Author’s response to reviews

Title: Current and potential role of grain legumes on protein and micronutrient adequacy of the diet of rural Ghanaian infants and young children: using linear programming

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Author’s response to reviews:

Dear Editor-in-chief, dear reviewers,

Hereby we present you with our revised manuscript “Current and potential role of grain legumes on protein and micronutrient adequacy of the diet of rural Ghanaian infants and young children: using linear programming”, and kindly ask you to consider it for publication in Nutrition Journal.

In this revision we have addressed the concerns of all three reviewers, both in the manuscript and in a separate response to all specific comments (attached as separate documents: ‘manuscript without track changes’, ‘manuscript with track changes’ and ‘response to referees’ with line numbers referring to manuscript without track changes). We would like to thank the reviewers for their time and useful review. The reviewer’s comments have proven very valuable for improving and refining our manuscript.

We thank you in advance for considering our revised manuscript for review. We look forward to hearing from you regarding our submission. We would be glad to respond to any further questions and comments that you may have.

With kind regards,

Ilse de Jager

Response to specific reviewer comments (also attached as supplementary extra file with our responses in blue)
Reviewer #1

Reviewer 1, comment #1

I want to commend the authors on a well-designed and thorough study that no doubt provides important information to help resolve critical nutrition issues among rural Ghanaian infants and children. The authors use an old but increasingly popular (in the field of nutrition) statistical technique (linear programming) to help solve a complex nutritional problem. This methodology leads the authors to conclude, that although an increase in grain legumes on top of the current rural Ghanaian diet did help with micronutrient inadequacies, it did not solve them. Thus, suggesting that other interventions are also needed. My relevant experience for this review is mostly in the use of linear programming to devise FBRs, therefore, I will focus my comments on this section. From this perspective, I would recommend that this study be published and have found no major concerns during my review. My comments and feed-back are minor and for consideration only. Tables are clear, well-designed and for the most part well referenced.

Thank you for taking to time to review our paper, especially on the linear modelling part. We are happy to hear that you agree with our methodology and to receive your valuable comments that we took into consideration, see our specific responses below.

Reviewer 1, comment #2

If word count permits, I would suggest creating a limitations section to the paper. Some limitations are mentioned already but are not explicitly identified as such. For example:

* line 425: If I understand correctly, prevalence of stunting in your groups did not correlate with insufficient EAA intake. Could this be an issue with estimates of EAA intake? Over-reporting? Possible role of inadequate energy? Micronutrient intakes? A limitations section could provide possible explanations

We indeed did not include a separate paragraph that discusses the limitation. Instead discussed the different limitations in relation with each specific topic separately. We indeed suggested that our findings show that there is no correlation between EAAs intake and stunting, however we have deleted this from the discussion (see also comment #33, reviewer 3) but this is discussed in the paragraph prior to this paragraph (line 422-424, line numbers indicated refer to the revised manuscript without track changes). The important limiting issues you raise are discussed in the paragraph before so we did not include these again in this paragraph specifically.

Reviewer 1, comment #3

Please indicate how daily costs were obtained or estimated. This information could simply be referenced in Additional file D.
We added this to Additional file D as a footnote (‘Daily diet cost per child were calculated by summing the price of each quantity of a food consumed per child, using the average price per edible 100 g portion (prices were collected from three different food sellers in the area’). Also see comment #7 from reviewer 2 below.

Reviewer 1, comment #4

What was set as the objective function in linear programming? Was it to minimize deviation from current diets? I suggest you include this information in the manuscript.

Optifood uses different objective functions, in module 2 it minimizes the deviation from the current diets while reaching nutrient goals using goal programming and in module 3 in the first run it minimizes each nutrient individually and in the second run it maximizes each nutrient individually. We explicitly added the objective functions, see line 239–240 and 245–246).

Reviewer 1, comment #5

The second sentence in the Abstract (background) should be modified to improve clarity. Suggestion: "They offer the largest potential benefits in emerging economies where future food demand is increasing and both under nutrition and overweight co-exist."

Thank you, adjusted.

Reviewer 1, comment #6

Line 201: Why were fortified foods excluded?

We specifically aimed to develop FBRs based on locally available foods and did not want to support specific brands of fortified foods.

Reviewer 1, comment #7

Line 205: Why was the 5th and 95th percentile selected? I assume this is because this is often seen in the literature; however, it is rarely explained. Being explicit and describing why this was chosen is recommended.

There is no golden standard which criteria to choose. Unpublished (under review) sensitivity analysis from the rural area in Kenya have shown that using the 10th-90th percentile instead of the 5th-95th percentile doubled the number of problem nutrients. Therefor we have chosen the 5th and 95th percentile and also to allow comparison with other studies using the same criteria.
Reviewer 1, comment #8

Line 210: For more clarity, suggest listing in a table the final nutrient constraints used in the linear modelling program. Why were EARs not used? Or rather, why wasn't 70% of the RNI used as the actual constraint in the model vs 100% of the RNIs?

Similar to other studies that developed FBRs using Optifood, nutrient goals were defined as 100% of recommended nutrient intakes (RNI) and 70% of RNI was used as cut-off for the final selection of FBRs. Prevalence of inadequacy is expected to be low when intake can be reached at or above the RNI (Trumbo, Barr, Murphy, & Yates, 2013).

Reviewer 1, comment #9

Line 219: I liked the author's use of a 5% bioavailability for iron. However, a reference is required for this. Why 5%? Line 220 provides a reference for zinc bioavailability.

We have added the reference. Also see comment #16 from reviewer 3 for adjustments with regard to zinc bioavailability.

Reviewer 1, comment #10

Line 222: How were diets deemed realistic?

For each nutrient, extreme diets are generated for each nutrient (maximal and minimal protein for example) and these need to be checked if these are feasible among the target population (are they consumed as such). We added to lines 235-237: ‘Module 1 was run to check that model parameters generated realistic diets that are feasible for the target population. Module 1 generates 19 different diets including poor, middle and nutrient rich diets and shows the energy range of these diets and a high range is preferred as this shows flexibility of the model.’

Reviewer 1, comment #11

Why in additional file G are nutrients below 100% of RNI highlighted? I thought your target was 70% of RNI to be at least consistent with an EAR (line 236).

We defined problem nutrients as nutrients below 100% of RNI as they do not cover the needs of all children and we want to model what FBRs are needed to increase intake of these nutrients. When even in the maximum optimized diet 100% RNI cannot be reached, it means that nobody in the target group will be able to reach the RNI. We expect 70% of the RNI will be sufficient for the whole target group when the less nutrient dense foods will be chosen (in the worst case scenario). But when the most nutrient dense foods are chosen, 100% RNI should be reached.
Reviewer 1, comment #12

Thank you for the opportunity to review! I hope that the next steps of this research involve testing additional interventions to improve micronutrient adequacy among infants and children in rural Ghana.

Thank you for your valuable comments. We continued with testing additional interventions although this was limited to linear modelling to test different specific agricultural interventions. We are unfortunately not in a position to test these experimentally.

Reviewer #2

Reviewer 2, comment #1

The authors describe a study in which they examine the role of legumes in meeting dietary needs. This manuscript is generally well-written and presented in great detail, and the analysis is rigorous. Yet there is so much information and analysis being presented in this paper that sometimes the larger key issues are not well explained - primarily, it's not clear what the central comparisons are. Are the authors comparing between groups, within groups, or between models of the same group? The authors need to be very clear about what the main comparisons are, why they are being compared, and how they are being compared. As it is, these explanations do not come through clearly in the manuscript, and as a reader I found myself losing the central parts of the study while I was sifting through all of the detail.

Thank you for taking time to thoroughly review our paper and your valuable comments. With regard to your main general comment, our main aim was not necessarily to compare groups. We analyse different age groups as this is recommended in developing FBRs because the age groups differ in nutrient requirements. Our target group is children 6 to 23 months old and our main objectives (as listed in the introduction) are: (a) identify the consumption and contribution to nutrients of grain legumes in the current diet, (b) identify a set of FBRs to best cover nutrient adequacy within the constraints of local current dietary patterns, and (c) evaluate whether including extra grain legumes additional to what is normally consumed would improve adequacy of problem nutrients which are present in relatively high concentrations in grain legumes (protein, EAAs, calcium, folate, iron, niacin and zinc). Nevertheless, we agree that the different groups (resulting in four times the number of results) is confusing for the reader. We have made adjustments to the manuscript that we think increased clarity of the central parts of the study, see replies to your specific comments below.

Reviewer 2, comment #2

Introduction. Well-written and clear. But acronyms appear to be over-used, which reduces readability. Can the authors please be sparing in their use of acronyms? Examples: EAA, LMIC, IYC, FBR.
We agree that acronyms should be used sparsely. We have replaced LMIC with ‘developing countries’ throughout the manuscript. However, we did not removed EAA, IYC and FBR as they are central in this study (and also common in other nutrition literature) and we are convinced this makes the manuscript more concise.

Reviewer 2, comment #3

Introduction. Can the authors please review the Global Burden of Disease publications to (e.g., Mozaffarian et al.) to see if they have published any studies on legume consumption in Africa?

Thank you for your suggestion. We have checked these publications but we find they report legume consumption only for men and women above 20 years old and therefore we did not include these reference in our manuscript (see https://www.sciencedirect.com/science/article/pii/S2214109X1470381X#sec1 & https://ars.els-cdn.com/content/image/1-s2.0-S2214109X1470381X-mmc1.pdf)

Reviewer 2, comment #4

Introduction. Since the authors focus specifically on grain legumes, please provide a clear distinction between grain legumes and other types of legumes. Are grains legumes a distinct type of legume? If so, please provide a rationale for why the authors are focusing on this specific type of legume at the expense of all others.

Grain legumes are cultivated specifically for their seeds for human food and animal feed, although the leaves of some grain legumes (e.g. cowpea) are also consumed by humans. Grain legumes are specifically the legumes that are the most important as human food. We added this definition to the manuscript as a footnote (referred to in line 51, line numbers indicated refer to the revised manuscript without track changes).

Reviewer 2, comment #5

Methods. At the outset of this section (or perhaps the "Subjects" subsection), it would be helpful to briefly mention the different groups. Otherwise, it can be confusing when the authors state, for example, that there was a breastfeeding group, since this was not mentioned previously as a rationale for the study. Also, it's not clear what is meant on lines 120-121. Briefly mentioning the study groups early on would help to clarify these things.

We added a first sentence to the ‘subjects’ section that introduces the four different groups, see line 111 to 114. For more clarity we specifically mention the ‘four groups’ in line 117-119 (the lines you indicate as not being clear, see reply to comment #6 about the sample size calculation).
Reviewer 2, comment #6

Lines 120-121: It's quite unclear how the sample size was arrived at. Why didn't the authors use a standard power calculation?

We used a standard sample size calculation based on what was previously used in literature with other linear programming techniques (see reference given of Santika et al. 2009). We assumed that the estimated serving sizes of foods commonly consumed (and used in developing FBRs) by our study population are within 10% (95% CI), assumed an SD of 50% of the mean serving sizes and allowing for a 5% rate of attrition. We added a sentence, see lines 129-130 and also comment #13 from reviewer 3.

Reviewer 2, comment #7

Lines 126-127: This is the first time food prices are mentioned, so it's unclear why the authors are collecting these data. Please clarify in the text why this is an important measure. This will also help to clarify lines 158-159.

Thank you for the comment, see additional sentence in line 135-136. Also see comment #3 from reviewer 1 above.

Reviewer 2, comment #8

Line 142: Were the days randomly selected or evenly distributed over the week? Please clarify in the text.

Days were evenly distributed, see correction in line 150-151.

Reviewer 2, comment #9

Lines 146-151: Please clarify in the text. What does it mean that there are duplicate amounts? Amounts of what? Also, please specifically clarify in the text what is meant by lines 148-151 - it's not clear what is being done here and why.

This is commonly done when conducting a quantitative 24-hour recall. It is assumed to be the best measure possible of the quantity of a food consumed the previous day. For example, if maize was reported to be consumed the day before and maize is available in the household the next day during the interview, the mother or caregiver of the child that prepared food the day before was asked to show the amount of maize used using her own cooking pots and pans. Some of the foods reported to be consumed the previous day might not be available in the household during the time of the interview and in that case other measures were used to estimate amount consumed. We adjusted the formulation for more clarity, see lines 155-162. The measures are also described in the reference given: see Conway et al. (2003). We decided not to add detailed
explanation for each measure in the methodology because this is ‘common’ for conducting quantitative 24-hour recalls and also to keep the method section concise.

Reviewer 2, comment #10

Line 156-157: Please clarify why it is needed to convert all of these units. Why do the authors need to convert monetary values to consumption amounts? And what is a weight-to-weight measure? What is a household unit? Please clarify in the text.

See reply to comment #9 above. To estimate nutrient intake, you need the grams of food consumed. When investigating quantities of foods consumed and foods cannot be weighed if not available in a household, we need other measures. These measures need to be converted to grams to be able to estimate nutrient intake with the help of a food composition table. For example: if in case the food is not available in the household, a mother/caregiver may be able to recall and report the price paid for the amount of food consumed, and we need to convert the amount paid in the weight of the food consumed. This is also valid for other measures mentioned. Weight-to-weight is: ‘compared with the weight of other foods (e.g. amount of sugar estimated with weight of same volume of corn flour)’ (see lines 159-160). A household unit are units such as a spoon or bowl (see line 161). We decided to provide a summary of the method and a reference if the reader wants to know more details (see also comment #9 above).

Reviewer 2, comment #11

Line 167: Since the sample is Ghanaian, why is the Ghana FCT the least preferred in this list?

Thank you for this question. The Ghanaian FCT was the least preferred as it is from 1975 with limited foods included and out-of-date measures are used (methods used to measure composition, limited sample sizes). See lines 177-178.

Reviewer 2, comment #12

Line 194: What four groups? The authors still haven’t made clear what these groups are.

See reply to your comment #5 above, we introduced the four different groups in lines 111 to 114. For more clarity, we in short specified the four groups again here, see line 198.

Reviewer 2, comment #13

Figure 1: How is it possible that, in the third group (Eligible for 12-23mo, BF), there were 100 subjects randomly selected by 114 provide 24HR?
As during the 24-hour recall it appeared that (1) a number of children reported to be non-breastfed did actually receive breastmilk during the 24-hour recall and (2) some children appeared to be older than reported in the census – we finally analysed more children in this age group. In addition, we oversampled 4 extra children in this group because we expected based on our previous experience we had to exclude more children because of misinformation of the census.

Reviewer 2, comment #14

Line 200: Why did the authors use only data from the first day of recall? They went through great lengths to collect a 2nd day of recall data from 20% of the subjects and adjusted for the intra-individual variation, which is crucial - so it's unclear why they didn't include the other day of recall. Please clarify.

We used the second recall to indeed adjust for intra-individual variation for estimation of habitual nutrient intakes of our study population and the contribution of current legume intake to habitual nutrient intake. This was one of our main objectives (see final paragraph of introduction). We did not use second recalls in the Optifood analyses to develop FBRs because we only did a second recall for 20% of the children. Including a second recall for only 20% of the children could introduce a bias as intake of these 20% of children would have a proportionally higher influence on the results. An unpublished (under review) sensitivity analysis from the rural area in Kenya also showed that using second recalls for all the children rarely changed FBRs.

Reviewer 2, comment #15

Results. OK, so we have our four groups here. But it's still not at all clear which groups are being compared. Lines 194-196 state that the comparison is between children who did and did not consume legumes, but that distinction is not made in any of the four groups listed here. Within each of these groups, did some children consume legumes and some did not, so the comparison is within each of these groups? Or is the comparison across each of these groups? Please clarify throughout the text in key places, specifically in the Methods section. The comparison should not be across groups, namely because of different age groups.

We have made adjustments accordingly throughout the method section (see replies to your other comments above) to improve clarity on what the target population of this study is and how and why the population is further divided into the four different groups. We did not compare across these four groups in the sense of being statistically different yes or no (for the reason you state that these are indeed different age groups). But we do need to show our results for each group separately and hence we discussed the results for each group.

We added an extra comparison between children consuming legumes and children consuming no legumes, to further look into our main research question with regard to legume contribution to the current diet. This was not done within each of the four groups but all children 6-23 months
were divided into 2 groups based on the criteria whether they consumed any legumes yes or no. We agree this was not clearly formulated as such, see adjustments in lines 206-208.

Reviewer #3

Reviewer 3, comment #1

The strengths of this study are the high-quality dietary data collected from a relatively large sample of children in Northern Ghana; and the authors modelled essential amino acids in addition to micronutrients, which is a unique contribution to the linear programming literature. However, the introduction needed strengthening, including recognition of study weaknesses. Please check the interpretation of references; and consider including a reference by Suri et al (2014), FNB 35:372-81 that models protein quality, using linear programming analyses, for legume and cereal-based complementary foods in Ghana. From my perspective, there were also a few issues with the analyses, and the authors need to fully address the apparent discrepancy between their findings on protein quality and quantity and the literature presented in the introduction. The manuscript would also benefit from a thorough editorial review.

Thank you for taking the time to thoroughly review our paper, your very valuable comments and the literature suggestion of Suri et al. We have addressed the above points raised, see below for specific replies and the revised manuscript. One of our authors is a native English speaker and has gone thoroughly through the manuscript once more.

Reviewer 3, comment #2

Lines 25-26 - delete "largest" and "most strongly" because legumes will offer the benefits to all societies not just emerging economies and future food demand increases are not necessarily the strongest in emerging economies.

Thank you, we agree, see adjustment in line 23-24.

Reviewer 3, comment #3

Lines 32-33 - reword these sentences slightly i.e, delete "best". The modelling process does not guarantee it's the "best" FBRs to cover nutrient adequacy, because the selection of final FBRs is somewhat subjective. Also - it should be "linear programming analysis" not "linear modelling"

Thank you for your comment, see adjustments in lines 30-31. We also replaced modelling by programming throughout the manuscript.

Reviewer 3, comment #4
Line 35 - indicate if its 20g per day of cooked or uncooked legumes; and if uncooked whether they are mature/dry or immature/fresh.

Thank you for your remark, the 20 gram refers to cooked legumes (see line 34).

Reviewer 3, comment #5

Line 60 - is maize the most commonly consumed staple globally? Rice? Perhaps reword to "one of the most commonly....." or "a commonly consumed...". Similarly, in lines 72-73 reword to "one of the highest prevalence...". Many non-African countries have high prevalence rates of stunting, especially South Asia; for example, India, Nepal, Afghanistan and Pakistan.

We have included both adjustments, thank you (see line 57 and line 70-71).

Reviewer 3, comment #6

Line 66-67 - reword this sentence, as the meaning is not clear.

We have adjusted accordingly to suggestions for similar sentence used in abstract (see lines 63-64).

Reviewer 3, comment #7

Line 68 - should this be "production" instead of "productivity"?

This should be productivity, the production is relatively high.

Reviewer 3, comment #8

Lines 70-78 - rewrite this paragraph in a more nuanced fashion. The evidence presented is based on weak data. For example, evidence that protein intakes are inadequate in Africa, is not based on estimated dietary intake data but instead food disappearance data in relation to the protein requirements of an adult man. These weaknesses can also be considered when interpreting the results from this study in the discussion.

Thank you for your comment. We added a sentence to indeed indicate the existing evidence is weak (line 69). We have adjusted the remaining paragraph as well, see comment #9 and #10.

Reviewer 3, comment #9
Line 77 - reference 22 is a commentary not a description of a trial; and reference 23 only describes a study that will be done it does not present results from the study.

The two references indeed do not present the results of the trial conducted, one describes the study and the commentary mentions the preliminary results of the trial. We referred to these two references as the results of the trial are not published yet. We have added this explicitly, see line 73-75.

Reviewer 3, comment #10

Lines 78 and 89 - check interpretation of the references. For example, does reference 16 show a decline in the estimated prevalence of inadequate micronutrients in sub-Saharan Africa or instead show a decline in micronutrient densities. These analyses also used food balance sheets rather than consumption data; please modify if necessary.

We checked and Beal et al. (reference number 16) do show a decline in prevalence of inadequate micronutrient intakes, not in micronutrient densities. They indeed estimated a mean intake per capita for each micronutrient based on food balance sheet data and estimated a population distribution based on within-subject variation from published dietary intake studies. We specially added that these results were based on food balance sheet data (line 75-77). Results from Mesfin et al. and Abizari et al. use consumption data, they used cross-sectional surveys, one included one day weighed food record and the other study qualitative dietary intake data. Therefore we did not modify this section. We added the study results of Suri et al., thank you for this suggestion (see line 83-87).

Reviewer 3, comment #11

Line 97 - reword the second objective slightly to -"… identify a set of FBRs that will improve…” because "best" is difficult to define.

Agree, adjusted (see lines 99-100).

Reviewer 3, comment #12

Line 98 - reword from "improve adequacy" to reduce "the number of ", because by definition "problem nutrients" are inadequate.

Thank you, adjusted (see line 101-103).

Reviewer 3, comment #13
Lines 120-123 - unclear as written. Reword. Also - 100 children were not selected for each of the 4 groups as stated (lines 114-15). The sample size for the non-breastfed children is very small and well below the specified criteria of 100 children. Consider excluding the results for these 29 children. For linear programming analyses, the number of foods in the food lists will be low and food portion sizes will not be estimated robustly. These factors will limit the comparability of the results of this group to the other groups.

-We added the following to the description of the sample size calculation: ‘This sample size is comparable to those previously used in studies with linear programming techniques in the literature (Santiga et al, 2009).’ (see line 129-130)

-Indeed not for all groups 100 children were selected, see addition in line 120-121. There were insufficient children in the non-breastfed children 12-23 months group.

-We agree that the sample size is indeed well below the specified criteria. However, as actually all the children from this group are included (all the non-breastfed children 12-23 months in this district) we do think the foods and portion sizes are realistic estimates. We added this point to the discussion, see line 416-419.

Reviewer 3, comment #14

Line 143 - state what will be adjusted for day-to-day variation.

Thank you, we have added: ‘day-to-day variation of nutrient intakes’ (see line 152).

Reviewer 3, comment #15

Lines 191-193 - for children under 12 months of age, with a few exceptions (for which there is an EAR), nutrient recommendations are Adequate Intakes. Population level dietary adequacy should not be assessed by calculating the percentage of children below the AI, as the AI could be well above actual requirements.

We completely agree. We decided to present the results for micronutrients as median intake [IQR], add RNIs (as also used in Optifood for all groups) and only when EARs are available to show % of children with inadequate intake, see Table 2 for adjustments. Therefore we adjusted the methods (lines 202-204), results (lines 298-303) and discussion (lines 458-461 and deleted results with regard to micronutrient adequacy for children under 12 months of age).

Reviewer 3, comment #16

Line 220 -please check and correct - as far as I am aware 15% absorption is not assumed by iZiNCG for an unrefined diet.
Thank you for your comment. The 15% absorption rate is indeed not mentioned by iZiNCG but by WHO/FAO 2004 (vitamin and mineral requirements in human nutrition). However, we used the RNIs from iZiNCG for unrefined cereal-based diets in our analyses (5 and 3 mg/day for 6-11mo and 1-3 years, respectively). See adjustments in lines 230-232.

Reviewer 3, comment #17
Line 222 - change verb tense "meet" to "met"
Thank you, adjusted (line 237).

Reviewer 3, comment #18
Line 227-228 - modify - module 3 is maximizing and minimizing nutrients not diets.
Adjusted (lines 242-245).

Reviewer 3, comment #19
Line 235 - was a cost constraint introduced or was this merely one of the criteria used to select the FBRs?
It was merely a criteria that was used to select FBRs. We have added reference to Additional file D to make clear we used it as a criteria for selection (line 254-256).

Reviewer 3, comment #20
Line 252 - typo - should be 75th not 75ht
Corrected (line 269).

Reviewer 3, comment #21
Line 278 - breast milk intakes were not quantified so it is not possible to definitively state energy intakes were below daily requirements. Perhaps these children had above average breast milk intakes? On the other hand, the high percentage of wasted children indicates energy intakes were inadequate, which would strengthen the point. Reword.
Reworded (line 294-296).
Reviewer 3, comment #22

Line 280-283 -for children under 12 months of age the percentage at risk is over-estimated i.e., the percentage below the AI where the AI is an intake that likely exceeds requirements. Percentages at risk should not be estimated for the younger children. Instead compare nutrient densities to desired nutrient densities for all breastfed children to identify nutrients that are likely low in their diets; which will also allow interpretation across age groups.

Thank you for this suggestion. We looked into reporting nutrient densities but as far as we understand this will still mean that you would use the AI/RNI related to recommended energy intake and compare this with actual intake related to actual energy intake – so then we still overestimate the percentage of children below 12 months with inadequate intake (using AI and not EAR) and face the similar ‘problem’ with comparing across age groups. Instead we decided to add RNIs (as also used in Optifood analyses as well) next to median intake and only show % of inadequate intake when EARs are available. See our reply to your comment #15.

Reviewer 3, comment #23

Lines 307-309 - as far as I can tell these analyses did not control for age or breastfeeding status. Older children were more likely to consume legumes and would have higher energy and nutrient intakes, which makes it essential to control for these factors in the analyses.

As the data is not normally distributed, also not after transformation therefore we used the Mann-Whitney U test to compare the two groups (children who consumed legumes with children who did not). It is not possible to control for age or breastfeeding status when using this test. Therefore we checked the same comparison separately for each age group and separately for breastfed and non-breastfed and found similar results. We have added this to the footnote of Table 3 and to the results (see line 329-330).

Reviewer 3, comment #24

Line 314 - delete the first line as it is not necessary i.e., it is not a result.

Sentence deleted.

Reviewer 3, comment #25

Line 319 - there seems to be some mixing of terminology for "best" diets. Module 2 were the nutritionally "best" diets and module 3 were "best-case scenario" diets. Change "best" diets to either "maximised" or "best-case-scenario" diets for module III.

Thank you for your comment, we adjusted the terminology in this paragraph (lines 335-347), as well as throughout the manuscript. For module 2 we use ‘best optimised diets’ and for module 3
we use ‘maximised diets’ (instead of best-case scenario) and ‘minimised diets’ (instead of worst-case scenario).

Reviewer 3, comment #26

Line 322 - were the "worst-case scenario" diet values for thiamine or vitamin B6 >70% when run with no FBR? If yes, then its okay to substitute the amino acids for these nutrients. If no, then either test the FBRs selected, for these two micronutrients, to ensure they are above 70% in the worst-case scenario analyses or choose alternative micronutrients to substitute, for example, vitamins A and C for the breastfed children.

Thank you for your comment and suggestions. Not in all four groups both thiamine and vitamin B6 are >70% of RNI when run with no FBR, they do are >100% RNI in best-case scenario. We tested the selected FBRs for these two nutrients and in all cases they are above 70% in worst-case scenario, except for vitamin B6 for breastfed children 6-8 months (49.4%). We agree that these nutrients should be above 70% in worst-case scenario with selected FBRs and otherwise it might be better to replace other nutrients for including EAAs in analyses. However, we wanted the final set of FBRs to be comparable across groups and therefore similar nutrients to be included in the analysis (there were also no other nutrients that reach 70% RNI in worst case scenario in all age groups). We excluded thiamine and vitamin B6 on the basis that they are above 100% RNI in best-case scenario in all four groups (not problem nutrients). See reformulation in method section (line 248 - 251) and results section (line 343).

Reviewer 3, comment #27

Table 5 - state whether it's the number of servings per day or per week.

Thank you, we added this important information to the table title and in the table itself.

Reviewer 3, comment #28

Line 364, 375 and 395 - typos? Should be IYC?

Corrected throughout the manuscript.

Reviewer 3, comment #29

Line 395 - missing a "were" ie, …products were….

Corrected.
Reviewer 3, comment #30

Lines 399-401 - are EAA at stake based on the results of the study? Consider modifying this statement. Also - do the authors have any insights into why the results from their study differed from others? Were legumes consumed by children in the other populations?

Thank you for the comment. We rephrased the statement as indeed this is not what we conclude based on the results of our study. We also added an extra sentence with regard to differences between our study and others with regard to measuring EAAs intake (see lines 421-428).

Reviewer 3, comment #31

Lines 402-411 - for me, the interpretation of the results in this section - at least as worded - assumes the results, for protein, were incorrect. Even though its true, protein requirements may be elevated because of malabsorption, etc, this also applies to micronutrients. Based on the results of this study i.e., a high percentage of children had inadequate intakes of micronutrients but generally adequate intakes (both quantity and quality) of protein, would not the interpretation be that they are at high risk of micronutrient deficiencies but are only at risk of inadequate protein intakes if requirements are elevated? A critical discussion of these results in relation to the protein literature presented in the introduction would strengthen the discussion.

Thank you for your comment. We agree that indeed we can conclude that if requirements are indeed elevated than our population is at risk of inadequate protein intakes (added to line 469). In this specific section we discuss protein and not micronutrients intake (in micronutrient paragraph as well as conclusion we state that our population is indeed at high risk on micronutrient deficiencies and not for protein). As suggested by some other comments as well, we emphasized limitations of current studies in the discussion and added results of Suri et al (lines 421-428).

Reviewer 3, comment #32

Line 419 - for lysine the % below daily requirements was only 17%. If the daily requirements are equivalent to recommended daily allowances, then 17% is an over-estimation of the percentage at risk of low intakes; so delete lysine from the list of AA which are a concern for the non-breastfed children.

We agree and indeed deleted lysine from this list.

Reviewer 3, comment #33

Lines 423-425 - was the percentage of stunted children statistically higher in this group than the other groups? If not, then delete this statement. The sample size of non-breastfed children was very small i.e., this apparent difference might merely reflect sampling
We agree, especially as there is no difference between breastfed children 12-23 and the non-breastfed in prevalence of stunting. We deleted this statement.

Reviewer 3, comment #34

Lines 460-463 and lines 491-492 - consider rewording, because the cut-off of 70% in the worst-case scenario analyses is an arbitrary choice i.e. cannot state it does not improve adequacy. Instead, these FBRs did not achieve the criteria selected to define a low risk of inadequate intakes for all children in the population.

We reworded as suggested (line 480-482).

Reviewer 3, comment #35

493 - delete "more-or-less"

Deleted.

Reviewer 3, comment #36

Line 590 - check this reference. It does not appear to be correct as written.

Adjusted.

Reviewer 3, comment #37

Table 2 - were the percentage at risk of inadequate iron intakes estimated using the full probability approach? Also - what percentage absorption was assumed for iron - 5% was used for modelling?

We did not calculate the probability of adequacy in this paper because (as also pointed out in your comment #15 and #22) for children 6-11mo for most nutrients we cannot calculate probability of adequacy. For actual intakes also 5% absorption was assumed and for zinc also unrefined cereal-based diets. Added as footnote in both Table 2 and Additional file B.

Reviewer 3, comment #38

Table 3 - the SD are high. Should the contribution of legumes to energy and nutrient intakes be presented as medians and inter-quartile ranges instead of means given the high SDs? Also, for the daily legume intake estimates, specify in the footnote whether these intakes are only for consumers or for all children?
We included all children, also the children not consuming legumes. Therefore medians are mostly close to zero (as 40% of children are not consuming legumes). We added to the footnote of Table 3 that daily legume intakes are for all children.