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)	Baby knows best? The impact of weaning style on food preferences and Body Mass Index in early childhood in a case-controlled sample
AUTHORS	Ellen Townsend and Nicola Pitchford

VERSION 1 - REVIEW

REVIEWER	Dr. Amy Brown Lecturer College of Human and Health Sciences Swansea University SA2 8PP
REVIEW RETURNED	01/09/2011

RESULTS & CONCLUSIONS	In terms of the results and discussion, I have three main questions. 1. What is the influence of breastfeeding duration? Mothers in the baby-led sample breastfed for a significantly longer duration than those in the spoon-fed group. Breastfeeding is associated with a reduced incidence of childhood overweight, potentially through encouraging infant self regulation of appetite. Could breastfeeding explain some of the variance in infant weight? Adding breastfeeding duration as a covariate would be useful. 2. Reducing the sample to a matched pairs design overcomes the issues of variation in infant age but limits sample size considerably. How do the findings compare if all participants remain in the sample but child age is used as a covariate? 3. Greater consideration needs to be given in the discussion to the issue of dividing mothers into baby-led and spoon-fed. For the purpose of the analyses I feel the distinction is fair and supported by data collected. However, in general, greater discussion and research needs to arise over the distinction between baby-led and spoon-fed. Mothers in the sample identified themselves as baby-led but still used spoon feeding at times. Are they really dichotomous and two distinct approaches or a continuum that mothers find a place along? Moreover, is the key component of baby-led weaning the fact that the infant is placed in control of choice and intake or is it something specific to do with self feeding and lack of puree use? Previous research has shown that mothers who follow baby-led weaning report lower levels of control. Which factor influences weight and eating style? And does this dichotomy matter? Are perhaps the key messages here developmental readiness for solid foods and a responsive maternal feeding style rather than a choice between self feeding or spoon-fed? Overall a prospective controlled study is needed to fully examine the outcomes of weaning style upon child weight and eating style.

REVIEWER	Prof MM Hetherington IPS University of Leeds LS2 9JT
REVIEW RETURNED	19/09/2011

THE STUDY	Limitations of the study include the potential confound of a significantly greater percentage of breastfed babies in the baby led weaning group influencing preference for carbohydrates since it is known that mothers transfer flavour in breastmilk and that breastfeeding confers benefits to babies including lowering BMI.
RESULTS & CONCLUSIONS	There is a confound with breastfeeding as described above which must be controlled for in the analysis of food preference by weaning strategy since this is likely to influence the findings.

VERSION 1 – AUTHOR RESPONSE

Reviewer: Dr. Amy Brown
Lecturer
College of Human and Health Sciences
Swansea University
SA2 8PP

In terms of the results and discussion, I have three main questions.
1. What is the influence of breastfeeding duration? Mothers in the baby-led sample breastfed for a significantly longer duration than those in the spoon-fed group. Breastfeeding is associated with a reduced incidence of childhood overweight, potentially through encouraging infant self regulation of appetite. Could breastfeeding explain some of the variance in infant weight? Adding breastfeeding duration as a covariate would be useful.

Response:
Dr Brown raises an important point about the protective effect of breastfeeding against overweight in childhood. The reviewer rightly points out that in the whole sample breastfeeding duration was longer in the Baby-Led group (see Table 1) which is a potential confound. To test the effect of breastfeeding duration on BMI (percentile rank) we conducted correlation analyses which revealed a weak correlation between breastfeeding duration and BMI in the whole sample which just failed to reach significance ($p=0.07$). The same pattern of results was observed for the matched sample. This has been noted both in the results section on page 7, paragraph 3, lines 7-9 and in the discussion section on page 9, paragraph 1, line 3. We have noted that this is in keeping with previous literature and that breastfeeding duration may be a mediating factor that requires further examination in relation to the impact of weaning practices on BMI.

The lack of a significant relationship between breastfeeding duration and BMI in our sample may be expected, given that nearly all children in the matched sample were breast-fed (92% in the Spoon-Fed group, 97% in the Baby-Led group). Indeed, in the whole sample 94.8% of the children had been breast fed. Thus, in future studies it will be important to compare food preferences and health outcomes in samples that contain a greater proportion of children who were bottle/formula fed as babies. We have added a note to this effect on page 10, paragraph 2, line 3.

2. Reducing the sample to a matched pairs design overcomes the issues of variation in infant age but limits sample size considerably. How do the findings compare if all participants remain in the sample

but child age is used as a covariate?

Response:
We chose to conduct a matched sample analysis as the preference data was significantly skewed for many food categories (making parametric covariate analysis inappropriate). However, we do report some robust effect sizes so it is unlikely that reduction in statistical power is a problem for our data. As the reviewer notes below, large prospective studies are now required to confirm our findings.

3. Greater consideration needs to be given in the discussion to the issue of dividing mothers into baby-led and spoon-fed. For the purpose of the analyses I feel the distinction is fair and supported by data collected. However, in general, greater discussion and research needs to arise over the distinction between baby-led and spoon-fed. Mothers in the sample identified themselves as baby-led but still used spoon feeding at times. Are they really dichotomous and two distinct approaches or a continuum that mothers find a place along? Moreover, is the key component of baby-led weaning the fact that the infant is placed in control of choice and intake or is it something specific to do with self feeding and lack of puree use? Previous research has shown that mothers who follow baby-led weaning report lower levels of control. Which factor influences weight and eating style? And does this dichotomy matter? Are perhaps the key messages here developmental readiness for solid foods and a responsive maternal feeding style rather than a choice between self feeding or spoon-fed?

Response:

These are important points. We have added a paragraph in the discussion section on page 9, paragraph 2, line 4 which encourages researchers to consider these issues in future studies.

4.Overall a prospective controlled study is needed to fully examine the outcomes of weaning style upon child weight and eating style.

Response:

We agree and have added a line to this effect on page 10, line 1.

Reviewer: Prof MM Hetherington
IPS
University of Leeds
LS2 9JT

1) Limitations of the study include the potential confound of a significantly greater percentage of breastfed babies in the baby led weaning group influencing preference for carbohydrates since it is known that mothers transfer flavour in breast milk and that breastfeeding confers benefits to babies including lowering BMI.

2) There is a confound with breastfeeding as described above which must be controlled for in the analysis of food preference by weaning strategy since this is likely to influence the findings.

Response:
The differences in breastfeeding between the groups (evidenced in Table 1) refer to the whole sample rather than the matched sample (for whom food preferences were analysed). When we examined differences between the groups in the matched sample we found no significant differences in breastfeeding with 92% of the Spoon-fed group and 97% of the Baby-Led group having been breast

fed. We have added a note to this effect to Table 1 on page 14. Thus, the potential confound with breastfeeding does not apply to the matched sample for which food preferences were tested. However, as noted in our comments above in response to Dr Brown there is a potential ceiling effect in operation here as most mothers in the matched sample had breast fed their children (indeed in the whole sample 94.8% of the children had been breast fed). This is an important factor that should be investigated in future prospective studies in tandem with weaning practices. We now need a large prospective study that examines weaning methods where breast fed children can be compared to bottle fed children. We have added a note about this in the discussion section on page 10, line 1.

VERSION 2 – REVIEW

REVIEWER	Jennifer Baker, PhD, Seniore Research Associate, Institute of Preventive Medicine, Denmark I have no conflicts of interest to declare.
REVIEW RETURNED	31/10/2011

THE STUDY	A flow diagram would be very helpful in understanding how the study population was constructed, especially with regard to the case-control matching procedure as well as to understand which participants were included in each analysis. The information is already included in the manuscript, but it isn't easy to follow in its current form. I am a bit in doubt about the method used to match the cases and controls. It would seem that cases and controls should be matched on the same criteria rather than a variety of criteria (i.e. some were matched on only age whereas others were matched on age and gender, etc.) to keep the comparisons between the groups similar. Please verify that the method used was appropriate.
RESULTS & CONCLUSIONS	I think the authors need to reconsider their conclusions about childhood BMI. I think they should consider using another analytic method (i.e. zscores rather than percentile rankings) and they should consider an effect of reverse causality; i.e. in the babyled group, as many children did not have height and weight measurements, was it perhaps only children with growth concerns who had these measurements available (as in they had been to the doctor's office more recently)? Thus the differences observed could be due to the ill children and not the feeding method.
GENERAL COMMENTS	The study "Baby knows best? The impact of weaning style on food preferences and Body Mass Index in early childhood in a case-controlled sample" addresses a very important gap in the knowledge of baby feeding, namely what are the consequences weaning methods on food preferences and health. The study used a population of infants sourced from the internet as well as a laboratory database. The authors found that there are differences in food preferences between feeding groups. I think, given the limitations of so much missing data on BMI and the method used to test the difference, I'm in doubt about the conclusions about effects on body size. The discussion was well written and interesting. I enjoyed reading the paper, and results from this study will contribute greatly to the literature. Page 5-6: The description of the statistical analyses used and the creation of the baby-lead weaning groups seem as though they belong in the "methods" section rather than the results section.

	Table 1 is informative, but if the analyses were conducted on the case-control groups, an additional table with the same information as in Table 1 based just upon these subjects should be included as well. Page 7: I would suggest reordering the paragraph describing Table 3. I expected to read the last sentence first before reading the other results. · If I understand correctly, the BMI analyses were performed on all children and not just the case-control group? This needs to be made more evident that these analyses are on a different group of children (as this is in contrast to the previous sets of results described). Discussion: Page 9: I'd suggest removing the priority claim; it is always possible that a literature search has missed another study. Discuss another strength of this study instead. Table 1: Was the mean duration of breastfeeding really 24 months among the baby-led group? Please double check the methods used to calculate the standard deviations; some of the values seem improbable (i.e. breastfeeding, BMI, etc.) Table 3: It would be useful if a footnote were provided describing what the exposure ratings indicate (i.e. higher number=greater preference). Similarly, why are the food categories ordered in the way they are? I'd expect them to be ordered by preference, or even just alphabetical order (and the same applies to eTable 1).

REVIEWER	Dr Amy Brown Lecturer Human and Health sciences Swansea University UK
REVIEW RETURNED	11/11/2011

GENERAL COMMENTS	Much improved - an interesting paper which raises a lot of questions for future reseach in this area of growing popularity.

VERSION 2 – AUTHOR RESPONSE

Reviewer 3: Jennifer Baker, PhD, Seniore Research Associate, Institute of Preventive Medicine, Denmark

1) A flow diagram would be very helpful in understanding how the study population was constructed, especially with regard to the case-control matching procedure as well as to understand which participants were included in each analysis. The information is already included in the manuscript, but it isn't easy to follow in its current form.

I am a bit in doubt about the method used to match the cases and controls. It would seem that cases and controls should be matched on the same criteria rather than a variety of criteria (i.e. some were matched on only age whereas others were matched on age and gender, etc.) to keep the comparisons between the groups similar. Please verify that the method used was appropriate.

Response: The information about the matching used has been clarified in the legend on table 2.

Crucially we matched all participants on age first. In some cases we had several control participants that matched a case participant on age. In this instance we used other criteria (gender, SES etc) to select the control participant. This was done in preference to random sampling of matches (where there were multiple cases matching on age to a control). Our procedure yields a more tightly matched sample that is more conservative in nature (it should be more difficult to find significant results).

2) I think the authors need to reconsider their conclusions about childhood BMI. I think they should consider using another analytic method (i.e. zscores rather than percentile rankings) and they should consider an effect of reverse causality; i.e. in the babyled group, as many children did not have height and weight measurements, was it perhaps only children with growth concerns who had these measurements available (as in they had been to the doctor's office more recently)? Thus the differences observed could be due to the ill children and not the feeding method.

Response: We have conducted an analysis of the BMI raw scores using z-scores as suggested using the WHO Growth Standards. These are presented in a new table (see Table 4). The analysis revealed an increased incidence of (a) obesity in the Spoon-Fed group (z-score >+2) and (b) underweight in the Baby-Led group (z-score >-2). We have used the new BMI z-cores for all the correlation analyses presented in the manuscript (see page 8). In addition, we have kept the information about percentiles in the ms as this information is of great interest to clinicians. We have provided percentile ranks for both UK and US reference norms for comparison as there is a consistent pattern of results across the different classification systems.

The abstract, 'Key Message' and discussion sections have been revised in the light of the new BMI analyses.

3) The study "Baby knows best? The impact of weaning style on food preferences and Body Mass Index in early childhood in a case-controlled sample" addresses a very important gap in the knowledge of baby feeding, namely what are the consequences weaning methods on food preferences and health. The study used a population of infants sourced from the internet as well as a laboratory database. The authors found that there are differences in food preferences between feeding groups. I think, given the limitations of so much missing data on BMI and the method used to test the difference, I'm in doubt about the conclusions about effects on body size. The discussion was well written and interesting. I enjoyed reading the paper, and results from this study will contribute greatly to the literature.

Response: We acknowledge the missing data as a limitation of the study which must be improved on in the future. We have improved the situation here by using the WHO growth reference tables to calculate BMIs for children less than 24 months (who were previously not included in our analyses). This means that BMI data for 81% of participants is now included. We note that for both groups the vast majority of the children were of an average/ healthy weight across measures of BMI (see page 10, paragraph 1, line 2).

4) Page 5-6: The description of the statistical analyses used and the creation of the baby-lead weaning groups seem as though they belong in the "methods" section rather than the results section.

Response: These sections have been moved to the methods section as suggested. We suggest that Table one should be kept in the results section.

5) Table 1 is informative, but if the analyses were conducted on the case-control groups, an additional table with the same information as in Table 1 based just upon these subjects should be included as well.

Response: We have clarified that the statistical analyses here are conducted on the whole sample. (The Ns for the analyses were already included in the table to indicate that the whole sample was used for the analyses, but we have now also added a comment on the table legend to highlight that analyses were conducted for the whole sample.)

6) Page 7: I would suggest reordering the paragraph describing Table 3. I expected to read the last sentence first before reading the other results.

Response: We have changed the order as suggested.

7) If I understand correctly, the BMI analyses were performed on all children and not just the case-control group? This needs to be made more evident that these analyses are on a different group of children (as this is in contrast to the previous sets of results described).

Response: We have highlighted in Table 1 that all analyses were performed on the whole sample. We have also added a sentence on page 7, paragraph 3, line 3. As noted above the effect for BMI is also found in the matched sample (the BLW group had lower BMI percentile scores than the spoon-fed group). We have added a note to this effect in the text on page 7, paragraph 3, line 3.

8) Discussion:
Page 9: I'd suggest removing the priority claim; it is always possible that a literature search has missed another study. Discuss another strength of this study instead.

Response: We would like to keep the priority claim as this is very common in science and is a key reason for the publication of novel, original empirical work. We have, however, qualified the claim to read 'To the best of our knowledge, this is the first study to ….' (see page 10, paragraph 3, line 1). We have conducted a thorough search of the literature.

9) Table 1: Was the mean duration of breastfeeding really 24 months among the baby-led group? Please double check the methods used to calculate the standard deviations; some of the values seem improbable (i.e. breastfeeding, BMI, etc.)

Response: The data, analysis and methods used to calculate these figure has been checked again (back to the original questionnaires for some cases with long breastfeeding durations). This revealed that the mean for the spoon-fed group had been rounded incorrectly and should have been 9.5. This has been corrected in the table. We also found that one breastfeeding duration data point had been entered incorrectly in SPSS, but as you can see from the revised table this made no significant difference to the mean duration in the Baby-Led group once this was recalculated. We have redone the analyses involving breastfeeding duration (the correlations with BMI) and these were also unaffected. We have also double-checked all the figures in all the tables presented in the manuscript. The breastfeeding durations are long but perhaps not improbable when viewed in the context of WHO guidelines on breast feeding which state: "On a population basis, exclusive breastfeeding for the first six months of life is the recommended way of feeding infants, followed by continued breastfeeding with appropriate complementary foods for up to two years or beyond." (see:http://www.who.int/child_adolescent_health/topics/prevention_care/child/nutrition/breastfeeding/en/index.html)

10) Table 3: It would be useful if a footnote were provided describing what the exposure ratings indicate (i.e. higher number=greater preference).

Response: A comment was already included in the footnote to Table 3 indicating that, “Lower exposure scores indicate more frequent consumption.”

11) Similarly, why are the food categories ordered in the way they are? I’d expect them to be ordered by preference, or even just alphabetical order (and the same applies to eTable 1).

Response: We had already stated explicitly in the title of Table 2 that the ordering of the data here was ‘presented in order of liking for each group’). We have ordered the food items presented in eTable1 alphabetically as suggested.