Reviewer's report

Title: Quality of Life Impairment Associated with Body Dissatisfaction in a General Population Sample of Women

Version: 3 Date: 8 May 2013

Reviewer: Petra Graham

Reviewer's report:

The authors have presented an article that examines a body dissatisfaction outcome measure against various quality of life indicators. Their analysis involves the dichotomisation of all variables and then uses either chi-squared analysis or logistic regression. The paper requires extensive revision before it can be considered for publication.

Major compulsory revisions:

1) Binary outcome

The former reviewer is correct that it is not appropriate for the authors to have used univariate logistic models. Not only can bias and loss of information occur but there are also statistical problems with this in that the estimated probabilities of the event of interest do not take into account the other categories. While the authors claimed not to be interested in the ordinal nature of BD, they then use posthoc tests for pairwise differences that essentially show the ordinality, a linear trend test for the chi-squared analysis and the first paragraph of the discussion treats BD as ordinal in its interpretation. So ideally the authors would redo the analysis using BD as ordinal and using an appropriate model as suggested by the previous reviewer. Alternatively, if the ordinal model is not used, the authors must use multinomial logistic regression which gives similar output and interpretations to what the authors currently have but preserving the necessary statistical properties. The authors would then have to be careful about the use of statements referring to ordinality in the outcome beyond noting that estimated odds are increasing with increasing levels of BD.

2) Dichotomisation of the predictors

This seems an extraordinary level of dichotomisation. I cannot see how this is necessary because retaining the variables in their original form is much more informative and avoids any bias in the choice of the point at which the dichotomisation is made. It will still be possible to interpret the results in terms of poorer quality of life. As it stands it is very difficult to tell from the labels for the rows in the tables what the interpretations should be as these are no longer SF-12/WHOQOL items; more informative labels and more interpretation in the results would be better for readers.

3) Was the sample size for all analyses 5159? The paper needs to clarify how the missing values were dealt with (i.e. all missing values were excluded at the
4) The sensitivity analysis regarding exclusion of the eating disorder subgroup should be noted in the methods and the results included in the paper.

5) Because of the large number of multiple comparisons p-values should be reported to 3 or 4 decimal places.

6) It is not clear to me what the authors mean by "stronger associations," the authors should be clearer in their interpretations as to what they mean by this.

Minor Essential Revisions

1) I'm not sure of the benefit of describing the correlations between scores and the outcome variable. It would be much nicer to incorporate both via an appropriate ordinal or multinomial model as noted above.

2) It would be nice to see a comparison of the QOL measures for the levels of BD to the norms.

3) The authors should include information about which statistical package was used to analyse the data in the methods section.

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

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I declare that I have no competing interests