Author's response to reviews

Title: Self-esteem is associated with premorbid adjustment and positive psychotic symptoms in early psychosis.

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Author's response to reviews: see over
Dear Dr. Paul Lysaker,

Editor-in-Chief

BMC Psychiatry

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Dear Sir,

Thank you for your e-mail of the 14th of June. We have read the reviewer’s positive and constructive comments concerning our manuscript: “Self-esteem is associated with premorbid adjustment and positive psychotic symptoms in early psychosis” (MS: 122546419548455) and have revised our manuscript accordingly. Please find our responses to the reviewer’s comments enclosed.

We are hopeful that you will find the revised version of our paper to be of interest to BMC Psychiatry. We are looking forward to hearing from you.

On behalf of the authors,

With regards,

Kristin Lie Romm
Reviewer 1:
Comment #1: ‘The authors justify clearly the need for this study but in their review they do not address at all the issue of fluctuations in self-esteem as linked to positive symptoms, not just poor self-esteem – this should be mentioned and why fluctuations in self-esteem were not measured.’

We agree that the issue of fluctuations in self-esteem is of importance. We have added comments regarding this both to the introduction section (page 6, § 1), and to the ‘limitations’ section (page 15, §3) of the revised paper. We recognize that data on fluctuations in self-esteem would increase the value of the study. However, as this study was part of a larger follow-up study that included a broad range of assessments (both cognition and brain imaging measures) the size of the assessment battery was necessarily limited in order to avoid too much strain on the participants. This information has now been added to the paper.

Comment #2: ‘On p.5, following reference 23, there should be a sentence summing the information thus far’.

We have added a summarizing sentence at this point in the paper as recommended.(p 6, § 1)

Comment #3: ‘On p.6, first par: This is of obvious importance… It is not so obvious to the reader, perhaps add a bit here on why it is so important. And if subsequent life deceptions can affect self-esteem, then why choose a global measure that is known to be stable across time?’

We agree that we could have elaborated on this issue and have now added more information about this in the manuscript (p6,§2). It is correct that the Rosenberg Self-esteem scale primarily measures global self-esteem but, as we are interested in premorbid functioning, a trait measure might be better used to capture possible relationships between premorbid functioning and self-esteem.

Comment #4: ‘Please explain the Sobel test for the novices… ’

To clarify the statistical procedure we have rephrased the last sentence on p.10 (P10, last sentence) to read: ‘Finally we performed various interactional analyses to explore whether self-esteem worked as a mediator or moderator of the relationship between premorbid adjustment and symptoms.’

Comment #5: ‘P.13, following reference 14, the sentence is not clear, please explain – in fact grandiose delusions often hides poor self-esteem – in fact it is recommended to measure positive and negative self-esteem separately since grandiosity can be linked to high positive self-esteem and high negative self-esteem as well.’

We recognize that this point is poorly explained in the original paper. Due to the complexity of the existing analysis we chose to keep the positive and negative dimensions as one (they are strongly inter correlated, above .95). In order to avoid confusion we have deleted the sentence
from the manuscript.

Comment #6: ‘P.14, 2nd par: another explanation for the gender effect might be trauma? Women are more likely to have experienced sexual trauma and that is linked to self-concept and to positive symptoms. Has weight gain linked to medication also been measured? Women are often more affected in terms of their self-image then men in terms of weight gain.’

We agree that the influence of trauma and weight gain is of interest in the area we have studied. Unfortunately no assessments of trauma or of weight gain were performed with respect to our study. Since they are of obvious importance to later studies we have added a comment to that effect in the discussion section of the revised paper (p15, §2).

Reviewer 2:
Comment #2a: ‘Clarify the age range of childhood used; was it birth to 11 years?’

We have clarified this issue in the revised paper (p.9, §1).

Comment #2b ‘Note the time range of when the assessments were completed.’

In the second paragraph on page 7, we have described the time range for the assessments i.e from February 2007 to October 2009.

Comment #2c: The age range of 18-65 is quite broad; 65 seem quite unusual to have untreated psychosis, please explain.

The peak age of onset for schizophrenia spectrum disorder is in the late teens and early twenties. Population studies have shown that these disorders can also arise de novo in people in their thirties and forties, with a possible second peak in females around menopause. Although the onset of schizophrenia before puberty and after the age of 60 is very rare, they have been described and are known as ‘very early’ and ‘very late’ onset schizophrenia, respectively.

Studies of first episode psychosis tend to originate in youth and young adult services. While an age adapted service is very useful clinically, recruiting from only these services risks creating a sample selection bias. We have thus included patients from the whole range of adult services. Our inclusion criteria follow those of the adult services (18-65 years). Since the majority of patients are in their mid-twenties, the age range in the present study is in line with other first episode studies: mean 25.8, SD 7.7. This is noted in Table 1.

Comment #2d: Explain the median duration of untreated psychosis as 78 weeks (range 0-1040). What does the N=106 mean? You reported 113 patients as being in the study. Were patients excluded, and if so, why? Also provide an explanation about those (I assume it was 1 person) who had 1040 weeks, or 20 years of untreated psychosis.

The number N=106 refers to the number of individuals with a valid measure of the Duration of Untreated Psychosis (DUP). Unfortunately there were 7 individuals with missing or invalid
information for this variable. Since DUP was not the main focus of the current study they were not excluded. As correctly noted by the reviewer, there is one subject with a very long DUP of 1040 weeks. This finding has been thoroughly discussed by the assessment team and it appears valid i.e. that the person has actually experienced psychotic symptoms for this period of time without seeking treatment. Even though it is a rare phenomenon, naturalistic studies may include persons that have avoided treatment for surprisingly long periods of time as shown in Melle et al, 2004.

Comment #2e: Provide validity/reliability for the RSES and PAS. Who collected this data?

All data was collected by qualified medical doctors who have completed or are undertaking psychiatry specialization and clinical psychologists with training in the use of the study instruments. The RSES was filled in by the patient during the interview session, while the PAS was based on the contents of an interview with the patient. Both measures have been used extensively in this patient population previously. They pose simple, straight forward questions with good face validity, and the context of the interview is such that the patient can ask questions to the interviewer for clarification purposes. The current study uses a broad range of assessments. The reliability training and reliability assessment have focused on central clinical instruments (e.g diagnostics, symptom measures). There was a limitation to the extent of reliability testing in the study as comprehensive reliability testing involves an extended time frame for the interview (videotaping etc). We agree with the reviewer that reliability measures for more of the central measures would strengthen the study.

Comment #4: The manuscript adheres to relevant standards for reporting except for Table 1, in which gender is reported as having a "mean." In the body of the paper, it states that nearly 1/3 of the sample is women, but this is reported as a mean value, and needs correction.

We agree that there is an error in table 1 regarding gender. This has been corrected in the revised manuscript.

Comment #5: The discussion is mostly well written, however I would question the addition of reference 34. I'm not really sure that the reference to social ranking theory is relevant for this paper. I would suggest removing the first sentence of the last paragraph on page 12 unless you explain it more fully and link it to the findings of this study. The conclusions are well balanced and adequately supported by the data. I would suggest for future research, in addition to looking at changes in self esteem which may occur over the course of the illness, to also investigate related factors, such as stigma or metacognition, that could promote or degrade self esteem.

We agree with this comment and have excluded the social ranking theory from the discussion as we found it problematic to elaborate further on this in the present context. Regarding the conclusion, we have included factors associated with stigma and metacognition as related factors relevant for future research.

Comment #6: The limitations of the work are clearly stated.

Thank you!
Comment #7: The authors clearly acknowledge work upon which they are building, however I
would recommend that the authors consider including the following articles to their review in the paragraph starting at the bottom of page 4:


*We agree that the suggested literature is relevant in this context. We have followed the reviewer’s advice and incorporated these papers into the review.*

Comment #9: The authors should add some statements about treatment implications. For example, they might add that psychotherapy interventions could be geared to increase self-esteem through CBT which could target beliefs about oneself. Also they should mention that psychotherapy can assist the patient in developing a richer personal narrative in order to deepen their self experience and improve self-esteem. Based on the authors' findings, these interventions may be expected to lessen the likelihood of development of or to decrease the severity of persecutory delusions and hallucinations.

*As suggested by the reviewer we have added more statements about treatment implications (P.16 , §1).*

Comment #10: The writing, however, is not acceptable and needs much revision. There are numerous grammatical errors, words which are not spelled correctly, and also some sentences which do not make sense. For example, the first sentence of the introduction would be much better if worded with the following: "Self-esteem, a global and complex concept, is comprised of appraisal of self-worth....." Also, I would recommend re-wording the last sentence in the first paragraph at the top of page 6, and many sentences in the last paragraph starting at the bottom of page 13 and continuing to the top of page 14. In terms of format, the Conclusion paragraph needs to follow the same form as the other sections. Table 4 has all commas instead of decimal points.

*Table 4 has been edited with decimal points. The manuscript as a whole has undergone a comprehensive editing process.*