Reviewer's report

Title: Domains of disgust sensitivity: Revisited factor structure of the Questionnaire for the Assessment of Disgust Sensitivity (QADS) in a representative German survey

Version: 3 Date: 5 June 2010

Reviewer: David P Valentiner

Reviewer's report:

I have served as a reviewer on earlier drafts of this manuscript. The revision has been quite responsive. I particularly appreciated the use of a more sophisticated analytic procedure. Here are my concerns with the latest version:

(1) The prose continues to be awkward. English is my first (native) language. As I read the article, I frequently observed that the authors are not native speakers of English. In most instances, the awkwardness of the prose is quite manageable and without substantial consequence. In some instances, the awkwardness had more substantial consequence. I have included some comments to assist with editing for basic language issues, but additional editing is needed. For example, the text describing the results of the EFA is quite problematic. For example, in the following sentence (lines 10-12 on page 6, beginning with “Throughout all the solutions, factor four was . . .”), do you mean something like: “In each of the rotated factor solutions, at least two items that had salient loadings on the fourth factor were complex items, i.e., also showing substantial loadings on other factors.”? Please consider additional editing by a native English speaker.

(2) Why is Scale 1 called “Core Disgust” rather than “Core Disgust Sensitivity” and why is Scale 3 called “Contamination-Based Disgust Sensitivity” rather than “Contamination-Based Disgust”? The paragraph in the middle of page 10 makes a strong case for using the term “disgust propensity” rather than either “disgust” or “disgust sensitivity.” Rather than indicating who is “sensitive to disgust” (3rd line from the bottom on p. 9), these items appear to indicate who is ‘more reactive to disgust eliciting stimuli.’

(3) The fit indices from the CFA indicate an inadequate fit. This issue is not fatal, though it does decrease my enthusiasm for the instrument. Consider fitting a model with secondary loadings. At a minimum, recognize the inadequacy of the simple structure in the discussion section – I think that “This model acceptably fits our data . . .” (line 16 on page 8) needs to be more nuanced. These items are apparently quite complex. The distinct meanings of the factors underlying these scales may not be clear unless multivariate approaches are used, in which the effects of the other scales are controlled for. Given the inadequacy of the fit, I speculate that a one-factor model might not be much worse than the three factor model. Consider doing some comparative model fitting.
(4) Were the tests of differences in mean levels of the factors/scales done within Mplus (i.e., as tests for differences in means of latent variables), or using traditional independent samples t-tests? The text lists capital “T” – should those be lower-case “t”s?

(5) The characterization of disgust as “a weakness” (last line on p. 9) is without basis. The tendency to experience disgust in response to contaminated foods could be interpreted as strong, healthy, and adaptive. The items in question (e.g., using terms like “hair” and “bad breath”) are not well described by “less tasty food” (last line on p. 9).

(6) Why were ANOVAs used to examine the association of the scales with age? I had expected correlations. Please give a rationale. If ANOVA is to be used, then what were the cell sizes? The discussion of these results using terms like “age-independent” (5th line from the bottom on p. 9) is misleading. I do not agree that there is any implication of “a time-stable trait” (5th line from the bottom on p. 9).

(7) I assume that the sample has been randomly divided into an exploration subsample (for the EFA) and a validation subsample (for the CFA), but I did not see this explicitly stated.

(8) Construct validity is not “a given” (1st line of p. 10), not at least at the scale level.

(9) The discussion of the DPSS (top of p. 10) does not seem relevant. At a minimum, there are at least two subsequent studies of this measure that address at least some of the concerns raised.

(10) The use of the phrase “theory-driven” (line 20 on page 3) seems inappropriate. EFA is “data-driven.”

(11) It is not clear what criteria were used for deeming items appropriate for inclusion in the EFA, nor what the rationale was for selecting those criteria. Can a more specific citation (specific article or chapter, or page numbers in a book) be included for the use of Mardia’s multivariate skew and kurtosis (on page 5).

(12) Please give a fuller description of the CFA. How and which many items loaded on each of the factors? What were the range of loadings and were they significant? What were the correlations among the latent variables?

(13) The text “. . . only three of the eight factors were found to be psychometrically stable . . .” (page 2, line 13) is problematic because factors and subscales are conflated. Again, “QADS factors inherited very good reliabilities . . .” (line 16 on page 9) uses “factors” and “scales” as if they were interchangeable. They are not. This happens throughout the ms.

(14) Factors are hypothetical constructs that do not exist in nature, but rather are conveniences for understanding the data at a reduced level of complexity. I am able to glean the value of these data despite the lack of a nuanced treatment of
these issues. Perhaps the editor can evaluate whether such issues (conflating factors and scales, and reifying constructs) are sufficiently important to require revision.

Minor concerns:

(15) Acronyms are used without being introduced/defined: USUMA (last line on page 3) and ADM (line 2 on page 4).

(16) Vague or unfamiliar terms are used without being adequately explained: “random-route-method” on line 2 of page 4, “item analysis” on the second line from the bottom on page 4, and “overall mean appraisal” on line 16 on page 5.

(17) The alphas for the five are reported (on p. 4) before the readers know what the five scales are, where they come from, etc.

(18) Is the parallel analysis (pages 5-6) conducted at the mean (50th percentile), the 90th percentile, or at another level?

(19) The criterion of “> .30” is appropriate (though perhaps a bit low) as a guideline for determining salience, but not by itself for determining whether an item is a hyperplane item (line 7 on page 6).

(20) Should “. . . while Kappa = 5 led the least number . . .” be “. . . while Kappa = 5 led to the least number . . .” on line 9 of page 6?

(21) I do not know what “. . . while controlling for convergent validity” (line 13 on page 9) means.

(22) An alpha of .69 is described as “good” (lines 5-6 on page 3), yet an alpha of .66 warrants the generation of additional items (last 2 lines on page 2). That seems inconsistent.

(23) There are formatting problems on page 3. Perhaps the blank line (line 8) should be omitted (see also line 13 on page 8). It is not clear why the following sentence ends with a colon.

(24) Should “Quads” be “QADS” (line 13 on page 3)?

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

**Quality of written English:** Needs some language corrections before being published

**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.