Author’s response to reviews

Title: An Asian Study on Clinical and Psychological Factors Associated with Personal Recovery in People with Psychosis

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Author’s response to reviews:

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Dear BMC Psychiatry Reviewers,

Second Resubmission of BPSY-D-19-00777

To all reviewers, thank you for your comments to help us further improve the manuscript. We have revised the manuscript further; please see below for our point-by-point replies.

Reviewer reports:

Randolph C. H. Chan, Ph.D. (Reviewer 1): This revised manuscript, of which I was an original reviewer, examines clinical and psychological factors associated with personal recovery in people with psychosis in Singapore. As a reviewer, I appreciate the authors’ careful consideration of feedback provided. In my view, doing so has significantly strengthened this paper. A few minor issues regarding clarification and interpretation remain, which are outlined below.

1) It is uncertain about the internal consistency of the scales used in the study (e.g., HHI, ISMI, Empowerment scale, WHOQOL-BREF, RYFF, etc.). Please consider reporting the Cronbach's alpha of the scales in the text or in Table 2.
We have proceeded to report the Cronbach’s alpha of the scales in the text (Materials and method section, in which all the internal consistencies were good as they were above 0.7- HHI: line 2-3 p9; ISMI-10: line 10-11, p9; Empowerment: line 20 p9; WHOQOL-BREF: line 9-10, p10; Ryff subscales: line 20-21, p10).

2) After recoding the score for empowerment in the revised paper, it seems that the authors did not make any change to the score in Table 2. Do the authors need to update the mean, standard deviation, median, and range accordingly in Table 2?

We apologise for overlooking this correction and have made the necessary changes in Table 2.

3) On page 17, the author stated that "As there were high inter-correlations amongst the psychological variables and multicollinearity (elevated variance inflation factor and decreased tolerance) was detected in the model when all psychological factors were, only the highest correlated measure with QPR-15, HHI (baseline) and WHOQOL-BREF (time point 2), were entered into the regression mode." Please report the VIF and tolerance values before and removal of the psychological factors.

Thank you for pointing out that this was unclear and we have proceeded to add the collinearity cut-off decision in Materials and method section, line 27, p11 to line 2, p12. We attempted to report the VIF values before and after the removal of the psychological factors but found that this would confuse the readers. Therefore, we report in a table form as below for your clarification:

<table>
<thead>
<tr>
<th>VIF</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>At baseline</strong></td>
</tr>
<tr>
<td><strong>With all psychological factors</strong></td>
</tr>
<tr>
<td>HHI</td>
</tr>
<tr>
<td>Empowerment</td>
</tr>
<tr>
<td>ISMI</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th><strong>Remove ISMI</strong></th>
</tr>
</thead>
<tbody>
<tr>
<td>HHI</td>
</tr>
</tbody>
</table>
Empowerment 2.449

Remove Empowerment
HHI 1.752
ISMII 2.176

At time point 2
With all psychological factors
WHOQOL-BREF 3.249
Ryff positive relations 2.300
Ryff self-acceptance 2.752

Remove Ryff positive relations
WHOQOL-BREF 2.787
Ryff self-acceptance 2.641

Remove Ryff self-acceptance
WHOQOL-BREF 2.621
Ryff positive relations 2.208

4) The authors highlighted a few interesting findings in the paper. One of these is that depressive symptoms appear to be a more robust predictor of personal recovery (QPR-15) than other clinical factors (e.g., positive symptoms, functioning, etc.). Also, it is found the clinical factors are no longer significant in explaining personal recovery when the psychological factors are accounted for in the regression model. What are the implications of the above findings for clinical care and/or policy in Singapore?
Thank you for your comments, which have helped us to expand on this in the discussion section. The two points are linked. While it may seem that depressive symptom is the most robust clinical predictor of personal recovery and that clinical factors no longer significantly explain personal recovery after psychological factors are accounted for, we have elaborated on how these relationships may not be straightforward. Hence, depressive symptom may appear to be the most robust predictor as it may have the most direct relationship with personal recovery and at the same time, it may also be a mediating factor for other clinical factors.

Similarly, psychological factors may have the most direct relationship, as well being a mediator, with personal recovery. Thus, it is not that clinical factors do not have any role or minimal role in personal recovery, but rather that it enhances personal recovery. Furthermore, it is not certain if depressive symptom is the main clinical predictor as there are mixed findings in existing literature. As this requires more research, we have not stated clinical implications but research implications for it. Please see all these from p20, line 1 to p21, line 14 of the second revised manuscript.

5) As shown in the study, the QPR-15 demonstrated adequate psychometric properties in people with psychosis in Singapore. The scale has the potential to be used for the assessment of personal recovery in clinical and community settings. Please elaborate more on how the tools can be applied and used beyond research purposes.

Thank you for your comment. We think that more research is required before the QPR-15 can be used in clinical practice, especially on whether the CHIME encapsulates personal recovery in our socio-cultural setting. Hence, we have not elaborated on the use of QPR-15 per se but the implementation of measuring or evaluating personal recovery. Please see p23, line 18-26.

6) Given that small sample size is a concern for the current study, a power analysis might be informative, so that the reader can know the achieved power of the study and understand how much confidence one should have in observed effect size estimates.

The achieved power was *1.00 for baseline, and power *0.97 for time point 2 and the alpha level used for this analysis was 0.05. We have added that we had carried out power analysis in the limitations section of the discussion, please see p22, line 12-14.

Akiko Kanehara, MPH (Reviewer 2): The authors did an excellent job at addressing my concerns.
Mona Eklund, PhD (Reviewer 3): This paper has improved considerably after the authors' major revision. I am still generally very positive to this paper, but further revision is needed. The first parts of the paper need only smaller amendments, but the discussion is still quite immature and needs meticulous review. The comments below are in order of the manuscript, and the line numbering refers to the right column.

1. Page 5, line 13: "these studies" in unclear. Guess you mean the studies Shank et al. reviewed.

Thank you for pointing this out. We refer to the existing studies that have evaluated the psychometric properties of the QPR. We have proceeded to reword this at p5, line 23.

2. Page 10 and forwards: why is RYFF in capitals? It is not an acronym, it is a name.

Thank you for pointing this out, we have made the necessary amendments.

3. Page 11, line 8: <0.6 is a very high limit for excluding items based on CITC. The referenced paper by Law et al. may have set that limit, but methodological experts rather suggest 0.3. See for example Streiner DL, Norman GR, Cairney J. Health measurement scales: A practical guide to their development and use. 5 ed. Oxford: Oxford University Press; 2014.

We have proceeded to use 0.3 as the limit and edited the manuscript accordingly. Please see p11, line 11 and p15 line 3.

4. Pages 13-14, and perhaps elsewhere: when you present Spearman correlations the proper symbol is rs.

Thank you for pointing this out. We have made the necessary amendments, reporting it as rs throughout the second revised manuscript.
5. Page 17, lines 12-14: new symbols for correlations??? And I still have doubts about expressing "stronger… compared to" when you did not analyze differences in strength of correlations. "Higher/highest correlation" is more fair to say.

Please also check for wording in the discussion, page 20, lines 11-12) regarding "…higher….compared to.."

Thank you for pointing this out. We have corrected the symbol for correlation. Please see p16 line 14-17. We have also reworded ‘higher’ by the usage of limits (as you have recommended below in point 11) and removed “compared to” as the clinical factors are no longer used for convergent validity (refer to reply to point 10 below for reason). Please see p16 line 13, p16 line 16 and p19 line 15-17.

6. Page 18, line 3: you write "predictor for clinical factors" but I assume you mean "predictor among the clinical factors".

We apologise for overlooking this mistake. We have amended this. Please see p17, line 11.

7. Same page, consider if you want to capitalize "time point 2". You do on line 7 but not elsewhere.

We note this mistake and have decapitalized it. Please see p17 line 16.

8. Table 5: The heading indicates all predictors are found in the table, but only the clinical factors are. The psychological factors are only mentioned in a note under the table. Some type of amendment is warranted to make Table 5 logical, either in the heading or in the table.

We have added a sentence as a note, that not all psychological factors are reflected in the multiple hierarchical linear regression model. We hope this helps. Further explanation is provided in the text. Please see p18.
9. Generally in the results: please look through every instance in the text where you present statistics and be consistent in use of spaces.

We have noted this.

10. Discussion, page 19, sentence lines 6-8: do the associations with clinical factors really indicate convergent validity of QPR? That depends on how you frame the phenomenon "personal recovery" targeted in QPR. As I perceive your study, you see clinical and personal recovery as complementary, not that clinical would be an aspect of personal. Please sort this out.

Thank you for pointing this out. In the first review, reviewer one (Dr. Randolph Chan) had commented that clinical recovery do make contributions to personal recovery. Upon your comment, we do agree that there is a difference between convergent validity and being complementary. The former refers to the scores on a scale that is testing a particular construct, is related to other test scores that are theoretically related to the same construct (different measures of a construct yield similar results) [1], whereas the latter refers to a combining effect that enhances the quality of each other.

We had previously answered reviewer one that we see it as complementary but yet also agree that clinical recovery factors could be used for convergent validity of QPR. We apologise for the confusion and agree with you that clinical recovery is not an aspect of QPR or personal recovery, but that they are complementary i.e. having clinical recovery would enhance the quality of personal recovery. This is because, by definition, clinical recovery factors are not included in personal recovery- “it involves the development of new meaning and purpose in life with or without the presence of clinical symptoms” and hence are not theoretical related constructs. Besides, convergent validity is demonstrated when a test correlates highly with other variables or tests with which is shares an overlap of constructs e.g. 0.5 correlation [2]. Clinical factors (except depressive symptoms as measured by CDSS) had lower than 0.5 correlation with QPR-15.

We have thus proceeded to edit this. Please see paragraph one of discussion section. This was also further discussed in later paragraphs of the discussion, please see p21 line 16-26. Additionally, this required some edits on our second aim of the study in introduction, please see second paragraph of introduction, p4 line 13-26 and p6 line 8-15.
11. Page 19, line 7: what limits do you apply, and what source, for "high correlations"? There are certainly no generally agreed-upon limits, but I assume Cohen, or a more recent follower, could be a reliable reference? (Cohen J. Statistical power analysis for the behavioral sciences. Hillsdale NJ: Lawrence Erlbaum; 1988.)

Thank you for pointing this out. We adopted the limits by Hinkle et al. (Hinkle, D. E., Wiersma, W., & Jurs, S. G. (2003). Applied statistics for the behavioral sciences. Boston, Mass: Houghton Mifflin.). We have added the interpretation limits and reference in section 2.4 statistical analyses, p11 line 20-21. We have also edited all the correlation size accordingly.


We have reworded this sentence as per your point 15 comment that there are repeated sentences in two separate paragraphs, and therefore the reference is no longer needed in this paragraph.

13. Page 24, line 5: you imply inpatients cannot give informed consent. I think this is a prejudiced thinking.

We apologise for this and have removed “whom might provide informed consent”. Please see p22 line 23-24.

14. Page 25, the conclusion: this section is very brief and does not clearly reflect your two aims. So further rework is needed here.

Thank you for your comment. We have proceeded to reword this whole section and focus on reflecting on our two aims.

15. The discussion needs thorough reading for clarity, logical reasoning and proper language. Each sentence needs to be clear in itself and be part of a logical flow of reasoning. And the
language must suit a journal such as BMC Psychiatry. This may sound as truisms but in its present form the discussion does not meet these standards. Just a few examples:

Page 20: line 1, should be "was no longer…"

We have corrected this, please see p19 line 6.

Page 20: lines 7-8 repeat page 19 lines 9-10

We have reworded our sentence in the first paragraph of the discussion. Please see p 19, line 1.

Page 20: line 14 should be "range of…was…"

Thank you for pointing out on the grammatical error. We have reworded this sentence, please see p19 line 17.

Page 21: sentence starting line 15 is strange and makes no sense.

We have edited this sentence and restructured the flow of our discussion accordingly.

Page 22: sentence starting line 4 does not make a logical flow in relation to the preceding one. I suggest you delete the first part and just insert "Moreover", such that this sentence reads "Moreover, WHOQOL-BREF contains items…".

We have restructured this paragraph, please see p19 line 17-26.
Page 23, lines 13-14: what exactly do you mean? You indicate that QPR estimates psychological factors. Besides, the reasoning is circular - QPR/personal recovery reflects psychological factors, important to personal recovery.

We acknowledge that it is a circular reasoning and have removed the sentence.

Page 24, line 19: Please change Leamy et al. into BMC reference format.

We have edited this, please see p23, line 8-9.

Page 24, lines 24-26: another example of circular reasoning that needs to be corrected.

Thank you for pointing this out, we have since removed the sentence.

Page 25, line 15: you write "QPR-15 demonstrated that it…" By this you ascribe intentions and capacities to a thing. There are cases when this can be feasible, such as "this study showed that…" or "the discussion does not…", because it is very obvious that there are people involved. But your wording in line 15 almost assigns independence to the instrument. So please reword.

We have proceeded to reword with consideration of your comments. Please see the edited conclusion.

Again, I am positive to this paper and would like to see it published. Please don't let these comments make you believe anything else.

Thank you Dr. Eklund for your committed review and for pardoning our oversights. We appreciate your comments and reassurance. We have worked towards improving the manuscript accordingly.
References
