Author's response to reviews

Title: The Structure of Psychological Life Satisfaction: Insights from farmers and a general community sample in Australia

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Author's response to reviews: see over
Dear Editor,

We would like to re-submit our revised manuscript for consideration for publication as a research article in *BMC Public Health*. Our paper is entitled ‘The Structure of Psychological Life Satisfaction: Insights from farmers and a general community sample in Australia’. We have addressed the helpful comments of our reviewers as detailed below:

**Referee 1 (Robert Cummins)**

**Suggested discretionary revisions:**

**Comment 1.1.** *The number of appendices and tables seems to me excessive, but this is an Editor’s decision.*

Response: The appendices were included to meet the STROBE checklist for reporting epidemiological research (http://www.strobe-statement.org/) while still preserving the article’s brevity. However we can condense the appendices if the editor recommends we do so.

**Comment 1.2.** *I recommend more information concerning the nature of the general population sample in terms of its city/rural mix and whether it includes farmers.*

Response: We have amended the Methods (p. 8) and the descriptive characteristics reported in the Results (p. 10) to include information on whether people in the general community sample are located in a major metropolitan area. Unfortunately, we do not have access to detailed occupation information for the community sample.

**Comment 1.3.** *A description of the Personal Wellbeing Index scores and domain scores in terms of their relationship to Australian norms would be informative.*

Response: We have conducted t-tests to compare the sample PWI means to the Australian norms measured by Cummins in the same year. Table 1 now has superscripts indicating whether a mean score is higher or lower than the Australian norm (p. 24). The Results section has also
been amended to include a brief description of the way the sample means either coincide with or differ from Australian norms (p. 11).

Comment 1.4. In Table 2, the sr2 statistic may not be familiar to many readers and should be explained.

Response: We have inserted a table note (p. 25) explaining that the sr statistic is the semi-partial coefficient, which provides the unique variance that each predictor shares with overall life satisfaction, adjusting for covariance between different predictors.

Referee 2 (Gareth Davey)

Major compulsory revisions:

Comment 2.1. The linkage between the findings of the study and health promotion and policy are weak. Therefore, the study’s the relevance to BMC Public Health is limited.

Response: We have made two changes to address this point. Firstly, we have updated our literature review (pp. 6) to place our study within the context of other research on understanding the drivers of overall life satisfaction with a view to health promotion. A number of articles on this topic have recently been published by BMC public health. Secondly, we have revised and reorganised our discussion of the results, and we now more specifically discuss how understanding the composition of life satisfaction supra-domains can inform the way health promotion programs are tailored to different populations (p. 17-18).

Comment 2.2. Concepts and terms investigated in the study—such as psychological life satisfaction and wellbeing—are unclear and not consistent with the literature. The authors could define and explain more clearly the way they interpret the constructs.

Response: As well as already explicitly defining life satisfaction (p. 4), we have amended our Background to also define social capital and personal efficacy, as the concepts underpinning our connectedness and efficacy supra-domains (p. 7). These concepts are closely related to different life satisfaction domains, and exploring this link is part of the novel contribution of the paper.

Comment 2.3. The literature review is incomplete. There are important articles that should be included, e.g. research from the International Wellbeing Group. The predecessor of the PWI (Comprehensive Quality of Life Scale; ComQol) could also be discussed.

Response: We have broadened our literature review to place the PWI in better context, citing its iterative progression from earlier research on
quality of life and also the current cross-cultural work being done with the scale by the International Wellbeing Group (p. 5). As noted above, we also now refer to a broader range of research on the drivers of satisfaction (p. 6).

**Referee 3 (Brent Donellan)**

**Major compulsory revisions:**

**Comment 3.1.1.** Farmer sample differs from the Community sample on a range of demographic variables that might impact the results... It might be worth doing additional analyses to control for all of these factors throughout all analyses.

**Response:** This comment broadly refers to all of the paper’s analyses, which we will address in turn. We agree that the influence of demographic variables is an important issue which should be brought to the reader’s attention. Additional analyses for the mean satisfaction differences in Table 1 showed that demographic characteristics accounted for many of the life satisfaction differences between populations. We consequently decided that the t-test comparisons were not helpful for understanding the data. Instead we have added superscripts to the life satisfaction means in Table 1 to identify mean differences that are significant when controlling for demographic characteristics (p. 24). This change has also changed some of the discussion of the results, since we have removed discussion of the t-tests from the paper (pp. 11, 17-18).

The final SEM models control for demographic characteristics, with non-significant variables deleted and we have amended the Methods statistical analysis section to make this clear (p. 10). The regressions testing the mediation relationships identified in the final SEM models (reported in Tables A4 and B6) also included all covariates, and deleted non-significant effects. Table notes have been added to the tables clarify this point (see appendices).

The regressions in Table 2 do not control for covariates because the purpose of these analyses was to identify covariance between different satisfaction domains within the two different populations as a precursor to SEM. A table note has been added to clarify this point (p. 25).

**Comment 3.1.2.** I think the authors need to use multi-group SEM techniques to make formal comparisons between the Farmer sample and Community Sample.

**Response:** An amendment has been added to the statistical analysis section of the Methods (p. 9) to clarify why multi-group techniques were not used in the analyses. Multi-group tests were conducted but the results indicated that both the factor weightings and conceptual structure of life satisfaction was different across the two
populations, making separate modelling the appropriate method of analysis.

Comment 3.1.3. In terms of Figure 3, I did not understand why income and the other demographic variables were modeled in what seems to be unconventional ways. I also did not understand why there was a directional path from connectedness to efficacy. This approach seems hard to justify theoretically.

Response: We used standard iterative methods to model the demographic variables (p. 9-10). However, to provide context for the reader, we have amended our language to make it clear that the way demographic variables behave in our models is consistent with past research, including citations for the reader’s reference (p. 14-15).

In terms of the relationship between connectedness and efficacy, as explained in the Method section, we used standard methods to model reciprocal pathways between connectedness and efficacy. The directional relationship was established based on significance and modification index tests (p. 9-10). To make this more salient for the reader, we have amended the Results section to reiterate that the directional relationship was established statistically (p. 14).

In addition, it is worth noting that the Discussion section does address how this relationship is consistent with theory and research on connectedness and efficacy (p. 19).

Minor Essential Revisions:

Comment 3.2.1. The authors might have made a typo on page 15 when interpreting fit statistics. They mention that adequate fit statistics were obtained for the community sample (CFI = .81) but seem to think the Farmer sample was inadequate (CFI = .90). However, these kinds of differences do not seem to be so clear from the reported statistics. Moreover, many of the models in the paper had CFI values below .95 whereas the authors argue for figures above .95 on page 9.

Response: The inconsistency on page 15 was indeed a typo, which we have corrected (p. 12).

In terms of the fit statistics more generally, we have made revisions to the Method section to provide a clear criterion for model significance (p. 10). We have selected RMSEA (criterion <= .05) as the core measure by which to assess the models because the Chi square test is affected by sample sizes greater than 400 and CFI is inflated by the number of parameters used, and it is therefore problematic to use these indices to make comparisons across our two samples and our three different types of models. This revision to provide better consistency in the way models are assessed has changed the way we report and discuss the farmer model of overall satisfaction, which is just adequate by the RMSEA measure (pp. 12,
Otherwise however, our reporting of the models is consistent with the RMSEA statistics.

Comment 3.2.2. More details about the EFA especially the initial eigenvalues for determining the number of factors to extract. Both of the supra-domains are strongly correlated and this might deserve some comment. This could create concerns when using both as predictors in models.

Response: In terms of the EFAs, we have amended Appendix B to add two tables giving more detail about the EFAs, including the initial eigenvalues.

Response: In terms of the correlated supra-domains, we have amended the Results section on to note that despite the close correlation of the two structures, the regressions showed no sign of multi-colinearity (pp. 14).

Discretionary Revisions:

Comment 3.3.1. Adding a d-metric effect sizes to Table 1 for the t-tests would be helpful.

Response: Brent Donellan’s helpful advice about controlling for covariates has led us to remove the t-tests and discussion of their relative effect sizes from the paper.

Comment 3.3.2. I worried that the language was sometimes too causal given that the data are from a cross-sectional study

Response: We have amended our language in the Discussion to note that the study is cross-sectional (p. 17), and to suggest in the limitations section that future research would need to test the paper’s models longitudinally (p. 18).

In making these revisions we have complied with the BMC Public Health instructions to authors. Reviewers will find that this has changed the numbering of pages, because we have moved the tables to the back of the manuscript. In addition, we have highlighted the altered sections in the paper to make it easier for reviewers to review the changes we have made.

Please contact Dr Léan O’Brien as below if you have any queries.

Yours sincerely,

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