Reviewer's report

Title: Factor structure, discriminant validity and standard percentages of the Questionnaire for the Assessment of Disgust Sensitivity (QADS) in a representative German survey

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Reviewer: David P Valentiner

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Review of “Factor structure, discriminant validity and standard percentages of the Questionnaire for the Assessment of Disgust Sensitivity (QADS) in a representative German survey”

I reviewed an earlier version of this manuscript as reviewer #2. Overall, the authors appear to make a good faith effort in addressing concerns associated with the initial version of the manuscript. The topic remains of interest to readers and represents an important area of research. However, some key issues remain in the revised version, and these issues preclude definitive conclusions about this revised manuscript. Here are my major compulsory revision issues:

1. The clarity of written expression remains a problem throughout the manuscript. Many parts of the manuscript seem hard to follow. I had to reread some parts many times to understand what the authors were intending to say. The following two examples are presented as illustrative and are not an exhaustive list of the writing problems:

   a. On the first page of the manuscript the author’s write:

   “Recent research about anxiety disorders such as specific phobias and obsessive-compulsive disorders (OCD) suggests that not only fear, but also disgust, plays a possible role in the etiology as well as for maintenance [01]. In addition, several other mental disorders are thought to be characterized by altered disgust processing [02]. At its peak, disgust much more so than fear leads to avoidance behavior [03]. However, the results concerning eating disorders and depression are diverse [04, 05] perhaps due to the still ongoing enhancement of the instruments.”

   It is unclear why the authors jump from talking about anxiety disorders to discussing eating disorders and depression. Is it because these two disorders are marked by “altered disgust processing” as well?

   b. Also on the first page of the manuscript:

   “Hereby, one might assume that individuals with a higher disgust sensitivity would be more easily triggered by related stimuli and show a more intense and longer disgust reaction.”
What is being triggered? My guess is that the individual is prompted to experience increased disgust reactions, but it is unclear from the present phrasing.

2. My most central concern, which I believe precludes a meaningful interpretation of the results, still resides in the way in which the factor structure of the QADS is analyzed.

a. In the last version of the manuscript, my recommendation was to use a variant of Weighted Least Squares approach (WLS) due to the apparent categorical nature of the QADS data. However, it is clear in this version of the manuscript that the QADS is actually represented on a presumably interval scale (5-point scale), which was unclear from the original version of the manuscript. For this reason, WLS does not need to be used.

However, the authors make the argument that an alternate form of estimation, other than maximum likelihood (ML), needs to be used due to the non-normality that appears to plague disgust measures, including the QADS. If this is their position from the beginning of the analyses, why is ML used for the initial, and later, CFA? Based upon their argument, might the initial lack of support for the original factor structure of the QADS be a result of them using ML instead of a more appropriate estimation procedure?

That being said, and as noted above, I am not convinced that ML is inappropriate to use in the present study. In assessing non-normality, the authors use the Shapiro-Wilk test of normality. Although this test is appropriate for testing univariate normality, univariate non-normality does not seem to affect ML as much as multivariate non-normality does, as ML is robust to minor departures in normality. Multivariate normality can be assessed via Mardia’s statistic. Further, if normality does appear to be an issue (univariate or multivariate), robust ML [Satorra, A., & Bentler, P. M. (1994). Corrections to test statistics and standard errors in covariance structure analysis. In A. von Eye & C. C. Clogg (Eds.), Latent variable analysis: Applications for developmental research (pp. 399-419). Thousand Oaks, CA: Sage.] can/should be used to correct for this concern.

Further, the WLS approach used by the researchers is inadequate. If WLS is used, the preferred method is to use robust WLS (WLSMV), which I think is only available via the Mplus statistical package at the present time [see Brown, T. A. (2006). Confirmatory factor analysis for applied research. New York: Guilford Press] for a discussion of the problems with regular WLS).

b. Another concern seems to be in regards to the use of Parallel Analysis (PA): Although PA does seem to be a favored method to use in determining how many factors to retain from EFAs, I am unsure of a PA program for a WLS EFA. I may be wrong on this subject, and would be pleased to know if a PA program for this type of EFA estimation exists. That being said, it would be nice for the authors to state in text what the eigenvalues for the PA were, as well as the EFA, so the reader can compare the PA results to the eigenvalues from the EFA.
c. The adequacy of the revised QADS factor structure is questionable at best. The authors cite Hu and Bentler (1999) as the benchmark for goodness-of-fit for the first CFA. Based on Hu and Bentler, the fit of the second CFA would not be adequate. All of the fit indices of this second CFA are well below Hu and Bentler’s recommendations. Further, the only fit index that could be considered adequate would be the RMSEA, as some authors advocate for a more lenient cutoff (< .08) than do Hu and Bentler. Regardless, only one of the fit indices reaching an appropriate level does not indicate an adequate solution. Also, the chi-square statistic is traditionally presented with the fit indices as well.

3. When talking about discriminant validity in the results section, the authors state the correlations are not meaningful due to the magnitude of the relations being < .10. This phrasing does not seem to properly describe whether discriminant validity is established. Further, in the discussion section, discriminant validity seems oddly characterized again. The authors’ state:

“Discriminant validity is given in regard to only a few constructs such as physical well-being. This weakness should be addressed by discovering interrelations to associated psychopathologies, i.e. blood and injury phobia-related cognitions, obsessive thoughts, and compulsive behavioral tendencies, or anxieties and fears in regard to panic disorders.”

Wouldn’t one expect the QADS to have fairly large correlations with the listed indices, as disgust is related to these conditions? Are the authors trying to make the point that the QADS should correlate more strongly with these indices than it would with well-being?

4. I am not sure of the practices of this journal, but the bullet points used in the discussion did not seem typical for other journals and seemed best placed in paragraph form.

**Level of interest:** An article of limited interest

**Quality of written English:** Not suitable for publication unless extensively edited

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I declare that I have no competing interests.