Author’s response to reviews

Title: The association between diet quality, dietary patterns and depression in adults: A systematic review

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Author’s response to reviews: see over
Dear Editorial Office,

**Manuscript ID: 7906348219198107**

We thank the reviewers and editors for their helpful comments regarding our manuscript titled “The association between diet quality, dietary patterns and depression in adults: A systematic review”. We have revised our paper accordingly, and provide responses to each of the reviewer’s comments (presented below) with page/paragraph/line numbers referring to the revised (red) text. We believe the reviewers’ comments have strengthened this manuscript.

Thank-you,

Sharon Brennan, for the authors, Shae E Quirk, Lana J Williams, Adrienne O’Neil, Julie A Pasco, Felice N Jacka, Siobhan Housden, Michael Berk, Sharon L Brennan.

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The association between diet quality, dietary patterns and depression in adults: A systematic review

Quirk et al

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EDITORS REQUEST:

1. Please add PRISMA statement in your manuscript.

Our manuscript now included the PRISMA statement, which reads as follows:

Page 4, Paragraph 2, Lines 1-2 (Methods): This systematic review adheres to the guidelines addressed in the preferred reporting items for systematic reviews and meta-analyses (PRISMA) statement 2009.

Reviewer 1: Russell J de Souza

1. Search strategy: thorough- used 3 databases; wonder why EMBASE was not searched?

Given that no single database covers all information needs, especially with regards to interdisciplinary topics such as associations between diet quality and depression, we agree with the reviewer that a mix of databases is appropriate in order to achieve undertake a thorough electronic search. We did not search EMBASE as it is a pharmacological and biomedical bibliographic database concentrates more on the pharmacological literature (Chapman, J Can Acad Child Adolesc Psychiatry 2009). Our search strategy also included a manual search of the reference lists of eligible studies and we tracked citations of those publishing in this area as a measure of identifying eligible articles that our electronic search strategy had inadvertently missed.

2. Methodological quality of included manuscripts- the scale chosen has good face validity and covers all the major threats to validity. Agree with hierarchy of obs [sic] designs.

Data Analysis: Deciding not to meta-analyze was, in my opinion, a good strategy. Criteria for evidence synthesis also have good face validity. I commend the authors on
taking this approach; it is, of course, somewhat subjective, but I think well-justified, and transparent.

We thank the reviewer for these comments.

3. QUORUM Statement: This (Figure 3) is acceptable, but my preference, this would be better displayed as a typical CONSORT diagram, which is what most readers will expect to see. I suggest re-formatting this figure to follow this format (see McDonald, Han et al., BMJ, 2010, Figure 1 for an example of this format). Other than this, text describing search and selection approach is concise and covers major questions a reader would have.

We agree with the reviewer, and now provide an adaption of the CONSORT diagram, presented as Figure 3.

Revisions Suggested Discretionary Revisions (which are recommendations for improvement but which the author can choose to ignore)

4. The authors claim that "high levels of heterogeneity remained in the 18 studies determined as high quality." This reason is used to justify not performing a quantitative synthesis. Was this a post-hoc decision based on tests of heterogeneity from a meta-analysis? Or did they make this decision a priori? I understand that this a commonly taken tactic, one thing that we must be careful of is inappropriately not pooling data. The question of heterogeneity I would have is whether not the authors consider this heterogeneity clinically relevant. For example if 50% of studies had OR <1.0 and 50% of the studies had OR >1.0, then we'd not want to pool; but if all studies had RR >1.0, but the heterogeneity may be in the quantification of the effect. For example, if the effects range from RR 1.2 to 1.5, and are "heterogeneous" by I2 >50%, is that really sufficient justification not to pool? Of course, this is a judgement call, and I leave it to the authors, who are the experts in the, area to make it. My suggestion, however, is to move the reasons listed in paragraph 2 of discussion to an earlier point in the manuscript (where "Methodological quality of included manuscripts" is discussed), to provide some detail on how they arrived at the decision that the data were "too
heterogeneous" to quantitatively pool. I would suggest things like variation in methods of assessment of diet quality, study design, assessment of depression, etc.

Our decision not to proceed with a meta-analysis of the data was determined *a priori*. Given the current work in this field of enquiry being undertaken by Jacka et al, the authors had an appreciation of the inherent heterogeneity of these studies, largely related to the measurement of diet and assessment of depression. Thus, we did not statistically investigate heterogeneity. In order to provide further clarity regarding our *a priori* decision, we have amended our text to read:

Page 5, Paragraph 4, Lines 1-4 (Methods): Our decision not to proceed with a meta-analysis of the data from reviewed studies was determined *a priori*. Given the current work in this field of enquiry being undertaken by the authors, our group had an appreciation of the inherent heterogeneity of these studies, largely related to measurement of diet and assessment of depression.

Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)

5. It is now "minimal standard of reporting" for systematic review authors to include a PRISMA checklist with systematic reviews. The authors would be well-served by filling out and attaching this with their MS (as supplementary material); and including a statement to this effect in the methods. (http://www.prisma-statement.org/statement.htm). Since the meta-analysis component was not done, some fields will be "not applicable", but for the sake of transparency, I think the checklist should be added. Methodologists will like to see it, and it will give an already high quality paper more "gravitas".

As suggested by the reviewer, we now include the PRISMA checklist as supplementary material. See also our response to request from the Editor.
Reviewer 2: Lesley MacDonald-Wicks

Major essential revision:
1. Only three data bases were searched, in particular PubMed was not searched.

Medline is the largest subset of PubMed. Given that we had based our search strategy and terms for this review using the MeSH controlled vocabulary, had we used the PubMed database rather than Medline, our search retrieval would have restricted citations to Medline alone anyway (US National Library of Medicine, NIH, [www.nlm.nih.gov/pubs/factsheets/dif_med_pub.html](http://www.nlm.nih.gov/pubs/factsheets/dif_med_pub.html)). We acknowledge that PubMed also includes in-process citations and ‘ahead of print’ citations, however we also determined that our best evidence synthesis would be based on published data. According to Item 8 of the PRISMA checklist ([http://www.prisma-statement.org/statement.htm](http://www.prisma-statement.org/statement.htm)) our methodology exceeded the minimal standard of electronic searching at least one database. (Please also see our response to Reviewer 1, Comment 1). We have revised our text to read:

Page 5, Paragraph 1, Line 1 (Methods): … was implemented using Medline (largest subset of PubMed), CINAHL, and PsycINFO for citations of relevant articles…

Minor Essential revisions:
2. In the introduction the term dietary patterns is used, without a clear definition of the interpretation the authors wish to impose. This is a widely used term with a variety of interpretations, the paper would benefit from a stated definition.

Given the relative infancy of this field of research there are considerable complexities in defining diet quality and/or dietary patterns as the reviewer has indicated. In response to this comment we have clarified our text as follows:

Page 4, Paragraph 1, Lines 1-3 (Introduction): For the purpose of this review we define diet quality and dietary patterns as the quality of overall habitual dietary intake, and the pattern of overall habitual dietary intake, respectively, which is consistent with prior research.

3. Method: inclusion criteria did not include RCT or similar study design, is there a reason they were specifically not included? This is level 2 evidence and if present should
be included. The heterogeneity is such that such different study design would not impact on presentation of results.

The aim of our systematic review was to assess the current levels of evidence in an emerging field of research where the extant literature is sparse. There are currently no data from RCTs that examine the impact of dietary quality on depressive symptoms; our research team has recently published a study protocol of the first RCT of its kind (O’Neil, Berk, Castle, Itsiopoulos, Ope, Pizzinga, Brazionis, Hodge, Mihalopoulos, Chatterton, Dean, Jacka, 2013, *BMC Psychiatry*). It should also be noted that our aims were to ascertain whether an association existed between habitual and/or overall diet quality and depression; rather than the study of dietary intervention effects, thus intervention studies were excluded.

4. **Method: Were there any date restrictions imposed?**

We had identified in our manuscript that citations of relevant articles were “…accessible between 1965 and October 2011”, however now provide further clarity. Our manuscript now reads:

Page 5, Paragraph 1, Lines 2-3 (Methods): …for citations of relevant articles, which were restricted to January 1965 to 31st October 2011.

5. **Why was the grey literature not consulted, web of science and the dissertation database are important sources of data.**

Given that grey literature has not been subject to peer review (an imperative issue when considering a nascent area of enquiry), we determined *a priori* to exclude this source from our review. Whilst we appreciate that dissertations may provide some data, we excluded this source due to bias inherent in some theses being subject to embargo. We did not search Web of Science (WOS) given the primary function of WOS is to index information from Science Citation Index, Social Science Citation Index and Arts and Humanities Citation Index (Chapman, *J Can Acad Child Adolesc Psychiatry* 2009) rather than data from nutrition, psychology, or multidisciplinary fields. We have amended our manuscript to read:
Page 13, Paragraph 3, Lines 1-4 (Limitations): We acknowledge that grey literature and dissertations regarding these associations may exist. Whilst the exclusion of these sources of literature may result in our review reflecting less than the existing evidential base, it was beyond the scope of this study to systematically ascertain and review grey literature and dissertations.

6. How were the search terms decided. Did you undertake a preliminary search and use MeSH headings to inform the search terms. This is the key to the systematic review and a bit more detail around the keywords and use of Boolean phrases is important. It may also be useful to search key authors in the field.

As indicated in our response to Reviewer 1, comment 3, the process to determine the search strategy was informed by the existing expertise held by one of our authors in this field of enquiry. Following the standard process of conducting a systematic review, we undertook a preliminary search of relevant search terms before identifying these under the MeSH headings. The following Boolean phrases were used in our computerised search for Medline (largest subset of Pudmed) and CINAHL databases:

Food habits (MeSH) OR Diet (MeSH)
Dietary (Keyword) OR dietary quality (Keyword) OR dietary consumption (Keyword) OR dietary patterns (Keyword) OR western diet (Keyword) OR Mediterranean diet (Keyword)
(Food habits OR Diet) OR (Dietary OR dietary quality OR dietary consumption OR dietary patterns OR western diet OR Mediterranean diet)
Depression (MeSH) OR depressive disorder (MeSH) OR depressive disorder, major (MeSH)
Depression (Keyword)
(Depression KW) OR (Depression OR depressive disorder OR depressive disorder, major)
[(Food habits OR Diet) OR (Dietary OR dietary quality OR dietary consumption OR dietary patterns OR western diet OR Mediterranean diet)] AND [(Depression KW) OR (Depression OR depressive disorder OR depressive disorder, major)]

The Boolean phrases were slightly modified for PsycINFO database:
Food habits (Keyword) OR Diet (Keyword)
Given the extent of this information, we have not included it in our manuscript, but had highlighted for the reader that this information is available from the Authors. Should the editor prefer, we would agree to include this as supplementary material.

Discretionary

7. The use of a flow chart rather than descriptive text for the included excluded studies is an effective tool in presenting this research.

Please see our response to Reviewer 1, comment 3, with regards to the revised inclusion of a CONSORT diagram.

8. I would like to see the authors draw some recommendations together to provide future researchers with some 'aids' to designing research in this area with a view to providing good scientific evidence to fill the evidence gap outlined in this paper.

We have added to our current recommendations, and our revised conclusion now reads:

Page 14, Paragraph 1, Lines 1-6 (Conclusion): Given the relative infancy of this area of research, we suggest that the construction of a standard definition for dietary quality and patterns would enhance future work in this area of enquiry. Higher quality cohort studies using more consistent measures of diet quality or dietary patterns to ensure findings are
generalisable and comparable are required; the validation of such tools would, in time, further enhance our understanding of these associations.