Reviewer's report

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

Version: 1 Date: 20 August 2012

Reviewer: Kyle De Young

Reviewer's report:

The article titled “Quality of life impairment associated with body dissatisfaction in a general population sample of women” describes a study that tests the relationship between body image dissatisfaction and quality of life in a large epidemiological sample of Australian women. The results indicate that body dissatisfaction is associated with both physical and mental quality of life as well as subjective quality of life such that worse body dissatisfaction is paired with poorer quality of life. In general, this study is methodologically-sound, uses a large, representative sample, and makes a meaningful contribution to our understanding of the public health importance of body image dissatisfaction. The paper is well-written and organized, and the tables aid in the interpretation of the results. A few comments are listed below that, if addressed, would improve the comprehensibility of the manuscript. With one exception, these are all very minor, which should be taken as an indication of the quality of this paper.

To begin with the most minor and nit-picky (discretionary):

1) On pg. 7, in the description of “Subjective Quality of Life,” the authors write “…domains relating to the individual’s subjective evaluation of their physical health…. Note that the subject (the individual) is singular and the pronoun (their) is plural. Elsewhere in the manuscript the authors appear to have been careful about agreement between subject and pronoun, so this is likely an oversight but should be fixed.

2) On pg. 9, in the last paragraph before the results, there are covariates listed that are separated by commas and semi-colons. It is not immediately apparent why they alternate.

A little less minor (minor essential):

3) Please explain in more detail the rationale behind averaging the two EDE-Q items and then re-categorizing them on the 7-point scale. It seems to me that there is an implicit argument that these data are ordinal rather than continuous (hence the ANOVAs to compare folks who chose different response options). However, the authors also conducted Pearson correlations, which would seem to make the opposite argument (i.e., that the BD measure is continuous). Re-categorizing the BD scores after averaging sacrifices a non-negligible amount of variability, which the authors would probably prefer to preserve for analyses that treat BD as continuous. For example, someone who answers a “1” and a “0”
is grouped into the same category as someone who answers a “1” and a “2”.
These two people are probably meaningfully different, and the variability that they contribute to the BD variable may improve the Pearson correlations. I think you can make an argument for both cases (i.e., treating the variable as ordinal and categorical) in separate analyses, but it is recommended that this should be made explicit and described a little more clearly in the statistical analysis section.

4) The results of the logistic regressions are a little confusing. My best attempt at understanding this analysis is that in comparison to people who report no BD (i.e., “0”), people who report each of the other levels of BD are at increasing likelihood of endorsing the various measures of QOL listed in Table 3. Elaborating the last paragraph on pg. 10 would likely be all that is required to hold the reader’s hand a bit more through the interpretation of these results. Also, please elaborate on the explanation provided for why increasing BD is associated with a lower likelihood of impairment in QOL for moderate activities. This seems counter-intuitive given the rest of the pattern of results.

Finally, the least minor (major compulsory):

5) I’m not convinced that excluding “probable eating disorder cases” is a sufficient procedure to conclude that “the observed associations between BD and quality of life impairment could not be accounted for by an association between BD and eating disorder symptoms.” First, it would be helpful to include more information about how these probable cases were identified in addition to citing a previous study. Second, this method of ruling out eating pathology as being responsible for the pattern of results seems inconsistent with the approach taken to rule out other possible explanations (e.g., BMI) that were included as covariates. If one takes the position that eating disorder symptoms occur on a continuum of pathology, then excluding probable eating disorder cases only removes the most pathological portion of variability along this continuum. It is possible that eating disorder symptoms still account for some of the association between BD and QOL even after excluding these cases. It appears as though the authors have at their disposal the EDE-Q. Why not use the global score (minus the two BD items) or one or more of the subscales as continuous covariates? Similarly, binge eating and purging frequency could be used as covariates, although their likely non-normal distribution may be more problematic and therefore a less attractive option. After controlling for eating disorder symptoms in this way, it would be helpful to see the results listed in more detail. Perhaps this control can be added from the start, so that the tables describe the relationship between BD and QOL independent of all of the covariates (including eating disorder symptoms), which seems to be the purpose of this study.

Thank you for the opportunity to review this manuscript.

Kyle De Young, Ph.D.
Assistant Professor of Psychology
University of North Dakota
**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.

**Declaration of competing interests:**

I declare that I have no competing interests.