Author's response to reviews

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

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Author's response to reviews: see over
Dear Professor Clark,

Thank you very much for considering our manuscript “Factor structure and standard percentages of the Questionnaire for the Assessment of Disgust Sensitivity (QADS)”. In the following we address the issues raised by the reviewers point by point.

These are the most important modification:

- In order to be more precise about the research topic, we changed the title to “Domains of disgust sensitivity: Revisited factor structure of the Questionnaire for the Assessment of Disgust Sensitivity (QADS) in a representative German survey”.

- Concerning comments by Reviewers #1 and #2, we have deleted the paragraphs about convergent validity (FEW questionnaire) and the role of disgust / disgust sensitivity for the etiology of psychological disorders.

- Concerning comments by Reviewer #2, we have revised the estimation methods of both the exploratory and confirmatory factor analysis.

- Finally, we included the Main Score and its descriptives, reliability, and percentages.

We look forward to hearing from you.
Sincerely yours,

Katja Petrowski, PhD
Comments on Reviewer #1

Reviewer's report:
Thank you for the revised version of this manuscript. The authors carefully attended to the many concerns raised by the reviewers. I particularly appreciated the more thorough review of other existing measures of disgust, the relevant literature on the factor structure of disgust domains, and their revisions to the statistical analyses. The manuscript is clearly improved from its prior version, although additional revisions are suggested to help improve its suitability to the BMC readership and the broader scientific community.

Major Compulsory Revisions
1) The language of the manuscript has been markedly improved from the first version, although further tightening of the language is still needed. The clarity of the language in the Results section should be a model for the flow of the language in the introduction and conclusion. The Results section was quite clear and easy to follow. The Introduction continued to be disjointed in several places, but again, improved from its earlier form. For example, in the Introduction, word choice and grammatical structure were questionable and made it difficult to fully understand the authors' points (e.g., p.6: "The diverse results in the clinical field might be due to that the instruments with inhomogeneous item scaling have been developed from small specific samples or the scales' showing insufficient reliability."). Both the Introduction and the Discussion could use another review/rewrite in order for this paper to be published in a primarily English-speaking journal.

This is right. We further refined both sections and hope they will now meet with your approval.

2) Perhaps one solution to help tighten the manuscript would be to reduce the length of the Introduction and Discussion. In the Introduction, the authors could try reducing it to about 2 pages from its current 3.5 pages, staying focused on reviewing the factor structure of other disgust scales, justifying the rationale for a large scale replication of the QADS, and listing of hypotheses. The final 2 paragraphs in the Introduction can be dramatically reduced to just listing the hypotheses as several additional ideas were brought up in these paragraphs that did not seem particularly relevant and clearly made identifying the true scope and purpose of the paper ambiguous. This study is primarily a methods paper, and thus keeping their Introduction focused on this point would be helpful. Also, I would strongly suggest removing the term "area-unspecific" as this is ambiguous. Do they mean generalized disgust elicitors/domains?

For the Discussion, again, it could likely be reduced down to 2-2.5 pages. There were also some redundant sentences (e.g., last paragraph of p.13) that could be removed. The middle paragraph on p.14 was very well-written and should be a good model of how to craft the rest of this section for clarity.

A very good and helpful point. Hence, the discriminant validity and the role of disgust sensitivity in the etiology of mental disorders are no longer considered. The Introduction was shortened to 2 pages, the Discussion to 3 pages.

Minor Essential
1) In the Abstract, please use a term different than area-unspecific (see comment above). Please review the punctuation and remove or clarify abbreviations (e.g., AMD). Probably do not need to list alpha coefficient numbers in the Abstract.

The particular term have been deleted.

2) Consider removing data associated with the FEW. It really does not seem to add anything to the manuscript, and the low, but significant correlations appear completely accounted for by the large sample size. This could also help reduce the length of the manuscript. In doing so, Table 5 could be removed. I can see
how it can serve as a discriminant instrument, but it would seem that measures of negative affectivity, depression, and/or anxiety would be much stronger examples of discriminant validity.

*Thanks for this advice, we have removed this point.*

3) Consider removing Table 6 as presenting this level of detail does not appear necessary.

*Standard values are one of two scopes of this study. We still believe that this table is valuable to any practitioner using the QADS in order to assess individual disgust sensitivity. More important, no study using a representative sample has been published to our knowledge.*

4) In the Methods section, the last 2 sentences in the first paragraph on p.8 could be deleted. Under Instruments (p.8), just present the alpha coefficients after each subscale rather than repeating "Alpha" (e.g., Death (.85), Body secretion (.74), etc.)

*We rewrote these sentences.*

**Level of interest:** An article of limited interest  
**Quality of written English:** Needs some language corrections before being published  
**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.
Comments on Reviewer #2

Reviewer’s report:
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.

   This point was well-taken. We decided to delete all the sections which focus on the role of disgust & disgust sensitivity for the etiology of mental disorders. In general, we completely revised the manuscript and were assisted by a native English speaker. Hence, we hope to have eliminated all the confusing 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?

   In general, ML seems to be the most used estimation method in CFA and is reportedly robust towards minor non-normality (see Brown, 2006). However, we underestimated the influence of normality issues for CFA estimations; therefore we used the robust
Maximum Likelihood estimation [MLM] instead.

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. 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.

We decided to take the same approach as Olatunji et al. (2007) did in the analysis of normality. Therefore, the Shapiro-Wilk statistics are presented. We are grateful for the pointer about Mardia’s statistics, and have, thus, added it to the manuscript. Notably, non-normality was found as both univariate as well as multivariate. The other book by Brown (2006) turned out to be a valuable source for rationales. Accordingly, we used MLM for CFA and Principal Axis analysis for EFA. We thank the reviewer very much for his advice on this issue!

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).

WLSMV seems indeed to be a more appropriate estimation method, however, MLM is recommended over WLSMV by Brown (2006) and was implemented in our analysis.

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.

The PA was computed using a Principal Axis estimation (PAF) with raw data permutation. As PAF is recommended for the EFA in case of seriously non-normal data (Brown, 2006), this method appears indicated for the previous PA. Interestingly, it made no difference whether the PA was computed either using Principal Component estimation or Principal Axis estimation. EFA eigenvalues and PA eigenvalues are now presented.

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.

Another helpful remark by the reviewer. We have consulted different sources (e.g. Kenny 2010 via http://davidakenny.net/cm/fit.htm; Bentler 1990, Browne & Cudeck 1993) and are reporting the most common ones now. The Chi-Square statistics are presented as well. We are very aware that not all indices reached the recommended threshold, but we still think that this is the best available factor structure after using all the appropriate statistical methods and having a large, representative sample.
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?

**Discriminant validity is a very interesting issue, therefore we had wanted to address it. Nevertheless, the other reviewer found it to be more of a side issue and beyond the article’s main scope. Hence, we decided to leave this section out. Thanks to the reviewer’s interesting remark, we might investigate this issue in the future, regarding more appropriate instruments as described in the manuscript’s latest discussion.**

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.

**The bullet points were deleted.**

**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.