Author's response to reviews

Title: Mental health literacy as a function of remoteness of residence: an Australian national study

Authors:

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Author's response to reviews: see over
Editor,  
BMC Public Health.

Dear Dr Norton,

Re: MS: 1752461557215926. Mental health literacy as a function of remoteness of residence: an Australian national study. Kathleen M Griffiths, Helen Christensen and Anthony F Jorm

Thank you for providing feedback on the above paper from four referees. We have revised the paper in accordance with these comments. Please find below a point by point response to the referees’ comments together with a summary of the changes made in the revised paper.

Referee 1:

1. Given the multiple indicators used in this study, it would be helpful if a formal definition for mental health literacy were provided.

A formal definition is now included in the first paragraph of the Background.

2. It would also be helpful if the paper contained some operational detail to flesh out the geographical classification used so that readers could better understand what these terms mean in terms of population size or density (for example).

More detail about the AGSC is included and a reference to the system is cited. Due to the nature of the index system underpinning the AGSC it is not possible to describe each category using a simple population descriptor.

Referee 2:

Major essential revisions:

1. A major concern is the low participation rate, 34 %, of the total sample. However, an even more problematic point of view is that differences in participation rate between the three categories of remoteness is not investigated and declared. If participation varied in the three categories this could relate to the differences in attitudes. By the way, why did the methods include more telephone reminders to metropolitan participants than to participants in rural areas if participation rate in different areas was not a relevant issue? In the Discussion paragraph the author acknowledges the limitation of the low response rate, but I consider a thorough analysis of non-participation as essential in a scientific publication.

Two issues are raised here: (1) The low participation rate; (2) The absence of information about participation rate for different remoteness categories.
(a) Low participation rate: As the referee notes, this is noted as a limitation in the discussion. We have no empirical data to inform further discussion about the reason for refusal. We have therefore now added a comment to the Limitations section that the study lacks data on the characteristics of refusers. Note that the Method section refers the reader to some of our previously published papers which contain a more detailed outline of Method including details about non-contact. Despite the low participation rate, other scientific journals, including BMC Public Health (but also J Affective Disorders, BMC Psychiatry, Aust J NZ Psychiatry) have accepted other papers in this series for publication.

We have therefore now added a comment to the Limitations section that the study lacks data on the characteristics of refusers. Note that the Method section refers the reader to some of our previously published papers which contain a more detailed outline of Method including details about non-contact. Despite the low participation rate, other scientific journals, including BMC Public Health (but also J Affective Disorders, BMC Psychiatry, Aust J NZ Psychiatry) have accepted other papers in this series for publication.

(b) Response rate as function of remoteness. We have already noted this as a limitation in the Discussion. Unfortunately, we do not possess data on non-contact or refusals below the level of ‘State’ for this survey. Additional comment has been added (a) clarifying the limitations; (b) pointing out that the two previous comparative studies similarly did not report this data; and (c) noting that the respondents were approached to participate in a health survey not a mental health survey so that survey refusal was not directly linked to the mental health content of the survey.

With respect to the question concerning differential call backs, the number of call backs to the rural and metropolitan areas were set prior to commencement of the survey by AC Neilsen the survey company. We believe that this decision was pragmatic and reflected the additional costs associated with accessing rural households in this face-to-face survey.

2. Another major concern is the presentation of results. Nine tables is, according to my experience from other journals, far too many. The number of tables has now been reduced by 4 (from 11 to 7). It would be difficult to reduce the number of tables further without redefining the scope of the paper and reducing the number of measures reported and discussed. (For reference, the Goldney et al study, which was similar in scope to the current paper, included 8 tables and one figure).

3. Also the comments in the Results section include differences that were not statistically different when controlling for demographic variables. I strongly suggest leaving these out, the Results section would benefit from being much shorter. The Results section now focuses on the key results/patterns. From a pragmatic point of view (i.e., targeting rural regions for intervention) significant unadjusted differences can be important.

Minor Essential Revisions

4. The literature review is skewed towards Australian experiences. It would increase the interest of readers of the journal if a wider perspective was described in the background section. A consequence of the narrow perspective applied is the notion made by the author that suicide rates generally should be higher in rural areas. This may only partly be true, with a wider perspective, it could be contradicted that female suicide in many parts of the world has higher rates in urban areas.

We do not agree that the literature in the Background is substantially or inappropriately skewed towards Australian experiences:

(a) The statement regarding suicide rates referred to suicide rates across the world and only one of the cited references was Australian. However, in an effort to address the reviewer’s concerns, the statement about suicide has been modified somewhat and the references cited in support of it are reviews of the world literature. No Australian reference is now included. With respect to possible interactions between gender, remoteness and country; even if it is true that such interactions exist, it remains the case that the majority of studies have found that overall, suicide rates are higher in rural regions. In order to address this problem the level of mental health literacy of the community as a whole may be critical (eg, recognizing symptoms in others). Please note that the intention here is not to systematically describe the literature on suicide but rather to provide a rationale for a study of mental health literacy in rural areas.
(b) The literature cited on rural mental health literacy was based on a PubMed database search. The search revealed few relevant studies and only two relevant comparative studies: one Australian and one Canadian. Each is described. To ignore the Australian study would be inappropriate.

(c) There may be some grounds for criticizing the inclusion of a reference to beyondblue in the background section since it is Australian specific. However, the initiative is used to illustrate a more general point which is more universally applicable. Secondly, the reference provides background context for the current study which includes a measure of awareness of beyondblue.

5. A part of the background that could have been of interest to include is if there are actual differences in prevalence of mental ill-health depending on remoteness. Hypothetically, being exposed to mental disorder during childhood and adulthood could be a significant reason for different attitudes. This is partly studied in the present study by asking the respondents about current own mental health.

References to relevant studies of the prevalence of mental illness are deferred to the discussion. No attempt was made to include these in the Background for two reasons. First, in contrast to studies of suicide rates, there is not consistent evidence of a greater prevalence of mental disorder in rural regions. Accordingly, it was not appropriate to reference this literature in support of conducting a study of mental health literacy. Secondly, the primary aim of the study was to investigate mental health literacy, not the prevalence of psychological disorder and it was important that the background reflect this.

6. Educational background is one of the well-known reasons for differences in knowledge, attitudes and inclination to seek psychiatric help. This is not neglected in the present study, results are controlled for educational background. However, could it be that the two categories of educational background (having a bachelors degree or not) are too broad as a description of education?

Yes, we agree. This is now noted as a limitation in the Limitations section. (The data, which relied on the demographic questions employed in the original study, was not suitable for a finer ordinal analysis of the educational item).

Discretionary Revisions

7. A question that is not commented is why the participants were given a male or female version of the vignette. What was the argument for this, was any gender difference suspected? I do not understand if male respondents were given a male vignette or if the vignettes were distributed randomly?

The male and female vignettes were randomly allocated to respondents. This is now noted in the paper. No controls were placed on presenting the female or male scenarios to female or male respondents.

Reviewer 3:

1. The quantity of information covered in the manuscript together with the unusually large amount of tables presented make the reading and comprehension of the manuscript somewhat cumbersome. May be a short report will suffice. .....In light of the written above I suggest the authors to make a major revision by shortening the manuscript to a short report, detailing only statistically significant findings, stressing the importance of the lack of difference among the groups and the implications of these findings. All this should be done with cautious note of the limitations of the study.

The number of tables has now been reduced and the results section focuses on key results (see above).
2. Additionally, the following explanations and/or clarifications are required:

a. A more specific explanation of the Australian Standard Geographical Classification is required for non-Australian readers.
   Done. See comment for Review 1 above.

b. Page 12, last paragraph and page 13, line 4 from top. It should be Table 11 instead of 10.
   Thankyou (no longer relevant).

c. Page 16, lines 11 – 13 from top and lines 7 – 10 from bottom – these sentences need re-writing.
   Done

d. Page 17. Awareness and experience of depression and depressive related symptoms – the title appears without any discussion of the topic.
   Title deleted (erroneously included – discussion is already included elsewhere)

e. Tables, page 13, Table 7. The title does not reflect the content of Table 7ii.
   Title and internal heading clarified (on new Table 5).

Reviewer 4:

1. First paragraph, place the comma within the quotation brackets
   Comma eliminated with rewrite of paragraph.

2. Third paragraph, recommend that the sentence beginning with however be re-worded as seems awkward.
   Done (part of sentence deleted).

3. Paragraph above methods section, place a comma after rural and city populations[,]
   Done.

4. First paragraph in methods section, please identify AGSC
   Done.

5. Paragraph beginning respondents were asked … please use a hyphen in the phrase ‘self-rate’.
   In same paragraph, please elaborate included the inclusion of this self-rated health status questionnaire. What are the psychometric qualities of the measures.
   Done.

6. In the section depression vignettes, the sentence beginning with “the pattern of findings …” seems awkward.
   This sentence has now been deleted (in the context of shortening the results section as above).

7. Please clarify the last sentence in the paragraph above the discussion of the depression section.
   This sentence has been rewritten.

8. Page 15, last paragraph beginning with the finding that the prevalence … please insert a space after [25]
   Thankyou, inserted.
9. Under the section “Schizophrenia” please clarify the meaning of “first-line” evidence. Place a period before the sentence “The person ...” In same paragraph, “… that antipsychotic medications as [are] important tools ...”

The meaning of ‘first-line’ is now clarified.
A period has been inserted and ‘as’ has been replaced with ‘are’ (thankyou).

10. In paragraph avoid section “Awareness and experience of depression …” please place punctuation within the quotations. Please re-work the last sentence

Title deleted as noted above. It had been included in error.

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We hope that the above amendments are acceptable.

Yours sincerely,

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