Author's response to reviews

Title: Dietary intakes in people with irritable bowel syndrome

Authors:

Elizabeth A Williams (e.a.williams@shef.ac.uk)
XuiLi Nai (nai_xuili@hotmail.com)
Bernard M Corfe (b.m.corfe@shef.ac.uk)

Version: 2 Date: 26 November 2010

Author's response to reviews: see over
Dear Editors,
Thank you for the opportunity to offer a revised manuscript and for the careful assessment of your reviewers. We have set out in detail below the changes we have made to our manuscript in response to these comments and have highlighted the changes in the revised uploaded submission.
Yours faithfully,
Bernard Corfe

**Reviewer’s report 1:**

The introduction is long and detailed, the information given is well known for readers of gastroenterological journals. The introduction would have been more suitable in a nutritional journal read by nutritionists. I recommend a shortening.

The introduction has been shortened to exclude superfluous information known to readers of gastroenterological journals, whilst aiming to maintain a general readability and accessibility for those identifying the paper through other routes.

The “results” section needs a revision. The text uses the words: similar, below, exceed, far exceed, higher, lower etc. I want to know if the differences were statistically significant. In more detail – on page 6: Only male subjects showed a significant difference in BMI across IBS subtypes (p=0.036, data not shown). I find the data in table 1.

Were differences are statistically significant this has now been made clear in the text and the tables.

Table 2 is a very detailed description of the dietary intake divided between sex and IBS-subgroups. It might be ok, but if possible, try to simplify it. And make it clear also in the legend to the table that there were no statistically significant differences between the groups.

The data contained in Table 2 has now been split into 3 separate tables: Comparisons of intakes with Dietary Reference Values (Table 2), Intakes according to IBS sub-type (Table 3) and the Comparison of intake with the UK National diet and Nutrition Survey (Table 4). The tables have also been simplified with the removal of data according to both gender and IBS subtype. Were statistically significant differences exist these have now been made clear.

Figure 1 is not one figure but 16, and figure 2 is 10 figures. I do not recommend publication of these figures. Except for the reference values (RNI and NDNS), the results in the figures are also given in table 2, the only difference is that in table 2 the results are given as mean with SD and in the figures as median with percentiles. If the authors want to refer to RNI and NDNS for energy, macro- and micronutrients, fruit and vegetables dived into groups for sex and IBS subtypes, please find another way to do it – probably a table.

As recommended the reference to RNI and NDNS data has now been presented in table format. The supplementary figures have been removed.

More important for me is to know if the intake is statistically significantly above or below the RNI and NDNS.

This analysis has now been done and comparison with RNI and NDNS can be found in Tables 2 and 4 respectively.

The most important information is probably the proportion of subjects below the recommended limits and therefore in danger of deficiency disorders, which is not mentioned at all.
The key point of the paper is that distributions of intakes are not adverse in this population by comparison with national statistics.

I also lack information about the relation between the diet and comorbidity (e.g. psychiatric disorders), food intolerance, degree of symptoms, ethnicity, etc. The paper mentions trigger foods, have trigger food (like wheat and lactose) been registered and how did trigger food influence the diet? I hope(expect that some of this information could be added.

We had not collected information on comorbidities or intolerances, however data on symptom severity were collected as part of this study and have been included in the baseline characteristics. We have also reclarified the exclusion criteria to highlight that subjects were not taking IBS medication or probiotics or prebiotics when the data was collected.

Figure 2 is not mentioned in the text, and abbreviations (like RNI) should be explained before taken into use.

The figures (supplementary material) have now been removed. Abbreviations are now given in full at the request of reviewer 3.

The discussion has to be revised based on the new way of presenting the results.

The results section has been substantially reedited for the new presentation and the discussion has been modified accordingly to take into account these and other referees' comments.

**Reviewer's report 2:**

1. What are the main aim(s)? To analyse for differences between subgrous and gender or to compare to the general population and recommendations? (methods vs results).

   The Introduction section has been edited to include a clear statement of hypothesis.

2. The aims of the study were to compare IBS patients to dietary recommended values and population intakes, as well as assessing by gender and IBS subtype. There are however no hypothesis or discussion about why there should be differences in the dietary intake between gender or IBS subtype.

   The Introduction section has likewise been edited to include a justification of the hypothesis offered.

3. More information about the NDNS, i.e. method used for measuring dietary intake, basic demographics.

   More details on the NDNS have been added in the methods section.

4. Were the controls (NDNS) matched for gender, age, BMI...?

   NDNS data are taken from a representative sample of 19-64 year olds. NDNS dietary intakes are now presented in Table 4 according to gender and the BMI of the NDNS population in given in results section.

5. BMI of the IBS subjects compared to the healthy population (NDNS)?

   The information has now been provided in the results section.

6. The FFQ used (before modification) was found to give higher values for most nutrients than other methods (according to ref 17). Could this explain the results?

   Yes, possibly. This is explored as a possibility in the revised Discussion.
7. No statistics used when comparing IBS subjects and NDNS as well as recommended values.

This has now been rectified. Statistics are presented in the revised tables and the text. Some of the data show highly significant results in the study vs. NDNS comparison.

8. A very few number of men, especially when presented by subtype (type A n=7, type C n=5, type D n=11), is it meaningful dividing only 23 male subjects into subtypes? Statistical analysis in 5-11 subjects?

We thank the reviewer for this valid point. Presentation by subtype and gender has now been removed.

9. No p-values in tables, only a few in the text (BMI in male subtypes).

This has been corrected.

10. Describe the results from previously done studies on dietary intake in IBS subjects using 3-day dietary record or dietary recall. Did the FFQ method present other/devian results?

The FFQ, in line with other approaches, highlighted the generally good diet of IBS subjects.

11. Exclusion criteria: taking medication for their IBS. Any medications? For how long? Does this mean that the IBS subjects were relatively healthy IBS patients? Could this explain the results?

See above for the response to reviewer 1 – the subjects were not medicating when the data were collected. Data on symptom severity has now been included in the paper. Symptom severity for most subjects falls into the “moderate” category.

12. At page 7 and 8 the authors speculate about the higher intake of selected micronutrients in the IBS subjects compared to reference nutrient intakes and NDNS. This is not results, should be a part of discussion.

The results and discussion have been modified substantially now.

13. Page 8: Presentation of the intake of fruit and vegetables and alcohol – why revise this when it was not part of the aim(s) of the study and when suggesting that the high intake in the IBS subjects was a consequence of over-reporting. Alcohol could/should be presented as macronutrients.

Alcohol intake is now presented as % energy derived from alcohol alongside the macronutrients. Intake of fruit and vegetables have been removed.

14. The authors describe in the background about IBS-subjects to restrict their food intake of eliminate provocative dietary agents as lactose or wheat. References or speculations?

A reference has been cited to reinforce this statement, and typical trigger foods have been cited.

15. Explain o and * in figures.

The figures have been removed at the recommendation of reviewer 1.

**Reviewer's report 3:**

Dietary Assessment: I think it would be a good idea if you gave a bit more information on the EPIC FFQ. We are told that the (modified) questionnaire asked people to report their habitual intake of 134 food types over the previous 12 months. Why was it modified? In what way was it modified? What sort of questions, or statements, are asked in this questionnaire? Without obtaining the original reference, the reader does not know. Do you ask about exclusions? The abstract states that “relatively few studies have undertaken a dietary assessment in IBS sufferers to examine the wider impact of dietary exclusions” which seems to
suggest your study would be doing this, but although there is a brief mention of this at the end of the discussion, this has not been addressed explicitly. Perhaps you could add more about this?

More information has now been included in the methods section on the EPIC FFQ. We have clarified the purpose of the study, which was to assess global diet rather than to study trigger foods, in the Introduction, however the observation of a belief in dietary causes has led to the hypothesis formation.

Likewise, “the FFQ data were entered into SPSS…..living in Newcastle” – The reader will not be able to understand what has been done without reading the original references. Just a bit more information on this would be beneficial to the reader.

Dealt with in the revised methods section

Statistical analysis: You have used a criterion value of $p<.05$ – did you consider adjusting for multiple comparisons? I think you should defend this in the article. I’m a bit confused about the statistical analysis and results. You say you used ANOVA and unpaired t-tests, but I can’t see any t-values, F values, or p values in the results sections. On page 6 line above “Energy and macronutrients intake of IBS Subjects” you say “$p=.036$ data not shown” – here I would expect to see t or F values etc. The results section is the place for these statistics. It’s hard to assess the discussion, without having an adequate results section.

Statistical analysis has now been included in the results section. The statistical comparison with NDNS generally yielded highly significant scores (where significance was achieved) which would not be lost through application of multiple test correction.

How likely are the participants to remember their intake of 134 food types over the previous months? Wouldn’t a prospective study be a good idea for further research? Sample – how representative is your sample?

This is a limitation of the FFQ and the limitations of the methodology are explored in the Discussion. We agree that a prospective study is warranted in this area, but this is outside the scope of what is achievable in a manuscript revision!

The modified questionnaire – does validity hold once the questionnaire has been modified?

The modifications made to the questionnaire are minor and will not affect the validity of the questionnaire. These modifications are now described in more detail in the methodology section.

Could you please replace “subjects” with “participants” or “people with IBS”.

The word subjects has now been amended throughout the text

Could you clarify why you have used the Rome II criteria rather than III?

Rome II criteria remain recognised as a benchmark diagnostic set of criteria for IBS diagnosis, whilst it is suggested that Rome III criteria require further testing and evaluation (Whitehead & Drossman, 2010, Am J Gastroenterol)

Page 4, first two lines: “…prior to an intervention trial for pre-, pro- and synbiotics”. Could this be reworded – it’s not quite right.

This has been revised.

The use of initials rather than the full descriptions makes the text difficult to read in places. Page 7 in particular includes many initials, e.g. EAR, NDNS, NSP. Since there are no space constraints, the manuscript would be more readable with the name in full, the first time it appears on a page, e.g. line 2 on page 7 would have NDNS in full, further mention would have the abbreviation on this page. Likewise DRV and NSP. Similarly for other pages.
The request to have the name in full appearing the first time it appears on a page will not be possible until the proof stage. We have therefore avoided the use of any abbreviations and have used the full names throughout.