Reviewer’s report

Title: Adherence to the Australian Dietary Guidelines and development of depressive symptoms at 5 years follow-up amongst women in the READI cohort study

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Reviewer: Michael Orlich

Reviewer's report:

This is an interesting and important study looking at the prospective association of dietary guidelines compliance with depressive symptoms. The paper is well written and the methods described seem sound. The specificity of the investigation in terms of the exposure classification (Australian dietary guidelines) and the population (low SES women) are interesting strengths. I provide the following comments in hopes of further improving the paper:

1. The tables and figures in the paper are all devoted either to methodology or to descriptive statistics, whereas the main findings are reported only in the manuscript text. I think the reader would be greatly helped by providing the main findings in the form of tables (and/or figures) as well. Some of the current tables or figures could be moved to supplementary materials to make space for tabular presentation of the main findings.

2. The percentages of those with heightened depressive symptoms by dietary pattern do not appear to be correct (or helpful). (lines 308-312) They appear to be percentages of all those with heightened symptoms, which mainly reflects that the medium dietary group is the largest group. For the medium group, the helpful percentage should be 123/619 = 19.9%, for example.

3. In the results section (lines 314-320) (and also in the abstract), the authors state that for a 10-unit change in DGI, there is a 12% lower risk. But then they give a risk ratio of 0.987. These seem at odds. Is the RR for a 1-unit change? If so, that should be explicitly stated. I think sticking with the 10 unit change for consistency would be more helpful. I would expect to see an RR of ~ 0.88 with an appropriate CI.

4. The reported discretionary food consumption for the READI cohort seems implausibly low. I suspect this reflect very poor capture of the discretionary foods due to FFQ design or selective under-reporting on this item. I think the authors should at least discuss this possible concern. It might warrant a sensitivity analysis when excluding that item.

5. Throwing away the 3-year follow up data seems imprudent, given the huge losses of sample due to exclusions. The intervention studies discussed noted improvements in depression in weeks or months. It would seem important to also examine the 3 year data to see if it contains a similar signal over a shorter follow-up but with more people included.
6. It would be good to discuss explicitly some plausible unmeasured confounders in the limitations section.

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