Reviewer's report

Title: Effects of short-term fructooligosaccharide intake on equol production in Japanese postmenopausal women consuming soy isoflavone supplements: A pilot study

Version: 1 Date: 17 October 2012

Reviewer: Charlotte Atkinson

Reviewer's report:

Major essential revision
1. The purpose of the ‘pilot’ aspect of the study is not clear. Please can the authors include information on why they have classed it as a pilot study, e.g., was it intended to guide the planning of a larger-scale study (e.g., to estimate sample size, look at feasibility issues, etc, etc.)? Can conclusions (page 7, lines 7-9) really be drawn from this study when it was considered to be a pilot (and presumably was not powered to detect differences)? The term ‘pilot’ should not simply be used to describe a small scale study.

Minor essential revisions
1. Page 2, lines 3-4 & page 3, lines 8-9. ‘…the clinical effectiveness of isoflavones can be attributed to their ability to produce equol’. As written, this sentence does not make sense; strictly speaking it is not the ability of isoflavones to produce equol, it is the ability of individuals to produce equol from daidzein (via their gut bacteria). Please edit as appropriate.
2. Page 3, line 6 – the reference to support an effect of isoflavones on hormone-dependent cancers does not seem to be appropriate (the reference is for the Million Women study and is concerned with exogenous hormone use and endometrial cancer, and does not mention isoflavones). Please provide a more appropriate reference.
3. Page 3, line 20 – the reference used to support the statement that ‘Lactobacillus and Bifidobacteria species have been identified to play a role in the metabolism of daidzein to equol’ may not be appropriate, or the statement may not be correct. For example, among the studies discussed in the cited paper, one showed Lactobacillus to be part of a mixed culture that produced equol, and another showed no effect of Lactobacillus and Bifidobacteria given as a probiotic on equol production status. Unless I missed it, I don’t think that any of the studies discussed within the cited paper suggested that Bifidobacteria play a role in equol production. Please either cite a more appropriate reference (or references) or modify the sentence.
4. Page 4 – there is a lack of discussion of methodological factors such as allocation concealment or potential carryover effects. Please provide more details to allow the reader to assess the quality of the research.
5. Page 4, line 8 – please indicate why you have used the log equol:daidzein value of \(-1.70\) as a cut off (presumably this is from the Setchell et al. J. Nutr. 2006 paper?).

6. Page 4, lines 15-16 – please state how much daidzein participants consumed – was it 25 mg or did that amount also include genistein?

7. Page 5, lines 6-8 – if you started with 43 subjects and 5 withdrew then there should have been 38 remaining and not 34? Was an intention to treat analysis conducted?

8. Page 12, line 5 – should this say \(P<0.05\) for the difference between baseline and intervention?

Discretionary revisions

1. Page 2, lines 11-12 – the use of the word ‘respectively’ suggests that equol producers were assigned to FOS and equol non-producers assigned to control. Please clarify that the subjects were randomly assigned to treatment group. Also, please indicate the sample size in the abstract.

2. Page 2, line 14 – it might be useful to say here that a soy challenge was used to assess equol-producer status prior to the start of the study.

3. Page 2, lines 16-17 – this seems an odd place to mention that the equol:daidzein ratio is ‘a more reliable indicator of equol production’. I would suggest deleting it (it is discussed on page 5 anyway).

4. Page 3, line 4 – is there also an argument that isoflavones may act as antiestrogens given their affinity for estrogen receptors but weak estrogenic effects? It might be worth discussing this.

5. Page 3, lines 5-6 – I would suggest calling cardiovascular disease, osteoporosis, and hormone-dependent cancers something other than postmenopausal ‘symptoms’ as they are not, strictly speaking, symptoms. Rather, they are conditions that are more common following the menopause.

6. Page 4, line 3 – the exclusion criteria in the cited paper say that equol producers were not included in that study. Please clarify that not all of the exclusion criteria from the cited reference apply to the present study.

7. Page 5, lines 13-14 – if the results and discussion are presented together then it might be worth discussing whether your finding of little difference in diet between equol producers and non producers is similar (or otherwise) to other studies that have looked at dietary differences between these groups.

8. Page 6, lines 3-10 – I’m not really sure of the relevance of this section. Please can the authors clarify what they are trying to say?

Quality of written English: Acceptable

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.
Declaration of competing interests:

I declare that I have no competing interests