PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Service evaluation of the GOALS family-based childhood obesity treatment intervention during the first three years of implementation
AUTHORS	Watson, Paula; Dugdill, Lindsey; Pickering, Katie; Owen, Stephanie; Hargeaves, Jackie; Staniford, Leanne; Murphy, Rebecca; Knowles, Zoe; Cable, Tim

VERSION 1 - REVIEW

REVIEWER	Helen Elizabeth Brown CEDAR/MRC Epidemiology Unit University of Cambridge, UK
REVIEW RETURNED	19-Sep-2014

GENERAL COMMENTS	This manuscript cites two objectives (1) evaluate the effectiveness of the intervention, and (2) provide an exemplar for intervention reporting. Whilst (2) is achieved with the inclusion of the TIDieR checklist and additional modification details, the lack of control group limits our ability to interpret results with any confidence (thus failing to achieve objective (1)). General comments This innovative paper uses the TIDieR checklist to report on a family-based childhood obesity intervention, providing effect and some process evaluation for the first three years of implementation. It is great to see such rigour in reporting of interventions, and this manuscript may serve as an exemplar for future studies (particularly given the very useful open dialogue around study modifications). The lack of control group is an obvious weakness of this study; and though service requirements are cited as rationale for this, authors do not comment on the option of using, for example, a wait—list control design. Significant expansion here is required. Authors acknowledge the high attrition rate and somewhat low representativeness of the study sample. Results are clear and well-articulated; children completing GOALS demonstrated modest (but impressively, maintained at 12-month follow-up) improvements in BMI z-score. Small improvements in perceived social acceptance (most marked in those with the highest baseline BMI z-score) were also reported. Parents suggested positive changes to family PA and diet as a result of the intervention. The tables and figures, particularly those pertaining to the TIDieR checklist and study modifications, are clear and add substantially to this manuscript. The Discussion was excellent, providing interesting insights to the data reported, and comparing results with similar or relevant studies. With major adjustments to address objective 1 (i.e. further comment on the lack of control group), this study would be an excellent addition to BMJ Open, and may be of particular interest to those developing targeted interventions.

	Specific edits 1. Some attention should be paid to grammar and sentence structure throughout the manuscript (in particular, language is sometimes a little too colloquial). 2. P5 L20 – It would be useful to cite some evidence around obesity as a risk factor in children (the references here refer to adult samples). Equally, some acknowledgment of current obesity levels in UK children is needed. 3. P5 L35 – Could you please describe how the study was "unique in its whole family approach", particularly given that only one adult carer and one child were required for inclusion? 4. P5 L40 – I'd like to see some acknowledgement of other interventions that have featured weekly family PA sessions (for example, it would be worth checking the protocol of Healthy Dads, Healthy Kids (Morgan et al., 2011) for similarities). 5. P6 L3 – I'm unsure about the strength of rationale here. Perhaps add some consideration about self-esteem as a mediator for, or a result of, weight change? 6. P6 L39 – Minor point; the reference here is incorrectly formatted. Please provide the full citation. 7. P10 L29 – I understand the need to limit inclusion of only one child per family in the analysis – however, this is a shame; exploration of sibling support effect would be very interesting. 8. P10 L42 – Authors report that 85% of children were White-British, but do not provide information on the ethnicity of the remaining 15%. Please include these details, and comment on the representativeness of this sample compared with the wider Liverpool and UK population. 9. P11 L3 – It would be useful to provide some rationale for the removal of the one outlier (BMI z-score change -0.71); what motivated this decision? 10. P12 L22 – My apologies if I have missed this elsewhere in the manuscript, but there seems to be no discussion of confounders here – and subsequently, no adjustment in the analysis. Were confounding variables considered, and if so, how were they dealt with? 11. P12 L51 – If possible, please operationalise the BMI z-score change from baseline to post-intervention. What magnitude of weight loss might this relate to in 'real' terms? 12. P14 L12 - This paragraph ("Correlations between BMI z-score and self-perceptions") is extremely interesting. However, the language is somewhat convoluted and difficult to follow. Could you please re-phrase for clarity? 13. P15 L15 – It would be useful to cite some evidence for the validity of proxy-reported child PA levels and inclusion of parent's change perceptions. 14. P19 L40 – The intervention is described as being based on the Social Cognitive Framework here – and yet, this has not been mentioned previously. I'd like to see some discussion of the rationale for use, and operationalization of this theory in the methods. 15. P19 L48 – Do you have any information on sibling/other carer attendance at family PA sessions? Given the assertion that a strength of this study was its whole family approach, it would be good to examine how successful you were in engaging with wider members of the family. 16. P21 L7 – The manuscript would benefit some discussion of how the acknowledged levels of attrition could be reduced in future studies.

REVIEWER	Tania Griffin University of Birmingham, UK
REVIEW RETURNED	16-Oct-2014

GENERAL COMMENTS	The study objective is clearly defined but the paper would benefit from refining this to focus on the results of GOALS Confounding factors are not considered in the statistical methods Results could be made more structured to assist in clarity Dear authors, The GOALS intervention seems to be an extensively developed programme, in particular the challenges faced and methods for addressing and or resolving these challenges. The paper offers an interesting insight into the experiences of the GOALS programme and offers a useful addition to the literature. However, I suggest some amendments and revisions to make the paper flow better and the key messages need to be made clearer. Currently the manuscript seems a bit mixed as to whether it is a protocol paper or a results paper. I think the objective should be solely to report the results and findings of GOALS. The reporting of it using the TIDieR is useful and a valuable addition to the paper, and it can be emphasised in the introduction, methods and discussion the importance of clear reporting of childhood obesity interventions using this framework. However, I feel that the paper would be easier to follow if the main objective was to report the findings of GOALS. The use of the TIDieR is a method not a result. **<u>Abstract</u>** Line 9. Suggest removing reporting of GOALS using the TIDierR checklist from the objectives Line 24. Participants. Remove (/43) Line 25. Replace parents with Parents/carers Line 31. 18x 2-hour weekly group sessions – remove the x after 18 and replace 2 with two. (i.e. 18 two-hour sessions). How long does GOALS run for? Approx 6 months? Put this in the intervention section not line 38

	Article summary Line 28. Add '6 months' to define 'post intervention' **Key messages:** Line 37: The term 'promising' is ambiguous and offers little meaning in the sentence Include in this section BMI-z-score changes. Line 48: Was it a key finding of your study that the TIDieR checklist is a mechanism for clear reporting? I would expect this was reported in the original TIDieR paper and hence why the checklist exists. Perhaps rephrase this bullet point. Introduction It would be useful to state in the introduction that GOALS was based on SCT Line 20: reference 1 needs to be in square brackets. I think the risks for childhood obesity could be expanded a little here – not overly so as many people reading the paper will be familiar with the risk factors. However, as it reads at the moment there is no information on why childhood obesity is a problem. **Page 6.** Lines 8-13. It is difficult to understand what the meaning of this sentence is. Line 34. 'the checklist covers….modifications made during the study period' Your checklist doesn't seem to present modifications made. I see this is covered in supplementary information. This is very useful especially for researchers looking to learn from your experiences to assist in developing their own programmes. Is there any way this can be combined to all be presented in table 1? Lines 41-60. The paragraph and bullet points repeat each other.

	The use of the TIDieR checklist should come before the statement that the paper is reporting results of the intervention. **Page 7** This section needs to be titled Methods. The methods section is a bit unclear and would benefit from re-ordering. If the paper is to describe the results of this study (the effect of the GOALS intervention) a more logical order would be: Participants Intervention (GOALS) Outcome measures Analysis Line 9: add United Kingdom after Liverpool John Moores University Lines 18 & 22: Replace %ile with the term 'percentile' Line 38: optional addition 'the way **in which** the intervention was delivered' **Page 9** Line 12 'first author' it would be good to use author initials here Lines 11-20 (who provided). I think I know what you mean but the first line says delivered by the research team, and then later you say you use sessional staff. Just ensure it's clear whether you used a combination of people for delivery. **Page 10** Lines 6: Evaluation methods. I suggest omitting this section. Line 24: invited to take part in **this** study? (check where else this occurs in the manuscript, 'this study' makes it more specific than 'the study') Line 35: …had complete baseline and **post intervention (6 months**) BMI data Lines 38 onwards – Participant flow through study – Report this whole section as Results. Lines 42-47. These are participant characteristics not 'flow through the study'

	Lines 44. BMI-z-score. These aren't very meaningful, could you report x% were obese, x % overweight? Ethnicity data – 85% were White British – what were the other ethnic groups? Line 45: remove /143 Line 46-47. The numbers as percentages aren't very useful, leave these as whole numbers. Line 49. Use the phrase parent/carer throughout manuscript rather than abbreviating to parent. **Page 11:** Line 23: Measures – rephrase to Outcome measures BMI Lines 25-35 (BMI) - who measured the children? Trained researchers? Their parents? - was only one measure taken or two/three taken to ensure accuracy? Did you use a computer programme to convert to z scores? LMS method? Child self perceptions Line 41: Why italics for SPPC **Page 12** Changes in PA and Diet Why did you not use a combination of qualitative and quantitative questions / a validated PA questionnaire and diet measure. Please justify the approach used. How did you collect information on ethnicity? Did you collect information on deprivation status? Were the session staff observed and consistency in delivery checked across staff/centres? Did you assess anywhere the fidelity of implementation? **Analysis** SPSS v17 (date and reference?)

	Line 28-30 Analysed deductively – what does this mean? Was it possible to control for any confounders in your statistics? Ethnicity? Gender? Age of children? Weight status of parents? Which GOALS session they were taking part in? If you don't have individual participant postcode for deprivation can you use postcode of the centre where GOALS intervention was delivered? **Results** Can a table be included to present Participant characteristics? Table 2: 'complete with follow up' can this term be 'complete with 12 month follow up' Correlations between BMI z-score and self perceptions Lines 15-22. Measuring correlations was not measured in your analysis section. **Parent BMI** Could this be title be: Change in Parent BMI? **Parent reported changed in family PA and diet** Line 55: I do not understand what you mean by family pairs The suggestion is this section has been analysed qualitatively with the discussion of 'themes' but there is use of numbers throughout. e.g. '41 parents, 34 of whom felt their activity level had improved'. Providing a summary paragraph with the key findings and then tabulating the quotes may be a better way of presenting the results. This section is very long at the moment and would benefit from being made shorter and more concise. Line 54: should this be '…tired **or** struggling'? **Discussion**

	Page 18 Lines 17-19. First sentence is unnecessary Line 22: you say the findings are consistent with other feasibility studies – was GOALS a feasibility study? **Page 19** Line 11: First line is awkwardly worded; the term directionality should be rephrased **Figure 1** 'Excluded: non-referred overweight siblings n=17' why are these included in eligible participants? Defining completed as still attending at end of intervention – how many sessions did they have to attend to be classed as complete? What if they only went to 25% of the sessions, but came to the last one, was this classed as complete? Child excluded for large BMIz-score change – could this have been measurement error? **Supplementary Online resource 1** When describing school year - eg Year 1, Year 2, Year should be capital Y Replace %ile with percentile Year 1 interventions were delivered in primary schools but access to cooking facilities was limited – does this mean that a different intervention was delivered which didn't include cooking element?

VERSION 1 – AUTHOR RESPONSE

Response to reviewer comments

General comments

Reviewer comments	Authors' response
Editor	Taking into account the combined reviewer

Please tone down and shorten the information on the Tidier checklist (especially regarding 'setting a standard for improved reporting'), as the editors found this to be slightly presumptive and unnecessary	comments, we have re-shaped the article to focus on the impact evaluation of GOALS during the first three years of implementation. In doing so, we have toned down the information on the TIDieR checklist, such that it is a valuable addition rather than the main objective of the paper. For brevity we have also deleted the sentence from the discussion that stated we had followed the TREND guidelines (page 20, lines 9-12 in the original version), since our use of this checklist is evidenced through submission of the TREND checklist, and is not central to the article.
Reviewer 1 This manuscript cites two objectives (1) evaluate the effectiveness of the intervention, and (2) provide an exemplar for intervention reporting. Whilst (2) is achieved with the inclusion of the TIDieR checklist and additional modification details, the lack of control group limits our ability to interpret results with any confidence (thus failing to achieve objective (1)). The lack of control group is an obvious weakness of this study; and though service requirements are cited as rationale for this, authors do not comment on the option of using, for example, a wait—list control design. Significant expansion here is required.	We wish to clarify that this is an *impact evaluation* within a service delivery setting, therefore we are not attempting to evaluate *the public health effectiveness* of the GOALS intervention. Sometimes experimental methods are not viable in real-world service delivery settings (see [1]) therefore alternative research designs must be employed to evaluate interventions. This article provides an example of how a rigorous mixed-methods evaluation can be conducted within the practical constraints of service delivery, providing important information for the translation of evidence to practice. We have already published an extensive discussion on the methodological constraints we faced in meeting the needs of both academia and service delivery, including our reasons for not being able to include a wait-list control (see [2]). For the sake of brevity in our first draft we referred readers to this 2013 article. At the request of reviewer 1 however, we have expanded our discussion in the current article to make clear why a control group was not viable (see limitations section).
Reviewer 2 Currently the manuscript seems a bit mixed as to whether it is a protocol paper or a results paper. I think the objective should be solely to report the results and findings of GOALS. The reporting of it using the TIDieR is useful and a valuable addition to the paper, and it can be emphasised in the introduction, methods and discussion the importance of clear reporting of childhood obesity interventions using this framework. However, I feel that the paper would be easier to follow if the main objective was to report the findings of GOALS. The use of the TIDieR is a method not a result.	

Specific edits

Editor

Reviewer comments	Authors' response
Please include the study design in your title	Added

Reviewer 1

Reviewer comments	Authors' response
Some attention should be paid to grammar and sentence structure throughout the manuscript (in particular, language is sometimes a little too colloquial)	We have paid careful attention to sentence structure to ensure meaning is clear throughout and have corrected colloquial style.
P5 L20 – It would be useful to cite some evidence around obesity as a risk factor in children (the references here refer to adult samples). Equally, some acknowledgment of current obesity levels in UK children is needed.	We have added information about the current obesity levels in children and associated health risks.
P5 L35 – Could you please describe how the study was "unique in its whole family approach", particularly given that only one adult carer and one child were required for inclusion? P5 L40 – I'd like to see some acknowledgement of other interventions that have featured weekly family PA sessions (for example, it would be worth checking the protocol of Healthy Dads, Healthy Kids (Morgan et al., 2011) for similarities).	We have acknowledged the success of other interventions that include parent-child PA sessions, whilst highlighting the absence of such inclusive PA sessions within childhood obesity treatment. We have moved the discussion of GOALS to the final paragraph of the introduction (to reduce repetition) and amended the wording to make the family element clearer. We have removed the word "unique" from the abstract also.
P6 L3 – I'm unsure about the strength of rationale here. Perhaps add some consideration about self-esteem as a mediator for, or a result of, weight change?	We have added some clearer rationale for investigating the relationship between self-esteem and weight change.
P6 L39 – Minor point; the reference here is incorrectly formatted. Please provide the full citation.	MRC reference citation corrected in list.
P10 L29 – I understand the need to limit inclusion of only one child per family in the analysis – however, this is a shame; exploration of sibling support effect would be very interesting.	We agree this is of interest. Unfortunately however, the number of siblings with complete data was very small (n=8), therefore we cannot perform a meaningful analysis of the sibling support effect. However, this data is discussed in Paula Watson's PhD [3] and it would be possible to share with any interested readers.

Reviewer comments	Authors' response
	We have amended the data sharing statement to make clear this data is available by contacting Paula.
P10 L42 – Authors report that 85% of children were White-British, but do not provide information on the ethnicity of the remaining 15%. Please include these details, and comment on the representativeness of this sample compared with the wider Liverpool and UK population.	Information added.
P11 L3 – It would be useful to provide some rationale for the removal of the one outlier (BMI z-score change -0.71); what motivated this decision?	Whilst it is likely the BMI z-score change observed in this child was a true reflection of his weight loss rather than measurement error (this was apparent through visual inspection), a BMI z-score change of -0.71 is not typical of the children who have completed GOALS (both during this study period and since). Therefore we felt that inclusion of this data would provide a misleading picture through escalating the mean change beyond that which was true of the wider GOALS population (reflected by the fact the BMI z-score change was over 3 standard deviations from the mean).
P12 L22 – My apologies if I have missed this elsewhere in the manuscript, but there seems to be no discussion of confounders here – and subsequently, no adjustment in the analysis. Were confounding variables considered, and if so, how were they dealt with?	We have added an explanation to show how the potential non-independence of children (within intervention cohorts) was addressed. In terms of other potential confounders: *Child age* - the conversion to BMI z-score accounts for changes that would have occurred over time with children becoming older, therefore no further adjustment is necessary. *Child physical activity and diet* – these are the mechanisms through which GOALS was aiming to change children's BMI z-score, therefore any changes in PA and/or diet would be mediators rather than confounders. We do not have sufficient data to report pre- and post- physical activity and diet. We do not feel an explanation of the above factors is necessary within the text, since our procedures are aligned with other repeated

Reviewer comments	Authors' response
	measures childhood obesity treatment intervention research published in BMJ Journals (e.g. [4,5]). We acknowledge the only way we could entirely account for confounding factors would be to include a control group which was not viable (as discussed above).
P12 L51 – If possible, please operationalise the BMI z-score change from baseline to post-intervention. What magnitude of weight loss might this relate to in 'real' terms?	BMI z-score is the recommended outcome measure for childhood obesity treatment interventions (see [6]) and is well understood within medical and academic communities. Unfortunately, it is not possible to operationalise the BMI z-score change in absolute terms, since the amount of weight change will vary depending on the child's age, gender and starting BMI z-score. A couple of examples may help explain this: - A boy of 6.9 years with a starting BMI z-score of 3.7 increased his BMI from pre- to post-intervention (25.4 to 25.7). But because this is a time when other children his age are increasing BMI rapidly, his BMI z-score *decreased* (-0.11). - A boy of 15.6 years with a starting BMI z-score of 3.4 decreased his BMI from pre- to post-intervention (38.1 to 37.7). But because this is a time when BMI is only increasing gradually in other children his age, his BMI z-score decreased by less than the younger boy (-0.06). Therefore any attempt to operationalise a mean change of -0.07 amongst a sample of children aged 4-16 years would require a lengthy discussion that is beyond the scope of this article.
P14 L12 - This paragraph ("Correlations between BMI z-score and self-perceptions") is extremely interesting. However, the language is somewhat convoluted and difficult to follow. Could you please re-phrase for clarity?	This paragraph describes some complex relationships, and careful attention was paid to ensure the results are presented accurately and without ambiguity. We have made some minor

Reviewer comments	Authors' response
	adjustments that we hope has made it clearer for the reader.
P15 L15 – It would be useful to cite some evidence for the validity of proxy-reported child PA levels and inclusion of parent's change perceptions.	We have added a sentence into the discussion (paragraph that starts "a key challenge for childhood obesity treatment") to acknowledge the potential for social desirability bias from proxy-report, and cited some evidence that parent-proxy report can be reliable and valid in the obesity domain.
P19 L40 – The intervention is described as being based on the Social Cognitive Framework here – and yet, this has not been mentioned previously. I'd like to see some discussion of the rationale for use, and operationalization of this theory in the methods.	We have added a brief discussion of Social Cognitive Theory (SCT) and the rationale for its use to the introduction, plus inserted references to SCT throughout the article. There is some detail of how SCT is operationalised in table 1 (mostly in sections 2 ("why") and 9 ("tailoring")). Space limitations prevent further discussion here, but readers are referred to Stratton and Watson [7] for further discussion of techniques used to enhance self-efficacy for PA at GOALS.
P19 L48 – Do you have any information on sibling/other carer attendance at family PA sessions? Given the assertion that a strength of this study was its whole family approach, it would be good to examine how successful you were in engaging with wider members of the family.	We do not have an exact figure because siblings and wider family members often attended sporadically. However anecdotal evidence from GOALS monitoring data suggests approximately 60% of children attended with a parent/carer plus at least one other family member. We have added this information to support our argument.
P21 L7 – The manuscript would benefit some discussion of how the acknowledged levels of attrition could be reduced in future studies.	Unfortunately we do not know how levels of attrition can be reduced in community-based interventions of this nature. Over 8 years of delivering a child weight management programme in a socio-economically deprived urban community, we tried many different strategies to improve retention (e.g. incentives, telephoning families in between sessions, supporting families with transport). Yet the attrition rate remained constant at approximately 50%. Perhaps therefore the question should be about whether those who drop-out early benefit from the intervention. We have tried to follow families up who have dropped out but recruitment rates have been very low therefore we have been unable to gather any meaningful data. In the years that followed the study period, the

Reviewer comments	Authors' response
	structure of GOALS changed to a rolling, open-group format and we saw a small improvement in retention (possibly due to families feeling more able to return if they missed a week or two). However, this data is currently in preparation for publication elsewhere and we do not feel it would be appropriate to speculate about the potential for open groups to improve retention until we can present the data to support it.

Reviewer 2

Reviewer comments	Authors' response
Abstract Line 9. Suggest removing reporting of GOALS using the TIDierR checklist from the objectives Line 24. Participants. Remove (/43) Line 25. Replace parents with Parents/carers Line 31. 18x 2-hour weekly group sessions – remove the x after 18 and replace 2 with two. (i.e. 18 two-hour sessions). How long does GOALS run for? Approx 6 months? Put this in the intervention section not line 38	Changes completed
Article summary Line 28. Add '6 months' to define 'post intervention' Key messages: Line 37: The term 'promising' is ambiguous and offers little meaning in the sentence Include in this section BMI-z-score changes. Line 48: Was it a key finding of your study that the TIDieR checklist is a mechanism for clear reporting? I would expect this was reported in the original TIDieR paper and hence why the checklist exists. Perhaps rephrase this bullet point.	Changes completed. We also deleted the word "promising" from the conclusion for consistency. We would like to note the middle section of the article summary refers to key messages, rather than key findings. We acknowledge the use of TIDieR is not a finding of our paper, but we wish to put across the message that using frameworks such as TIDieR is important to improve the standard of reporting in childhood obesity treatment research.
Introduction It would be useful to state in the introduction that	

Reviewer comments	Authors' response
GOALS was based on SCT Line 20: reference 1 needs to be in square brackets. I think the risks for childhood obesity could be expanded a little here – not overly so as many people reading the paper will be familiar with the risk factors. However, as it reads at the moment there is no information on why childhood obesity is a problem.	Brackets for reference 1 corrected. We have added a brief discussion of SCT to the introduction, plus information about the current obesity levels in children and associated health risks.
Page 6. Lines 8-13. It is difficult to understand what the meaning of this sentence is. Line 34. 'the checklist covers….modifications made during the study period' Your checklist doesn't seem to present modifications made. I see this is covered in supplementary information. This is very useful especially for researchers looking to learn from your experiences to assist in developing their own programmes. Is there any way this can be combined to all be presented in table 1? Lines 41-60. The paragraph and bullet points repeat each other. The use of the TIDieR checklist should come before the statement that the paper is reporting results of the intervention.	Sentence re-worded to clarify meaning. As BMJ journals limit tables to 2 pages, it is not possible to include the modifications made within table 1. We also feel this information is more clearly presented as a separate table (since it requires different column headings, and the changes relate to a number of other TIDieR components). However, we agree it would be optimal to include the intervention modifications within the main article (at the editor's discretion) therefore we have changed supplementary online resource 1 to a table (table 2). We have refined the text to reduce repetition, and to reflect the focus of the paper on the evaluation of the intervention.
Page 7 This section needs to be titled Methods. The methods section is a bit unclear and would benefit from re-ordering. If the paper is to describe the results of this study (the effect of the GOALS intervention) a more logical order would be: Participants Intervention (GOALS) Outcome measures Analysis	We have re-ordered the methods section as suggested.

Reviewer comments	Authors' response
Line 9: add United Kingdom after Liverpool John Moores University Lines 18 & 22: Replace %ile with the term 'percentile' Line 38: optional addition 'the way in which the intervention was delivered' Page 9 Line 12 'first author' it would be good to use author initials here Lines 11-20 (who provided). I think I know what you mean but the first line says delivered by the research team, and then later you say you use sessional staff. Just ensure it's clear whether you used a combination of people for delivery.	Changes completed. Author initials added and text amended for clarity.
Page 10 Lines 6: Evaluation methods. I suggest omitting this section. Line 24: invited to take part in this study? (check where else this occurs in the manuscript, 'this study' makes it more specific than 'the study') Line 35: …had complete baseline and post intervention (6 months) BMI data Lines 38 onwards – Participant flow through study – Report this whole section as Results. Lines 42-47. These are participant characteristics not 'flow through the study' Lines 44. BMI-z-score. These aren't very meaningful, could you report x% were obese, x	Section omitted. As this was removed, we have expanded the wording under "design" in the abstract to explain the study combines a single-group repeated measures design with qualitative questionnaires. We have used "the study" throughout the methods section (which is exclusively focussed on our work), whilst "this study" is used during the introduction and discussion (which discusses our work in the context of other research). This approach is in line with other BMJ Open publications in childhood obesity treatment (e.g. [5,8]). Changed as suggested.

Reviewer comments	Authors' response
% overweight? Ethnicity data – 85% were White British – what were the other ethnic groups?	
Line 45: remove /143	Section re-ordered and moved to the start of the results under two headings ("baseline characteristics" and "participant flow through study"). We have moved the criteria for inclusion in the complete case analysis to this section also.
Line 46-47. The numbers as percentages aren't very useful, leave these as whole numbers. Line 49. Use the phrase parent/carer throughout manuscript rather than abbreviating to parent.	Data added about other ethnic groups and numbers of children falling into each weight category (to maintain consistency, we used whole numbers rather than percentages). Removed. Amended to whole numbers. Sentence deleted and amended to parent/carer throughout article.
Page 11: Line 23: Measures – rephrase to Outcome measures	Completed.
BMI Lines 25-35 (BMI) - who measured the children? Trained researchers? Their parents? - was only one measure taken or two/three taken to ensure accuracy? Did you use a computer programme to convert to z scores? LMS method?	Details added as requested.
Child self perceptions Line 41: Why italics for SPPC Page 12	
Changes in PA and Diet Why did you not use a combination of qualitative and quantitative questions / a validated PA questionnaire and diet measure. Please justify the approach used.	

Reviewer comments	Authors' response
	Italics removed.
How did you collect information on ethnicity? Did you collect information on deprivation status? Were the session staff observed and consistency in delivery checked across staff/centres? Did you assess anywhere the fidelity of implementation?	At the time of the study, we were unable to locate a suitably validated measure of child PA or diet. During the initial year of delivery (2006-2007), we piloted a 24-hour food intake questionnaire[9] and some PA recall questions, but because baseline data collection often fell after a school holiday period and post-intervention data collection fell in term-time, the results of such recall tools could not be considered valid. We also found the food intake questionnaire did not pick up key changes that we were observing in families, such as reduced portion sizes. Therefore we developed an open-ended questionnaire that provided more in-depth information about the kind of changes participants were experiencing. We collected ethnicity and other demographic information from participants at baseline. If participants did not wish to disclose their ethnicity they left this section of the form blank. We have added a sentence in the participants section explaining how demographic data was collected. Yes, we collected participant postcodes. We have added deprivation information in the baseline characteristics section at the start of the results. Information about consistency of delivery is provided in the bottom row of table 1 (TIDieR items 11 & 12).

Reviewer comments	Authors' response
Analysis SPSS v17 (date and reference?) Line 28-30 Analysed deductively – what does this mean? Was it possible to control for any confounders in your statistics? Ethnicity? Gender? Age of children? Weight status of parents? Which GOALS session they were taking part in? If you don't have individual participant postcode for deprivation can you use postcode of the centre where GOALS intervention was delivered?	Date and reference added. Deductive analysis is a standard qualitative term that means data is analysed against pre-determined themes. For clarity, we have removed the term from this sentence since it reads more clearly without it. See above for discussion about potential confounders. We have added an explanation to show how the potential non-independence of children (within intervention cohorts) was addressed. We have added information about measuring the relationship between age and gender and child BMI z-score change, plus a sentence in the results (under "child outcomes") to note that there were no significant relationships with gender or age. Since we did not have parental BMI and ethnicity data for all children, the influence of these factors was not measured.
Results Can a table be included to present Participant characteristics? Table 2: 'complete with follow up' can this term be 'complete with 12 month follow up' Correlations between BMI z-score and self perceptions Lines 15-22. Measuring correlations was not measured in your analysis section. Parent BMI Could this be title be: Change in Parent BMI?	It is not possible to add a table due to space limitations after incorporation of the modifications and qualitative data into tables (BMJ Open states a maximum of 5 tables/figures). Instead we have summarised the key baseline characteristics in the first paragraph of the results section. Changed as requested.

Reviewer comments	Authors' response
	Information added to the analysis section. Changed as requested.
Parent reported changed in family PA and diet Line 55: I do not understand what you mean by family pairs The suggestion is this section has been analysed qualitatively with the discussion of 'themes' but there is use of numbers throughout. e.g. '41 parents, 34 of whom felt their activity level had improved'. Providing a summary paragraph with the key findings and then tabulating the quotes may be a better way of presenting the results. This section is very long at the moment and would benefit from being made shorter and more concise. Line 54: should this be '…tired or struggling'?	Re-written to clarify meaning. We have restructured the post-intervention section so the key changes are reported in a table (table 4) and summarised in a supporting paragraph. We have not changed the structure of the 12-month section as the small numbers and heterogeneity of responses are more appropriately represented as qualitative data in the text. We believe this is correct, and the meaning is that the child spent a lot of his time struggling, and when he got tired of struggling he gave up. However to avoid ambiguity, we have used the first part of the quote only as this illustrates the point sufficiently (i.e. "my son tries much harder now without giving up too soon")

Reviewer comments	Authors' response
Discussion Page 18 Lines 17-19. First sentence is unnecessary	
Line 22: you say the findings are consistent with other feasibility studies – was GOALS a feasibility study?	Sentence deleted.
Page 19 Line 11: First line is awkwardly worded; the term directionality should be rephrased	We have re-worded this to clarify the similarity between GOALS and these particular studies.
Figure 1 'Excluded: non-referred overweight siblings n=17' why are these included in eligible participants?	Sentence re-worded to clarify meaning.
Defining completed as still attending at end of intervention – how many sessions did they have to attend to be classed as complete? What if they only went to 25% of the sessions, but came to the last one, was this classed as complete?	
Child excluded for large BMIz-score change – could this have been measurement error?	These children were within eligible *families*, then were excluded at the participant level. We have amended the wording in figure 1 to make this clearer.
Supplementary Online resource 1 When describing school year - eg Year 1, Year 2, Year should be capital Y Replace %ile with percentile	
Year 1 interventions were delivered in primary schools but access to cooking facilities was limited – does this mean that a different intervention was delivered which didn't include cooking element?	Participants were required to have attended at least 50% of sessions and still be attending at the end of the intervention. We have added this detail in (in the participant flow through study section), plus a median attendance figure (83.3%) to provide a picture of the overall attendance rates.
	See explanation provided above.

Reviewer comments	Authors' response
	Changed as requested. All interventions included the cooking element. Although access to cooking facilities was possible in primary schools, it was more challenging to gain access since we often required use of school kitchens, whereas secondary schools had ready-made food technology rooms. We have rephrased to make this clearer.

References

1. National Obesity Observatory. Standard Evaluation Framework for Weight Management Interventions. Available from www.noo.org.uk National Obesity 2009.
2. Watson PM, Dugdill L, Murphy R, *et al.* Moving forward in childhood obesity treatment: A call for translational research. *Health Educ J* 2013;72(2):230-9.
3. Watson P. Feasibility eveluation and long-term follow up of a family-based behaviour change intervention for overweight children (GOALS). Doctoral thesis: Liverpool John Moores University 2012.
4. Rudolf M, Christie D, McElhone S, *et al*. WATCH IT: a community based programme for obese children and adolescents. *Arch Dis Child* 2006*, 91*, 736-739.
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7. Stratton GS, Watson PM. Young people and physical activity. In: Dugdill L, Crone D, Murphy R, eds. Physical Activity and Health Promotion: Evidence-based Approaches to Practice. Oxford: Wiley-Blackwell 2009: 150-69.
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9. Johnson B, Hackett, A F, Roundfield M, *et al.* Coufopoulos, A. (2001). An investigation of the validity and reliability of a food intake questionnaire. *J Hum Nutr Diet* 2001, 14, 457-465.

VERSION 2 – REVIEW

REVIEWER	Dr Tania Griffin University of Birmingham, UK
REVIEW RETURNED	15-Dec-2014

GENERAL COMMENTS	The authors have made good changes to the manuscript and it is much improved however I think still needs some work to improve its flow and focus. At the moment it's quite long and rambling in places which means as a reader you start to lose sight of what the papers overall aims are. In particular the results and discussion could be shortened and made more concise. A few specific comments: The abstract conclusion bears a lot of weight to the TIDieR checklist which has been toned down in the main article, the abstract conclusion benefits from its inclusion but perhaps in more concise terms. The article summary section – is this necessary, I think the manuscript would flow better without it but this may be a requirement of the journal. A quick look at other recent BMJ open publications I didn't spot this section, but this may be I was looking at the wrong article type. The line numbers have not worked on the PDF so I will refer to pages and blocks of text where necessary: Bottom of page 5 'Despite many childhood obesity ….[16]' I do not understand this sentence Page 6, top paragraph: I think there are a number of child obesity programmes who provide PA sessions with family. And this statement if it remains needs to have an age example as the next sentence contradicts it, highlighting a study with pre-schoolers which involved parent/child PA Page 7: methods. No indication of how a child was referred to GOALS. Analysis: I am unfamiliar with this method of testing the effect of clustering and then deeming it to not impact. There is no discussion of how you tested for normality. In response to authors comments regarding confounding factors you refer to deprivation. Could this have been incorporated into your statistical models by using regression analysis. I am not a statistician, and these methods may be acceptable, it is just an approach I am less familiar with. Baseline characteristics: This section is rambling and difficult to follow. Apologies if I miss it, but I can't find the gender divide of both children and parents in your final sample. Page 16: you have a title 'child outcomes' could you add in 'parent/carer outcomes' when you get to that section to make it consistent Discussion is long and rambling and difficult to read, would benefit

	from being made a little more concise, in particular the section on social acceptance/self esteem.

VERSION 2 – AUTHORS RESPONSE

Response to reviewer comments, following e-mail dated 17th December 2014.

General comments

Reviewer comments	Authors' response
The authors have made good changes to the manuscript and it is much improved however I think still needs some work to improve its flow and focus. At the moment it's quite long and rambling in places which means as a reader you start to lose sight of what the papers overall aims are. In particular the results and discussion could be shortened and made more concise.	Following agreement from Surayya Johar from the BMJ Editorial Office (e-mail dated 22nd December 2014), we have kept the results as they read now and made only minimal changes to the discussion (to make writing more concise).
Discussion is long and rambling and difficult to read, would benefit from being made a little more concise, in particular the section on social acceptance/self-esteem.	The rationale for this decision was as follows: - **Results:** Following the initial reviews of our manuscript, the results section was shortened for revision 1. We feel to condense this section further would require the removal of key information. - **Discussion:** In the review of our original manuscript (dated 24th October 2014) the reviewer commented: *"The Discussion was excellent, providing interesting insights to the data reported, and comparing results with similar or relevant studies."* Other than some minor changes we did not change the text between the original manuscript and revision 1 (including the section on social acceptance/self-esteem), therefore we have retained the discussion due to its previous positive reviews.

Specific edits

Reviewer comments	Authors' response (page numbers refer to revision 2 of the manuscript as it appears in the word document)
The abstract conclusion bears a lot of weight to the TIDieR checklist which has been toned down in the main article, the abstract conclusion	We have amended the abstract conclusion to tone down the emphasis on TIDieR and added in detail that was removed from the "key

Reviewer comments	Authors' response (page numbers refer to revision 2 of the manuscript as it appears in the word document)
benefits from its inclusion but perhaps in more concise terms.	messages" section (see point below).
The article summary section – is this necessary, I think the manuscript would flow better without it but this may be a requirement of the journal. A quick look at other recent BMJ open publications I didn't spot this section, but this may be I was looking at the wrong article type.	We understand the article summary is no longer required by BMJ Open (only strengths and limitations). Therefore we have removed the "article focus" and "key messages" sections.
Bottom of page 5 'Despite many childhood obesity ….[16]' I do not understand this sentence	Amended for clarity.
Page 6, top paragraph: I think there are a number of child obesity programmes who provide PA sessions with family. And this statement if it remains needs to have an age example as the next sentence contradicts it, highlighting a study with pre-schoolers which involved parent/child PA	It is possible there are some childhood obesity treatment interventions that do provide parent/carer and child PA sessions. To our knowledge however there are no published evaluations of these. The next sentence refers to studies outside of a childhood obesity treatment setting (i.e. focussed on PA promotion or father weight management), rather than a difference in age group. We have amended the text to make these emphases clearer.
Page 7: methods. No indication of how a child was referred to GOALS.	Details of how children were referred to GOALS are included in table 1 (In the row entitled "*What – procedure (4)"*).
Analysis: I am unfamiliar with this method of testing the effect of clustering and then deeming it to not impact.	This analysis approach follows the guidelines of the Bristol Centre for Multilevel Modelling. Chapter 5 (section C5.1.1) of the LEMMA (Learning environment for multilevel methodology and applications) online learning course (http://www.cmm.bris.ac.uk/lemma/) states that if, after testing the amount of variance accounted for by between-group differences (i.e. intervention cohort) *"we fail to reject the null* [hypothesis] *we would be justified in fitting a single-level model"*. It is common protocol for repeated measures evaluations of childhood obesity treatment interventions to employ single-level analyses, with little acknowledgement of the potential effects of clustering (e.g.[1,2]). We go beyond this by acknowledging the potential non-independence of data and explaining to the reader the process through which we ensured the statistical analysis was the most appropriate for the data set.
There is no discussion of how you tested for	We have added a sentence in the analysis

Reviewer comments	Authors' response (page numbers refer to revision 2 of the manuscript as it appears in the word document)
normality.	section to explain how we tested the data for normality.
In response to authors comments regarding confounding factors you refer to deprivation. Could this have been incorporated into your statistical models by using regression analysis. I am not a statistician, and these methods may be acceptable, it is just an approach I am less familiar with.	It would have been possible to conduct a regression analysis with BMI z-score change as the dependent variable, however the aim of this paper was not to explore the predictors of BMI z-score change. It is not necessary to adjust for confounding factors such as deprivation in repeated measures designs (see[1,2] for examples of similar studies), since individual differences are accounted for by the repeat measures and there are no between-group effects being investigated.
Baseline characteristics: This section is rambling and difficult to follow.	This section describes the necessary participant details as requested by the reviewers in their previous comments. It is not possible to put this information in a table due to space limitations (BMJ Open states a maximum of 5 tables/figures).
Apologies if I miss it, but I can't find the gender divide of both children and parents in your final sample.	We have added the relevant details into the paragraph entitled "Participant flow through study" (under "Results", p.15). Apologies for the oversight.
Page 16: you have a title 'child outcomes' could you add in 'parent/carer outcomes' when you get to that section to make it consistent	We have amended the heading at the bottom of p.16 to read "Parent/carer outcomes" (previously read "changes in parent BMI"). We also noticed one or two places where parent needed to be replaced with parent/carer (in the abstract and table 1) and amended these accordingly.

References

10. Rudolf M, Christie D, McElhone S, *et al*. WATCH IT: a community based programme for obese children and adolescents. *Arch Dis Child* 2006*, 91*, 736-739.

11. Smith LR, Chadwick P, Radley D, *et al.* Assessing the short-term outcomes of a community-based intervention for overweight and obese children: The MEND 5-7 programme. *BMJ Open* 2013;3:e002607. doi:10.1136/bmjopen-2013-002607