PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	How different are Baby-Led Weaning and conventional complementary feeding? A cross-sectional study of 6 to 8 month old infants
AUTHORS	Morison, Brittany; Taylor, Rachael; Haszard, Jillian; Schramm, Claire; Williams Erickson, Liz; Fangupo, Louise; Fleming, Elizabeth; Luciano, Ashley; Heath, Anne-Louise

VERSION 1 - REVIEW

REVIEWER	Charlotte Wright University of Glasgow, UK
REVIEW RETURNED	17-Dec-2015

GENERAL COMMENTS	Thank you for asking me to review this paper that addresses the question of the dietary content of foods eaten by children following the baby-led weaning approach compared to traditional spoon feeding. The paper describes results for 25 children showing a mainly or partly baby-led weaning approach compared to 26 following a traditional spoon feeding approach at the age of 6-8 months. The strength of this analysis is that it draws on dietary data collected using a weighed diet record and addresses a reasonably novel question. However there are many methodological problems in this analysis and my overall feeling is this study constitutes pilot data that does not in itself merit publication. Some of the limitations of the data could be addressed by limiting or adjusting the analysis, but most of these would result in greatly reduced power. As the sample size is already very modest, this would then expose the whole analysis to the risk of finding no apparent difference when in fact the study was underpowered to detect real differences. My comments in detail: 1. The analysis brings together subjects recruited for 3 different studies, although all took place at roughly the same time and in the same geographical setting. One of the three studies used a different methodology, which meant that 7 of the total number of children had only 1 day weighed dietary record as opposed to a 3 day dietary record. 2. The families involved are very unlikely to be representative of the New Zealand population. They were recruited by advertisements and social media sites rather from any systematic sampling approach and then, to add further limitation, in the first 2 studies only about two thirds of the parents had completed the 3 day food diary and in the third study participants were restricted to those who had consented to participate in a randomised trial of baby-led weaning.

	In this latter study we are not told how many mothers were approached regarding possibly recruitment or the total number actually recruited, but again, realistically the mothers included are unlikely to be representative of the general population. 3. While this is ostensibly a study of baby-led weaning, compared to traditional spoon feeding, only 18 of the participants were fully following the baby-led weaning pattern yet these are pooled with those who were partially following it. This makes the results hard to interpret since, for example, one third of the 'baby-led weaning' group had started solid feeding with spoonable purees and only half of the baby-led weaning group were all or mostly self-fed. I suspect the authors have opted to combine the partially and fully baby-led weaning group in order to get a larger number, but the result is that the findings have really very little useful meaning in a study of BLW. Therefore an essential modification of this paper would be to examine the families in 3 groups – traditional, partially and fully baby-led weaning. 4. Some of the analysis of the dietary data uses only the first day of the diet record, which is available for all 3 groups of records, while other analyses use some sort of averaging methodology using all the days of the diet record. I am not familiar with the statistical technique specified, but I would strongly suggest that statistical review is sought about this, since the results of these analyses seem inherently implausible given the small numbers. Thus in table 4 there are many highly significant differences in dietary intake, even though some of these are as low as a 7% difference. Dietary data are notoriously variable, so it would be most unusual to be able to detect significant differences between 2 groups as small as this. This leads me to suspect that the analytical method has used each day of the food diary as a separate unit of measurement, which would of have inflated the population size nearly threefold and this might explain the much higher than expected significance level. 5. While there is a 3 day weighed record of the solid food intake, the volume of breastmilk consumed is assumed on the basis of population averages when we know that this is also very likely to be very variable, which make it particularly difficult to tell what the true total intake of energy actually was for each individual child. 6. Both groups seem to have much lower than recommended intake levels but the authors make a big thing of the fact that the baby-led weaning infants have the lowest iron intake, mainly because they do not consume nearly so much in the way of commercially produced infant cereals which are fortified with mineral iron. However, consumption on red meat seems to be exactly the same between the 2 groups and it is haem iron that is particularly important as mineral iron is a very low bioavailability. Most important, there are no measures of actual iron status. 7. Similarly we are told that both groups eat a number of foods that present choking hazards but we are given no information about actual choking incidents. 8. Finally, the discussion of these results is inappropriate, given that this is a very small, unrepresentative sample of mothers of young infants in New Zealand. The authors make a number of general statements about the risk of choking hazards, iron intake and saturated fat intake, implying that these should be extrapolated to the population as a whole. If the group really feel it is worth continuing to pursue publication of this very small, preliminary dataset, it should reframe the discussion to emphasise that this study was a means of generating hypothesis for future studies of BLW rather than providing any robust information about baby-led weaning or the process of weaning in general.

REVIEWER	Amy Brown Swansea University UK
REVIEW RETURNED	20-Dec-2015

GENERAL COMMENTS	This is a useful and interesting paper in an emerging area of research. We know that more parents are following baby-led weaning despite the lack of empirical evidence for its safety and efficacy. Building an evidence base for this approach is important and of relevance to health professionals and those in health policy. Overall I think this is a well designed study. The results presented here form part of it. They present the energy and nutrient intake from a small number of matched infants and offer insight into the diet these infants may be eating. The sample size is small but the authors recognise this and given the novelty of the research (and the pressing research need) this work is an important step that should now lead to larger samples being explored. One point to consider is the self identification of the participants as baby-led or traditional. We know this can be an issue as self identification does not necessarily equate to one set of behaviours. It is clear from the sample behaviours that some who identify as baby-led do not follow 'strict' baby-led behaviours e.g. they were spoon-fed at the start of weaning. However, saying this, baby-led weaning is not a defined behaviour but more of a concept of guidelines that parents follow. What appears to emerge is a split between parents who follow traditional weaning (likely making their own purees / baby jars) and those who use family foods and allow the infant to self feed to varying degrees. It would be interesting in a larger sample to be able to split these infants into degrees of self feeding. However, what is notable is that it does not appear to matter how babies are fed / feed in terms of nutrient intake which challenges the concern that blw would not eat enough. Although BLW may consume less iron, it would be interesting to know whether this persists into later infancy. Similarly, the greater level of saturated fats. Is this to do with the types of foods babies can self feed at 6 - 8 months (e.g. before the pincer grip develops?). The wider picture also needs to be considered - if BLW are low in iron (a negative) but better at appetite regulation (a positive) then it may be beneficial for infants to be BLW but simply given iron supplementation and / or encouraged to consume iron fortified foods such as non infant cereals that are fortified with iron The high rate of infants being offered potential choking foods is concerning. However the lack of difference between the two groups suggests that following BLW does not increase this risk. Given that traditional babies ate fewer finger foods then logically this would suggest that on a ratio of food offered to frequency, these babies are being offered potential choking hazards more frequently. Finally, as always, we need to consider just what the key message from BLW is. Is it self feeding? Or is it allowing the infant to regulate intake?

VERSION 1 – AUTHOR RESPONSE

Reviewer 1:

1. The analysis brings together subjects recruited for 3 different studies, although all took place at roughly the same time and in the same geographical setting. One of the three studies used a different methodology, which meant that 7 of the total number of children had only 1 day weighed dietary record as opposed to a 3 day dietary record.

The reviewer is correct – the same data were collected from participants in all three studies, with the exception that a one-day rather than a three-day weighed diet record was collected from the 7 participants in the "How to Measure Infant Feeding" study. These 7 participants were evenly distributed between the Baby-Led Weaning (BLW) group (n=4) and the Traditional Spoon-Feeding (TSF) group (n=3).

2. The families involved are very unlikely to be representative of the New Zealand population. They were recruited by advertisements and social media sites rather from any systematic sampling approach and then, to add further limitation, in the first 2 studies only about two thirds of the parents had completed the 3 day food diary and in the third study participants were restricted to those who had consented to participate in a randomised trial of baby-led weaning. In this latter study we are not told how many mothers were approached regarding possibly recruitment or the total number actually recruited, but again, realistically the mothers included are unlikely to be representative of the general population.

Unfortunately it was not feasible to recruit using random sampling because of the huge screening effort that would have been needed – the detailed dietary data required have to be collected prospectively (i.e. they cannot be collected by parents recalling the intake of children who were baby-led weaned at some – variable - time in the past), a narrow age range was needed in order to observe behaviours during the early complementary feeding period (in this case 6 to 8 months of age), and, although anecdotal reports suggest that baby-led weaning is becoming more popular it is not yet being followed by a substantial proportion of parents, and we know that many parents who try baby-led weaning use it alongside traditional spoon-feeding (Cameron et al., 2013), so only a small proportion of parents with infants in our narrow age band would be able to contribute data on full baby-led weaning.

Our particular interest was in seeing whether Baby-Led Weaning differed to traditional spoon-feeding, so both BLW and TSF participants were recruited from each study, and participants were carefully matched for age (to within a week) and sex. This means that although they may not be representative of all infants of this age, they are demographically similar to each other so the differences seen in their diets are unlikely to be due to demographic factors.

We agree that it would be useful for the reader to have some information on how representative the participants are likely to be. The mean age of the maternal participants was higher than that for women giving birth in New Zealand in 2013 (33.8 vs 30.0 years; Statistics New Zealand, 2014a) – in keeping with the higher than expected proportion who were multiparous (86%). However, the number of participants identifying as non-European was the same as in the 2013 New Zealand census (74% vs 74%; Statistics New Zealand, 2014b). The only available data from a random sample of women suggests that 6% exclusively breastfeed to 6 months of age in New Zealand. In comparison, 0% the traditional spoon-feeding participants in the current study exclusively breastfed to 6 months.

We have added the following text to the limitations paragraph of the Discussion (Lines 443-450): "The maternal participants were older,25 and more likely to be multiparous,26 than typical New

Zealand mothers. However, they were just as likely as New Zealand mothers to be of non-European ethnicity,27 and the participants in the TSF group had a similar very low rate of exclusive breastfeeding to 6 months.27 Although there were some differences, therefore, between the study participants and the general public, the groups were closely matched for age, sex and the demographic variables measured so differences in the diets of the infants studied are not likely to be due to differences in demography."

In response to the Reviewer's concerns about missing data and recruitment rate: the approximately one third of participants who expressed interest in the study but whose data were not included were most commonly excluded because their infant did not meet the strict age criteria (6-8 months), or in the case of traditional spoon-feeders, were not a close enough age- or sex-match to a baby-led weaned infant, rather than because they did not complete a diet record. The BLISS study recruited 23% of all eligible births in Dunedin.

We have added the following text to the Methods (Line 125):
"All women booking into the only birthing facility in Dunedin, the Queen Mary Maternity Unit, Dunedin Hospital, were invited to participate in the BLISS study (23% of those eligible volunteered)."

3. While this is ostensibly a study of baby-led weaning, compared to traditional spoon feeding, only 18 of the participants were fully following the baby-led weaning pattern yet these are pooled with those who were partially following it. This makes the results hard to interpret since, for example, one third of the 'baby-led weaning' group had started solid feeding with spoonable purees and only half of the baby-led weaning group were all or mostly self-fed. I suspect the authors have opted to combine the partially and fully baby-led weaning group in order to get a larger number, but the result is that the findings have really very little useful meaning in a study of BLW. Therefore an essential modification of this paper would be to examine the families in 3 groups – traditional, partially and fully baby-led weaning.

Thank you for this suggestion. We have separated the participants into three groups – traditional spoon-feeding, partial BLW, and full BLW in the text and tables. All differences that were statistically significant in the original analysis have remained statistically significant, with larger differences for most variables. In addition, the separation of the partial and full BLW participants has made the full BLW group more homogenous so the number not yet introduced to iron-fortified infant cereal, % energy from protein, dietary fibre, zinc, and calcium are now statistically significantly different.

Appropriate changes have been made to the Tables, Abstract, Methods, Results and Discussion to reflect this, and are indicated in red in the revised paper.

4. Some of the analysis of the dietary data uses only the first day of the diet record, which is available for all 3 groups of records, while other analyses use some sort of averaging methodology using all the days of the diet record. I am not familiar with the statistical technique specified, but I would strongly suggest that statistical review is sought about this, since the results of these analyses seem inherently implausible given the small numbers. Thus in table 4 there are many highly significant differences in dietary intake, even though some of these are as low as a 7% difference. Dietary data are notoriously variable, so it would be most unusual to be able to detect significant differences between 2 groups as small as this. This leads me to suspect that the analytical method has used each day of the food diary as a separate unit of measurement, which would of have inflated the population size nearly threefold and this might explain the much higher than expected significance level.

As the reviewer notes, dietary data are notoriously variable so detecting small differences between

groups with a small sample size should be questioned. However, it should be noted that the smallest differences detected are for the % energy variables where variation between individuals is likely to be smaller. Furthermore, 95% confidence intervals of the mean results are given to illustrate the precision of the estimates and the data are log-transformed which reduces the variability in differences between the groups.

Tables 3 (foods), 4 (nutrients), and 5 (meals) all report data from the weighed diet records. However, the types of data differ, and this has impacted on the (a) summary data presented, and (b) statistical approach used.
Table 3 = Number of infants consuming the food of interest on any day of the weighed diet record*
(a) Binary data (i.e., "yes" –consumed the food at least once during the diet record; "no" – did not consume the food) presented as percentages.
(b) Odds ratios with a chi-squared test comparing the two groups.
Table 4 = Mean nutrient intake per day
(a) Continuous data presented as a mean for the diet record days.
(b) Mixed regression using every individual day of the data but taking into account correlation within individuals.
Table 5 = Number of infants eating the meal with their family
(a) Binary data (i.e., "yes" – consumed the meal with their family on that day; "no" – did not consume the meal with their family that day) presented as the percentage of infants with this behaviour on the first day of the weighed diet record. We decided not to use the mean for the weighed diet record as a whole for our summary statistic (which would have been analogous to Table 4) because this would have required substantial footnoting (infants of this age do not consume all three meals every day so the denominator would have varied for each of the 27 summary statistics presented).
(b) Generalized estimating equations using every individual day of data but taking into account correlation within individuals.

* NB: The original Table 3 used generalized estimating equations so that each individual day of data for every participant was included in the analysis, and we reported just the first day's intake in the tables. In hindsight this complicated things unnecessarily, making the tables difficult to understand. Ultimately, we were interested in whether the food was eaten at all during the weighed diet record, and this simpler question could be answered using a chi-squared test, with the percentage of infants consuming the food at all during the weighed diet record being used as the summary statistic.

We have added the following text to the Methods to reflect the new analysis for Table 3 (Lines 220-223):
"The number of infants in each group who consumed a food type of interest (e.g., foods posing a choking risk) or a type of milk (e.g., breast milk) on any day of the diet record was recorded and odds ratios with a chi-squared test comparing the two groups were calculated (Table 3)."

So, in summary, we have altered the analysis for Table 3 so that each person contributes a single value to the analysis (i.e. was there any consumption of the food at any time during the diet record) and this has enabled us to make the statistical analysis more intuitive. The analyses for Tables 4 and 5 remain the same, but we can reassure the reviewer that the mixed effects regressions (for the nutrients) and population-averaged generalized estimating equations (for the family meals) use all of the information available, but account for the within-person correlation so that each day of the diet record is not treated as an independent observation. The differences between the groups in the summary statistics (means, etc) are consistent with the inferential statistics (confidence intervals, p-values) suggesting that the precision of the estimates is not overestimated (i.e. our analyses have not reported statistically significant differences between the groups when none exists).

5. While there is a 3 day weighed record of the solid food intake, the volume of breastmilk consumed is assumed on the basis of population averages when we know that this is also very likely to be very variable, which make it particularly difficult to tell what the true total intake of energy actually was for each individual child.

We used an estimated volume of breast milk based on population data because test weighing would have significantly increased participant burden, and stable isotopes were prohibitively expensive. Estimated breast milk volume is commonly used in dietary studies in this age group (for example, Devaney et al., 2004; Sharma et al., 2013; Skinner et al., 1997).

It is somewhat reassuring that the energy intakes in the full BLW and TSF groups were not statistically significantly different, and were also very similar (a difference of just 3.7%). If the breast milk value we used underestimated breast milk volume, then overall energy intake would be higher in BLW – this is unlikely as studies to date have suggested lower rather than higher BMI in infants following BLW (Brown and Lee, 2015; Townsend and Pitchford, 2012). If the breast milk value we used overestimated breast milk volume, then nutrient intakes would be even lower than we have reported – that is, our estimates are conservative.

We have added the following text to the limitations section of the Discussion (Lines 451-460):
"Another limitation was the use of estimated breast milk volume. It was not feasible to use test weighing or stable isotopes to measure intake in each individual, and estimated volumes are is commonly used in dietary studies in infants.28-30 It is also somewhat reassuring that the energy intakes in the full BLW and TSF groups were not statistically significantly different, and were also very similar (a difference of just 3.7%.) If the breast milk value we used underestimated breast milk volume, then overall energy intake would be higher in BLW – this is unlikely as studies to date have suggested lower rather than higher BMI in infants following BLW.31 32 If the breast milk value we used overestimated breast milk volume, then the differences in nutrient intakes would be even greater than we have reported."

6. Both groups seem to have much lower than recommended intake levels but the authors make a big thing of the fact that the baby-led weaning infants have the lowest iron intake, mainly because they do not consume nearly so much in the way of commercially produced infant cereals which are fortified with mineral iron. However, consumption on red meat seems to be exactly the same between the 2 groups and it is haem iron that is particularly important as mineral iron is a very low bioavailability. Most important, there are no measures of actual iron status.

Non-haem iron is not absorbed as well as haem iron, however, it constitutes a large majority of the dietary iron intake so is still very important.

We have added the following text to the Discussion to emphasis the importance of the measurement of iron status (Lines 495-501):
"It is important that these results are confirmed in other studies, particularly studies determining iron status as well as intake so that it can be determined whether infants following BLW have poorer iron status. In the meantime, health care professionals should emphasise the importance of including iron rich food sources in infants' diets in the complementary feeding period because of the well-accepted challenges of achieving adequate iron intake at this age, whether BLW or TSF is being followed."

And retained the comment in the Conclusion that (Lines 579-582):
"Given the widespread interest and debate regarding the suitability of BLW as an alternative infant feeding method, further research in a larger, ideally representative, sample of children, preferably with measurements of growth, biochemical nutrient status, and rate of choking, is required to confirm these

findings."

7. Similarly we are told that both groups eat a number of foods that present choking hazards but we are given no information about actual choking incidents.

Data on choking incidents were not collected because of the small sample size, and relatively low prevalence of choking episodes.

8. Finally, the discussion of these results is inappropriate, given that this is a very small, unrepresentative sample of mothers of young infants in New Zealand. The authors make a number of general statements about the risk of choking hazards, iron intake and saturated fat intake, implying that these should be extrapolated to the population as a whole. If the group really feel it is worth continuing to pursue publication of this very small, preliminary dataset, it should reframe the discussion to emphasise that this study was a means of generating hypothesis for future studies of BLW rather than providing any robust information about baby-led weaning or the process of weaning in general.

We have added qualifiers in a number of places in the Discussion, such as "suggest", "may be", and "small study", and have also added the following text:

(Line 495)
"It is important that these results are confirmed in other studies …"

(Line 561)
"If confirmed …"

(Lines 524-526)
"It is, therefore, extremely important that exposure to foods posing a choking risk, and choking prevalence, are investigated in future, larger, studies of BLW."

(Lines 557-558)
"It is important to note when interpreting these dietary data that our study was small, cross-sectional, and not from a random sample of families"

And retained the comment in the Conclusion that (Lines 579-582):
"Given the widespread interest and debate regarding the suitability of BLW as an alternative infant feeding method, further research in a larger, ideally representative, sample of children, preferably with measurements of growth, biochemical nutrient status, and rate of choking, is required to confirm these findings."

Reviewer 2:

1. This is a useful and interesting paper in an emerging area of research. We know that more parents are following baby-led weaning despite the lack of empirical evidence for its safety and efficacy. Building an evidence base for this approach is important and of relevance to health professionals and those in health policy.

Overall I think this is a well designed study. The results presented here form part of it. They present the energy and nutrient intake from a small number of matched infants and offer insight into the diet

these infants may be eating. The sample size is small but the authors recognise this and given the novelty of the research (and the pressing research need) this work is an important step that should now lead to larger samples being explored.

Thank you for your comments.

2. One point to consider is the self identification of the participants as baby-led or traditional. We know this can be an issue as self identification does not necessarily equate to one set of behaviours. It is clear from the sample behaviours that some who identify as baby-led do not follow 'strict' baby-led behaviours e.g. they were spoon-fed at the start of weaning. However, saying this, baby-led weaning is not a defined behaviour but more of a concept of guidelines that parents follow. What appears to emerge is a split between parents who follow traditional weaning (likely making their own purees / baby jars) and those who use family foods and allow the infant to self feed to varying degrees. It would be interesting in a larger sample to be able to split these infants into degrees of self feeding. However, what is notable is that it does not appear to matter how babies are fed / feed in terms of nutrient intake which challenges the concern that blw would not eat enough.

The issue of self-identification as BLW is an important one. In the original analysis, we combined data from those who self-identified as following BLW and those who reported using both BLW and spoon-feeding. We have now separated these into what we have called “full” BLW and “partial” BLW. There are still some in the full BLW group who were spoon-feeding when solids were introduced, but this is a smaller number now that the partial BLW group are in a separate group (34% vs 52%). We agree, it would be very interesting to compare the food and nutrient intakes of infants who are fully adherent to BLW with a group of traditional spoon-feeders in a large study.

3. Although BLW may consume less iron, it would be interesting to know whether this persists into later infancy. Similarly, the greater level of saturated fats. Is this to do with the types of foods babies can self feed at 6 - 8 months (e.g. before the pincer grip develops?).

The wider picture also needs to be considered - if BLW are low in iron (a negative) but better at appetite regulation (a positive) then it may be beneficial for infants to be BLW but simply given iron supplementation and / or encouraged to consume iron fortified foods such as non infant cereals that are fortified with iron.

Yes, it would be very interesting to look at intake at 12 months of age, for instance, once TSF infants would also be expected to be eating family foods. By that age the pincer grip would also be developed.

We hope that this paper will encourage further studies on dietary intake in infants following BLW, and that these will measure biochemical iron status as well as iron intake. In the meantime, it seems wise to encourage the intake of high iron foods given that it is widely accepted that iron is a critical nutrient in the complementary feeding period for all infants.

We have made the following comment in the text (Lines 495-501):
“It is important that these results are confirmed in other studies, particularly studies determining iron status as well as intake so that it can be determined whether infants following BLW have poorer iron status. In the meantime, health care professionals should emphasise the importance of including iron rich food sources in infants’ diets in the complementary feeding period because of the well-accepted challenges of achieving adequate iron intake at this age, whether BLW or TSF is being followed.”

4. The high rate of infants being offered potential choking foods is concerning. However the lack of difference between the two groups suggests that following BLW does not increase this risk. Given that traditional babies ate fewer finger foods then logically this would suggest that on a ratio of food offered to frequency, these babies are being offered potential choking hazards more frequently.

As discussed above, we have now split the BLW infants into those who were partial BLW and those who were full BLW. There is still no statistically significant difference between the full BLW and the TSF groups in the odds of offering these foods, but the upper end of the confidence interval is large enough to suggest that a practically significant difference between the groups could be present in the population as a whole.

We have added the following text to the Discussion (Lines 520-526):
"It is not clear from our study sample whether the population of infants following BLW are more likely to be exposed to foods posing a choking risk. Although there was no statistically significant difference between the groups, the upper limit of the wide confidence interval for the odds ratio suggests that a substantially higher odds of exposure is possible. It is, therefore, extremely important that exposure to foods posing a choking risk, and choking prevalence, are investigated in future, larger, studies of BLW."

Ultimately, though, the most useful finding from the current study is that the majority of both BLW and TSF were offering foods that posed a choking risk, so education is needed to address this whatever complementary feeding approach is being used.

It is difficult to determine whether the TSF infants were being offered finger foods that were more likely to pose a choking risk than the finger foods being offered to the BLW infants, because many of the foods posing a choking risk could be offered as either finger foods or on a spoon. For instance, the second and third most commonly offered foods that posed a choking risk in the TSF group were small pieces of meat and corn kernels, both of which could be offered for the infant to pick up and feed themselves, or could be offered on a spoon.

5. Finally, as always, we need to consider just what the key message from BLW is. Is it self feeding? Or is it allowing the infant to regulate intake?

This is an interesting question – self-feeding by its nature maximises the likelihood that the infant has an opportunity to self-regulate intake (in terms of the amount of food eaten within the limits of what is offered). Although the infant can regulate their intake when spoon-fed it requires that they give clear satiety cues, and that the parent identifies these correctly and responds appropriately. We have tended to focus on self-feeding as it can be measured objectively- it would be much more difficult to measure infant regulation of intake.

References

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VERSION 2 – REVIEW

REVIEWER	Charlotte Wright University of Glasgow UK
REVIEW RETURNED	16-Feb-2016

GENERAL COMMENTS	This paper is greatly improved by the reclassification of the BLW children into 2 groups and it was pleasing to see that there are still sufficient numbers in the pure BLW group, and that some of the observed differences have been accentuated, with the part BLW group on the whole midway in between the pure BLW and the conventional group. The authors have clarified and simplified the analysis which makes it all easier to follow. I remain sceptical about the robustness of the energy estimates, given the fact that breast milk intake was not measured and was not varied by either the age or the weight of the child. 750mg milk per child would supply 750 * 0.67kcal = 2092 kjoules 502 calories, which is 75% of their total mean intake (2800). Thus 75% of the energy intake of the breast feeding infants – who are 100% of the pure BLW group – has been applied generally with no individual measurement. The fact that it produces very similar results on average is therefore not surprising. The estimates may be OK – but they are guesses, not measurements. This applies to all the micronutrients too – the authors cannot in fact know what intake for individual breast feeding babies was – and these could vary hugely. I accept that it would not be feasible to measure breast milk intake – but that doesn't make their crude estimate any more accurate. Therefore the tone of the discussion of the nutrient component of the paper needs to be much more provisional even than the revised version. The authors should also bear in mind that if 75% of the BLW groups' requirements are being met by breast milk it is maybe not surprising that the fat content is high and iron content low. Is BLW really a proxy for prolonged reliance on breast feeding ? So the authors also need to acknowledge that the majority of the intake in the BLW group was from breast milk not food. Incidentally the current weight and z score of the infants is never shown – was this the same for each group?

	There is much in this paper that is robust and informative – it is a pity for them to major so much on aspects that are not robust. Further comments Abstract The sentence “The confidence interval was wide so we cannot rule out increased odds with BLW (odds ratio, 95% CI: 2.57, 0.63-10.44)” is not appropriate – the CI are wide as the sample is so small – you cannot use this to imply that there is actually a large effect. Equally they embrace a possible protective effect – because the difference is non significant. This should be deleted as well as references to this later in the text. “Of particular concern is the high proportion of both groups being offered foods posing a choking risk. Further education on foods posing a choking risk appears warranted for BLW and TSF parents.” Without evidence of widespread choking in New Zealand in this age range this is given far too much prominence, particularly as the list of foods seemed to include all sorts of things we surely want children to be offered – such as pieces of fruit? I suggest rephrasing to: “A high proportion of both groups offered foods thought to pose a choking risk”

VERSION 2 – AUTHOR RESPONSE

Reviewer 1:

1. This paper is greatly improved by the reclassification of the BLW children into 2 groups and it was pleasing to see that there are still sufficient numbers in the pure BLW group, and that some of the observed differences have been accentuated, with the part BLW group on the whole midway in between the pure BLW and the conventional group.
The authors have clarified and simplified the analysis which makes it all easier to follow.

Thank you.

2. I remain sceptical about the robustness of the energy estimates, given the fact that breast milk intake was not measured and was not varied by either the age or the weight of the child.
750mg milk per child would supply 750 * 0.67kcal = 2092 kjoules 502 calories, which is 75% of their total mean intake (2800). Thus 75% of the energy intake of the breast feeding infants – who are 100% of the pure BLW group – has been applied generally with no individual measurement. The fact that it produces very similar results on average is therefore not surprising. The estimates may be OK – but they are guesses, not measurements. This applies to all the micronutrients too – the authors cannot in fact know what intake for individual breast feeding babies was – and these could vary hugely. I accept that it would not be feasible to measure breast milk intake – but that doesn’t make their crude estimate any more accurate. Therefore the tone of the discussion of the nutrient component of the paper needs to be much more provisional even than the revised version.
The authors should also bear in mind that if 75% of the BLW groups’ requirements are being met by breast milk it is maybe not surprising that the fat content is high and iron content low. Is BLW really a

proxy for prolonged reliance on breast feeding ? So the authors also need to acknowledge that the majority of the intake in the BLW group was from breast milk not food.

We have added the following sentence to the limitations section on estimated breast milk intake (lines 460-463):
"However, it must be borne in mind that breast milk provided the majority of the energy intake in the BLW group (approximately 77% of energy), and that breast milk intake was not individually measured."

We have also 'toned down' the statements about nutrient intake so that they now read (for example lines 420-421):
"However, the iron intakes of full BLW infants appeared to be even lower than those of infants following TSF …"

And we have added the following sentence to the conclusion (lines 577-580):
"It would also be useful to determine the relative importance of the delayed introduction of solids and the high breastfeeding rates seen in infants following BLW, compared to BLW itself, in determining nutrient intakes in 6 to 8 month olds."

3. Incidentally the current weight and z score of the infants is never shown – was this the same for each group?

We did not collect information on the current weight and z-score of the infants.

4. There is much in this paper that is robust and informative – it is a pity for them to major so much on aspects that are not robust.

Thank you. We have endeavoured to qualify our statements on nutrient intake as requested (see #2 above).

5. Abstract: The sentence "The confidence interval was wide so we cannot rule out increased odds with BLW (odds ratio, 95% CI: 2.57, 0.63-10.44)" is not appropriate – the CI are wide as the sample is so small – you cannot use this to imply that there is actually a large effect. Equally they embrace a possible protective effect – because the difference is non significant. This should be deleted as well as references to this later in the text.

We have carefully considered the reviewer's suggestion that we do not include an interpretation of the upper end of the 95% confidence interval for foods thought to pose a choking risk, but consider that we need to include a statement about the magnitude of the upper limit of plausible values. This is because (a) choking risk is consistently raised as a possible issue in Baby-Led Weaning (by both health professionals and parents), and (b) our sample size is small so we are not powered to detect a 'clinically' important increase in the odds in the population. We have been careful to state that "we cannot rule out" increased odds, rather than saying that the odds may be greater in the BLW group.

We have altered the description of these findings in the Results to better align with this cautious interpretation (lines 312-316):
"However, although there was no statistically significant difference between the full BLW and the TSF groups in the number of infants consuming foods thought to pose a choking risk (78% vs 58%, p=0.172), the confidence interval for the odds ratio was wide so we cannot rule out higher odds of

offering these foods in the full BLW group (odds ratio, 95% CI: 2.57, 0.63-10.44)."

And similarly altered the Discussion (lines 523-527):
"It is not clear from our study sample whether the population of infants following BLW are more likely to be exposed to foods thought to pose a choking risk. Although there was no statistically significant difference between the groups, the confidence interval for the odds ratio was wide so we cannot rule out higher odds of exposure in the full BLW group."

6. "Of particular concern is the high proportion of both groups being offered foods posing a choking risk. Further education on foods posing a choking risk appears warranted for BLW and TSF parents." Without evidence of widespread choking in New Zealand in this age range this is given far too much prominence, particularly as the list of foods seemed to include all sorts of things we surely want children to be offered – such as pieces of fruit? I suggest rephrasing to:
"A high proportion of both groups offered foods thought to pose a choking risk"

The Abstract Conclusion has been modified as suggested so that it now reads (lines 47-48):
"A high proportion of both groups were offered foods thought to pose a choking risk."

We have added the following text to the Discussion conclusion (lines 589-596):
"In the meantime, our findings suggest that families of infants following BLW should be encouraged to include a variety of nutrient dense foods in family meals, and to offer their infants foods rich in iron, zinc and vitamin B12; and that all parents, no matter what approach they take to complementary feeding, should be given advice on how to minimise their infant's exposure to foods thought to pose a choking risk - ideally by changing methods of food preparation rather than by excluding foods, many of which make an important contribution to the diet if offered in safe shapes and textures."

We have also changed the term "foods posing a choking risk" to "foods thought to pose a choking risk" throughout the paper.

VERSION 3 – REVIEW

REVIEWER	Charlotte Wright University of Glasgow, UK
REVIEW RETURNED	14-Apr-2016

GENERAL COMMENTS	I am happy with the final changes made