Reviewer's report

Title: Mediterranean dietary pattern and depression: the PREDIMED randomized trial

Version: 1 Date: 16 May 2013

Reviewer: Allison Hodge

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This study addresses an important and so far under-researched topic on dietary pattern and depression. The analyses appear appropriate for the available data and the manuscript is clearly written. However, there are several small grammatical issues that could be improved as detailed below.

Minor essential revisions:
1. I don’t know if this journal has a policy with regard to how people with diabetes are described but I believe it is preferable not to use ‘diabetics’ or ‘diabetic participants’ rather people with diabetes or type 2 diabetes. This needs to be addressed throughout.
2. First line in abstract, omit ‘the’ at end of line, ie before adherence.
3. Last line of Abstract background, omit ‘in’ before intervention.
4. Abstract methods, start with capital ‘M’ for multicentre.
5. Middle of abstract methods, ‘51% of them had type 2 diabetes’.
6. Abstract conclusions, insert ‘a’ before Mediterranean.
7. Page 5, first line, insert ‘a’ before leading.
8. Second line, delete ‘also’ before ‘the first leading’.
10. Para 3, page 5, delete ‘the’ at end of second last line.
11. Para 4, page 5, insert ‘by’ before carrying out.
12. Last sentence on page 5. Suggest rewrite as : Thus the aim of this analysis was to assess the effects of two Mediterranean diets on depression risk: MedDiet supplemented with virgin olive oil, and MedDiet supplemented with mixed nuts, in comparison with a low fat control diet.
13. In the first para on page 7, use the past tense re physicians and nurses roles.
14. Should energy be reported in kilojoules rather than kilocalories?
15. Bottom of page 10, ‘…we had complete data from 2,513…’
17. Last para on page 11: available evidence is sparse; interpretation of results from observational studies requires caution; delete rest of sentence after ‘caution’. This design is weak for inferring cause-effect relationships; delete ‘on
the other hand'; ‘These large studies generally use food frequency questionnaires…’. Delete ‘they’ so you have ‘generally have been validated’; delete uncontrolled before ‘residual confounding’.

18. Page 12, ‘low insulin secretion has been associated with an increased risk of developing depressive symptoms’.

19. Page 12, The association of leptin with depression could be explained not only by its metabolic properties…’

20. Top of page 13, ‘…the control diet exhibited…’

Other issues

21. The discussion should at least consider why you found significant results for nuts but not olive oil.

22. Page 11, in the description of results from the Australian Longitudinal Study on Women’s Health it is not clear whether the OR relates to cross-sectional or longitudinal analysis.

23. At the bottom of page 11 when considering the weaknesses of cross-sectional studies you should mention the strong possibility that being depressed could make people eat a less healthy diet and perhaps find a reference for this. This is a specific example where reverse causation could be important.

24. Is it possible that the association was stronger in people with diabetes because they we more likely to be diagnosed with depression because they had more contact with various health professionals?

25. The paragraph on page 13 about walnuts and tryptophan was confusing. I assume you mean that walnuts contain tryptophan and not serotonin. Were walnuts part of the PREDIMED nut intervention?

26. At the bottom of page 13 the argument that the lack of significance in interventions compared with observational studies may be due to lack of variability in intakes for interventions seems odd. Even sub-optimal adherence to an intervention could put people at intake levels beyond those seen in the general population. Many observational studies have been criticised for the homogeneity of the diet of participants. Unless you can actually show data from real studies that supports this I would omit this argument. I do agree on the other hand that because this study is conducted in a Mediterranean population, they may all have higher adherence to the MedDiet than a population in another area, and this might make the effect of the intervention less.

27. In the text describing the data in table 1, some differences are note between control group and others but it does not state whether these diffs are significant and this information is not provided in Table 1. It would be worth showing the significance of between group diffs in the table.

28. Table 4, missing ‘confidence’ before ‘intervals’ in title.

29. In Table 4 footnote ‘Model 2 additionally adjusted ‘for’ …

30. Ref 16 has been published, update details in refs.
Quality of written English: Needs some language corrections before being published

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:
I declare that I have no competing interests