessments only, including 6-monthly follow-up, conducted in Taranaki, NZ. For the purposes of this study, given we were interested in stage of change in relation to outcome…”

I found the 'RFC as a dichotomous measure' quite difficult to follow, and still not 100% sure I understand how you dichotomized this outcome.

Reply: We have subsequently edited this section, and simplified it, so that it now reads:

“The readiness for change questionnaire was based on Rollnick et al.’s original readiness to change questionnaire,[3] which we modified to focus on beliefs around weight, eating habits, and physical activity levels. A 5-point Likert scale was used. Given the complexity of obesity, additional questions were added to the original questionnaire, resulting in a 21-item child/adolescent questionnaire and a 27-item questionnaire for the family member, with 6 extra questions related to attitudes/behaviour of the wider family unit. The questionnaire was tested for understanding and comprehension in a randomly selected cohort of clinic patients prior to trial commencement, who were underweight, normal weight, and overweight. This pilot testing found the questionnaire was acceptable for use (i.e. underweight children were scored pre-contemplative).

Questions were reverse keyed in their language to negate the need to reverse the pre-contemplative scaled score when comparing the three scores for each stage of change with each other. Scoring was undertaken, which calculated the sum totals for each stage of change (pre-contemplation, contemplation or preparation/action). This was divided by the number of questions asked to obtain an adjusted score for each stage of change. The highest adjusted score was designated as the stage of change of the child/adolescent or family member.”

Results: reliability. As said above, I think you should explain the Cronbach's alpha values - are these values showing good reliability?

Reply: The Cronbach’s alpha as used here was only to give an indication of the internal consistency of our measure, not to provide a validation of the measure. A number of researchers have suggested a Cronbach’s alpha of >0.70 is required to establish validity. However, there remains a good deal of controversy as to whether Cronbach’s alpha is the best statistic to use and how it is used in research

(Taber, 2016, doi: 10.1007/s11165-016-9602-2). For instance, in test development or construction many other statistics or methods should be employed, however, we are not attempting in this paper to develop a new measure of RFC, only to gain some insight as to whether an adapted version of a standardised measure that has been used to measure readiness for change in a number of populations where lifestyle change is required or recommended was useful in determining if the stage of readiness for change was subsequently linked to adherence to treatment and treatment outcomes.

Your subheading 'outcome' - I would replace with a description of what you are actually providing results for in this section.

Reply: This is now called ‘Quantitative stage of change and outcome measures’.

In addition, attendance is listed as the second objective in this review so I would provide the results for attendance after the SOC outcome.

Reply: We agree with the Reviewer that this makes sense, so we have moved the section under the heading "Attendance" and placed it after the section now entitled "Age of child". Please note that we have also added more details on the stratified analysis comparing stage of change with attendance levels.

Table 2 - I think you need to summarize these results in the text, highlighting what is shown.

Reply: We apologise for this oversight as this should have indeed been done. We have since expanded the first paragraph under the heading "Quantitative stage of change and outcome measures", which now reads as:
"Of the 96 participants, 68 had attendance data and assessment data at 12 months.
Table 2 shows the stratified association between quantitative stage of change (caregiver) at baseline assessment and outcome at 12 months.

There were no differences in BMI SDS change from baseline between groups according to caregiver's stage of change (p=0.27; Table 2). Among secondary outcomes, family members in the stage of preparation/action spent 16.2 minutes more on moderate-intensity to very vigorous physical activity per day compared to those in pre-contemplation/contemplation (p=0.03; Table 2). There were no other differences in secondary outcomes (Table 2)."

I also think it would be useful to give the difference between the groups and the associated 95% CI, alongside the p value.

Reply: The Reviewer makes a fair request, and we have since added these data into Table 2.

Under 'Readiness for change as a continuous measure' - you have put 'data not shown' - could you not include these results in a supplementary file?

Reply: After re-examining our data, we realised that in hindsight this section should not have been included. The distribution of state of change scores is such that the vast majority of data points are in just two levels. Thus, analyses examining readiness for change as a continuous measure would not be robust, and the associations are more reliably captured by the stratified analyses. We apologise for this oversight, but in light of the above we have removed the respective paragraph from the manuscript.

Discussion - I enjoyed reading this section - but I have three comments. The first is in relation to the second paragraph. You say it's 'not surprising that the adult's stage of readiness to make lifestyle

changes was not a good predictor of child/adolescent outcome' - but then say their acknowledgement of child obesity is a problem - but I thought this was taken into account in the 'family member's stage of change' - i.e. if they didn't perceive their child to be overweight then they would be in 'pre-contemplation' stage?

Reply: We believe these are two separate constructs; stage of change may not necessarily reflect parental perceptions of child weight status. A parent perceiving their child is overweight doesn't necessarily mean they are ready to make changes; one doesn't necessarily lead to the other.

I think in this section you do need to highlight the other contributing factors which are likely to also influence change in child/adolescent weight status and attendance. i.e. that adult's stage of readiness may be factor but there are likely to be many others factors influencing these outcomes.

Reply: We have amended this section, which now reads:

"It was not surprising that caregiver's stage of readiness to make lifestyle changes was not a good predictor of child/adolescent outcome. Indeed, caregiver readiness is one factor in a complex multitude of factors predicting success in achieving reductions in weight status, such as perception of child weight status, and recognition of weight as a problem. [11, 12] Environmental factors, such as access to transport to sessions, food security, and availability of a caregiver to attend sessions also will affect outcome. Such factors are likely to be why the results from this study were not significant."

Secondly, you say on pg 19 that it is possible that the study was underpowered to detect an association between the two groups. Could you not perform a power calculation to see if this is true?

Reply: We have now included the below in the methods, which reads:
"Based on the changes from baseline observed at 12 months in our study population, and with n of 32 and 36 in each group, our study was powered to detect statistically significant differences in change from baseline in BMI of ±0.21 SDS, in waist circumference of ±3.5 cm, and in parent's total generic scaled score of ±11.1, with α=0.05 and 80% power."

Finally, you also say on pg 19 that age may have had an impact but you did not present results by age because adults accompanied both children and adolescents. I still think an analyses split by age would be interesting to see as even though children and adolescents had adults present at the intervention, the level of involvement and influence of the adult I think could be different in these age groups. For example, in young children, adults are more likely to be able to control their diet and encourage physical activity. However, an adolescent is probably more likely to be independent and to start making their own decisions in relation to their diet and physical activity. Hence, I would expect 'caregiver's readiness for change' would be more likely to influence a child's weight status, as opposed to an adolescent.

Reply: Thank-you for these comments, which we believe are absolutely correct. As per comments for Reviewer #2, we have subsequently included these data in Supplementary tables 1 and 2, and have included in results a section "Age of child", which is included in comments for Reviewer #2.

REVIEWER #2

Comments to the Author
The authors should consider review the MAIN GOALS of the paper, regarding the objectives. I am fully concerned that two different approaches do not present the results properly.

Reply: We are grateful to the Reviewer for their comments. We have addressed Reviewer one's

comments in detail, and hope that the paper is clearer. We have also changed wording to aims of study in the introduction rather than purpose.

In terms of the two different approaches, as per Reviewer #1, we have decided to remove readiness for change as a continuous variable from the paper, focussing on analysis as a dichotomous variable.

Also, the statistical analyses should be separately performed, including children below 11y old and above this age.

Reply: The Reviewer makes a valid comment, and we have since run these statistical analyses, examining the associations between family member's state of change and study outcomes within each age group. The results are mostly unchanged, except that there was a between-group difference in the <11-year group for actual moderate to very vigorous activity (p=0.02). We have consequently added a new paragraph into the Results, and we provide the respective results in Supplementary Tables 1 and 2. This now reads:

“Age of child
There was no association between changes at 12 months from baseline among children based on age, and caregiver’s readiness for change (Supplementary Tables 1 and 2), apart from a between-group difference in the <11-year group for actual moderate to very vigorous activity (p=0.02).”

VERSION 2 – REVIEW

REVIEWER	louise cominato Instituto da Criança- Hospital das clinicas fmusp
REVIEW RETURNED	24-Oct-2018

GENERAL COMMENTS	Congratulations on the job. The summary is unclear.