Author's response to reviews

Title: Exploring health-related quality of life in eating disorders by a cross-sectional study and a systematic review

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Author's response to reviews: see over
Exploring health-related quality of life in eating disorders by a cross-sectional study and a literature review

REPLIES

1. Reviewer: Waguih W IsHak

Major Compulsory Revisions: none
Minor Essential Revisions:
   1. The WHO definition of QOL should be cited exactly as defined by the WHO cited in the article and should not be re-worded to state: "the individuals' perceptions of their position in life in the context of the culture and value systems in which they live and in relation to their goals, expectations, standards and concerns."
   R: modified according to suggestion. Moreover we added a more specific explanation about HRQoL
   2. The literature review idea to put the results of a cross-sectional study is creative. However, I would shy away from calling it a systematic review (which entails a thorough assessment and synthesis of all available evidence including appraisal of the strength of the evidence, which was not performed here).
   R: we modified the text and the title of the paper according to this suggestion

Discretionary Revisions: none
Level of interest: An article of importance in its field
Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.

2. Reviewer: Greta Noordenbos

MINOR ESSENTIAL REVISIONS
The title of this article is 'Exploring health-related quality of life in eating disorders by a cross-sectional study and a systematic review'
OVER ALL: This is a very interesting and relevant article.
Most articles however start with a review of the relevant research literature before they present the empirical research. In this article this order is reversed. A suggestion might be to change the order: first present the review of the literature and then present the empirical study.
   R: we understand this point, and we have considered this option before writing the paper. In the end, we preferred to present first the empirical research, in order to put it among the other contributions of the literature. We would like to preserve this order, but we are ready to change if the editor thinks so.

TABLES
In all tables (1a, 1b abd 2) you present the subgroups of EDs. You start with BN and then mention AN followed by BED and EDNOS. However it might be more logical to start with the AN subgroup, followed by BN, BED and EDNOS. (see my suggestion below in table 1b)
Table 1b: Socio-demographic and clinical characteristics of ED patients: categorical variables
TABLE 1B IS DIFFICULT TO READ, because several numbers are presented in one line meaning different things. Also the percentages are missing, which make it difficult to compare the numbers of the subgroups. My suggestion for correction is presented below and worked out only for the AN group as an example.
Table 1b: Socio-demographic and clinical characteristics of ED patients: categorical variables

<table>
<thead>
<tr>
<th></th>
<th>AN (n=33)</th>
<th>BN (n=26)</th>
<th>BED (n=7)</th>
<th>EDNOS (n=14)</th>
<th>Etcetera</th>
</tr>
</thead>
<tbody>
<tr>
<td>Occupation</td>
<td>yes 7 (%)</td>
<td>no 26 (%)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Marital status</td>
<td>unmarried 29 (%)</td>
<td>married or coupled 3 (%)</td>
<td>divorced or separated 1 (%)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

This way of presenting table 1b make it much easier to compare the subgroups!

R: absolutely correct. We modified tables 1a, 1b and 2.

QUESTION: The number of BED patients is only 7: is this enough for a statistical analysis?

R: we are aware that with small sample size the statistics may not follow a normal distribution, so we used a non-parametric test (Kruskall-Wallis test), which in principle does not put limits in the sample size. We added a brief comment on this in the discussion.

RESULTS:

HRQOL SCORES (PAGE 7)

It might be interesting and relevant to describe here whether the HRqol scores were significant for the several weight groups (BMI groups): under weight, normal weight, overweigth and obese. This question is relevant because the BMI of the AN group is quite low (15.97 =underweight) and the BMI of the BED is quite high 32.40 (SD 8.26!) which means overweight or even obesity. Higher weight of the BED might explain their poorer quality of life (see also the second * point in the discussion (page 10).

R: this comment introduces an important issue. The question is whether the QoL depends upon the psychopathology or upon the weight of the subjects. The DSM-5 is quite ambiguous in this point, since it states that the severity of Anorexia is related to the weight (i.e. the BMI), while the severity of Bulimia and BED is related to the behaviour (compensatory behaviours or episodes of bing eating). In this study we chose to consider the psychopathology (i.e. the diagnosis), which is anyway strictly correlated (at least for AN and BED) with the weight of the subjects.

LANGUAGE

On several places you write: an history: might that be: a history? (please check the language here).

R: done

3.

Reviewer: ANA R. SEPULVEDA

This paper reports the results of a cross-sectional study of 80 female ED patients between 13-61 years of age (an heterogeneous group; mean=28 years). The authors have two different aims for the study, which is then presented somewhat awkwardly in the Results and Discussion sections. Firstly, to explore whether HRQoL differed among ED subgroups at the beginning of the psycho-nutritional programme Veneto (Italy), and the second aim was to investigate the effects of comorbid DSM-IV diagnosis, setting of care and history of previous treatment on self-perceived HRQoL in
ED patients. Another aim of this study was to do a systematic review of the HRQoL (scale of 26-items that measures subjective perception of quality of life) in ED patients. The issue of eating disorders and quality of life are very relevant for treatment outcomes and are a topic of interest in this clinical field. However, the study presents several weak points related to number of conceptual and methodological problems. I do not believe it is suitable for publication as presented in its current form.

1. Is the question posed by the authors well defined? No, the study would require dividing the different aims into two different studies (recommended study 1 and study 2)
   R: there are several examples of papers that include both an empirical study and a review. We live this decision to the Editor.
2. Are the methods appropriate and well described? Both could be better explained following the suggestion above.
   R: idem
3. Are the data sound? If presented in two studies, the data would be better explained, now they are fairly limited.
   R: idem
4. Does the manuscript adhere to the relevant standards for reporting and data deposition? The data follows presentation standards.
   R: ok
5. Are the discussion and conclusions well balanced and adequately supported by the data? Discussion and conclusions would be more coherent if the paper is divided into two studies. At the moment it is somewhat chaotic and difficult to follow.
   R: see above
6. Are limitations of the work clearly stated? The data of the cross-sectional study is correct, however the systematic review requires more in depth discussion.
   R: we modified the discussion, introducing a limitation of the study and organizing the discussion itself in order to put in the right order the issues concerning the characteristics of the sample, the QoL of our ED groups and the review.
7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished? Yes, the authors’ knowledge the topic and appropriate referencing is included.
   R: ok, thanks
8. Do the title and abstract accurately convey what has been found?
   R: I presume is ok
9. Is the writing acceptable? Yes
   R: ok, thanks

I have thoroughly gone over the manuscript and it would be necessary to do several and relevant amendments:

Major Compulsory Revisions

1. No data are reported about how the comorbidity diagnoses of patients were made. Were they done by clinical interview by health care professionals or a revision of medical history for each patient?
   R: the comorbidities were assessed taking in consideration the medical history, and confirming their presence with the clinical assessment. This was added in the paper.
2. In the systematic review section, the authors conclude that the SF36 is the most widely used instrument to assess patient’s quality of life. Why was the HRQoL used then? Was the systematic review done after the cross-sectional study?
   R: the review was done after the cross-sectional study. Both SF-36 and WHOQoL-Bref are in fact widely used instruments. We chose the WHOQoL-Bref since it is a WHO instrument suitable to measure quality of life in these patients.
3. I would recommend dividing the paper into study 1 and study 2. Therefore aims, method and results would be distributed differently in the manuscript, and hence, the discussion would also be clarified.

R: see above

The Discussion and Conclusion require developing and expanding more. For example, Why haven’t differences been found for HRQoL between inpatients and outpatients?, or patients with comorbidity and those without?, nor correlations with illness duration. This should be addressed and discussed.

R: a comment on the absence of differences between in and outpatients was already present. We added a further comment on comorbidity and illness duration.

Unfortunately there are very few data from the literature to be compared.

Furthermore, regarding ED diagnosis; the revised articles report differences in life quality between BED and obesity and AN and other ED diagnoses, while in the current study no differences are reports. Why is this?

R: this sentence is not clear. If the issue is the presence of obesity, we did not consider the overweight as a covariate because the number of BED was too small. On the contrary, the studies focusing on obese patients selected specific populations of obese subjects, often candidates to nutritional or surgical interventions.

Why in the SR table are the patient’s clinical data not reported (comorbidity, illness duration, type of treatment) so as to homogenise the comparison and discussion of the results.

R: Many papers do not report data on comorbidity and illness duration, so that a comparison among studies is not attainable. On the other hand, we included separately the studies where specific treatments were identifiable.

Minor Essential Revisions

1. On the 5th line of Discussion section, please correct “outpatients and inpatients.”
R: done

2. Add notes below the Systematic Review Table in order to make the table more legible.
R: we introduced a glossary of the ED instruments included in the table

3. Table 1b: DSM comorbidity among AN group 7/33? Is this correct?
R: correct

4. Table 1b: I think it may be better to use percentages instead of N.
R: done

5. Are there any suggested cut-off points for the instrument? Were they used? Perhaps significant differences between groups (ED subgroups, inpatient and outpatient groups) could have been found amongst patients that reached the HRQoL cut off point and those that didn’t.
R: There are no established cut-off points for WHOQoL-Bref

6. The means and SD for questionnaires are missing in the SR table. Without these results between studies cannot be carried out.
R: we introduced in the methods a sentence explaining the criteria followed in reporting data: “… means and SD where not shown since these statistics were not always inferable (for example, they were shown in figures and not in tables) or else were too many (one statistic for each ED and for each subscale) to be included in the table. Moreover, apart from studies using SF-36, most statistics were taken with miscellaneous instruments and thus were not comparable.” Besides, following also the suggestion of one of the other reviewers, we gave up the idea to call this a systematic review. As a simple review, we think that it is acceptable to present data in a descriptive form. For example, among the latest papers, Scope et al. (“Is group cognitive behaviour therapy for postnatal depression evidence-based practice? A systematic review” BMC Psychiatry 2013, 13:32) reported in a table a summary of the
main outcomes for each paper, without means and SD (even if this was called a systematic review!). In other reviews we can find a table including means and SD only when available in the single studies (see for example Ikeda et al.: “Assessment of quality of life in children and youth with autism spectrum disorder: a critical review” Quality of Life Research 2013), but this introduces some inhomogeneity in the table itself.

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.