Author's response to reviews

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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Dear Professor Clark,

Thank you very much for considering our manuscript “Standard percentages and psychometric properties of the Questionnaire for the Assessment of Disgust Sensitivity (FEE) in a representative German survey.” In the following we address the issues raised by the reviewers point by point.

We are especially thankful for the elaborat comments by Reviewer #2, who encouraged us to be precise in presenting our efforts to validate the QADS questionnaire. Two completely new confirmatory and an exploratory factor analyses were carried out. We are aware that there is still a great deal of evidence needed, such as studies about criterion validity and convergent validity.

We apologize that the title “…psychometric properties…” somehow caused confusion, since this generalization may have aroused false expectations about the study’s purpose (elaborated validation). Therefore, we propose changing the title into “Factor structure, discriminant validity and standard percentages of the Questionnaire for the Assessment of Disgust Sensitivity (QADS) in a representative German survey”.

We debated extensively whether or not a main score should be discussed and its percentages presented. Although we finally decided to leave this point out, we would be pleased to reconsider the main score, if you think it is worth considering.

We look forward to hearing from you.

Sincerely yours,

Katja Petrowski, PhD
Comments of Professor Hannah Clark:

Please also address the following points:

- Further consideration of your manuscript is conditional on improvement of the English used [...] Please ensure particular attention is paid to the abstract. You should have a native English speaking colleague help you with this, if possible, or use a commercial copyediting service. [...] Thank you very much for this feedback. A native speaker reviewed the revised manuscript for grammatical structure and punctuation. We now hope that the manuscript shows a better language quality.

- Ethics - Experimental research that is reported in the manuscript must have been performed with the approval of an appropriate ethics committee. Research carried out on humans must be in compliance with the Helsinki Declaration (http://www.wma.net/e/policy/b3.htm), and any experimental research on animals must follow internationally recognized guidelines. A statement to this effect must appear in the Methods section of the manuscript, including the name of the body which gave approval, with a reference number where appropriate.

The statement referring to the Helsinki Declaration is included on p.7, second paragraph. All individuals participated voluntarily and received a data protection declaration, in agreement with the Declaration of Helsinki. The study followed the ethical guidelines of the “German professional institutions of social researcher” [Arbeitskreis Deutscher Markt- und Sozialforschungsinstitute e.V. (ADM), Arbeitgemeinschaft Sozialwissenschaftlicher Institute e.V. (ASI), Berufsverband Deutscher Markt- und Sozialforscher e.V. (BVM)] which were implemented to improve the German federal law to protect the ethical rights of individuals. These guidelines specify amongst others how the person have to be approached and treated in the interview, how the personal as well as the collected data has to be handled and stored. The application of the guidelines secures that the studies are implemented according to the valid ethical. The study presented here was approved according to the guidelines of the German professional institution of social researcher.

- Please also include a copy of the questionnaire with the revised manuscript.

Of course! A copy of the original German questionnaire will be attached. In addition, two tables with an English version of the 30 and 37 items, respectively, is included as well (tables 2 and 3).

- Please also ensure that your revised manuscript conforms to the journal style (http://www.biomedcentral.com/info/ifora/medicine_journals ). It is important that your files are correctly formatted.

We checked and corrected the style of the manuscript. If there is still anything wrong, please let us know!

Reviewer #1:

Several limitations of the current manuscript seriously weaken its suitability for publication in BMC.

1) Major Compulsory Revisions

a) The manuscript in its current form will require a significant revision in grammatical structure, and to a lesser extent, punctuation. A consistent flow in reading the article was hard to establish due to difficulties with ambiguous language (e.g., p3: "...disgust sensitivity is associated to individual proneness or a clinical diagnosis report."; p. 9: "... area-unspecific self-report instrument of the individual disposition disgust sensitivity," ) and periodic sentence fragments (e.g., p.8: "The level of education was dichotomized as."). It was difficult to follow coherently how the existing study represents a unique extension to the current literature on the assessment of disgust sensitivity given the ambiguous language used to summarize the empirical and theoretical work in this area. I understand that English
may be a second language for the primary authors, but a significant re-write will be needed if this work is to be published in an English-speaking journal outlet.

We thank the reviewer for the remarks. We hopefully eliminated the ambiguous language and sentence fragments. A native speaker revised the manuscript, the grammatical structure and the punctuation errors. We hopefully made major improvements to the English written.

b) I would suggest that the analyses be reviewed by an expert in the statistical analyses used in the present study. In the methods section, it would have been helpful to include a sub-section describing the purpose and rationale for their chosen statistical analyses.

This comment clearly stated the need for a detailed explanation of the statistical analyses. Therefore, we included a detailed description of what we did with which method upfront of every statistical procedure in the result section.

2) Minor Essential Revisions

a) If the FEE is to be considered along side existing measures of disgust sensitivity, then it would be helpful for the authors to specifically name the alternate disgust scales in the Introduction. Only two of the four referenced scales were actually named in the text. The authors should also consider the Disgust Emotions Scale (Kleinknechet et al., 1997) which is also a commonly used measure of disgust in the literature.

We are thankful for the remark on the DES and included that instrument in the introduction (p.5). We also included the names of the instruments. However, naming all instruments was difficult: Wronska neither reported a name nor psychometric properties in their respective chapter. We hope to meet the requirements of the reviewer with the chosen solution.

The discussion could use additional work and refinement in comparing and contrasting the findings of the FEE relative to the other existing disgust scales. This will certainly help the argument as to why the FEE represents an improvement beyond existing measures of disgust. I would suggest that the authors review the following articles: A taxometric study of the latent structure of disgust sensitivity: converging evidence for dimensionality (Olatunji & Broman-Fulks, 2007); and, The Disgust Scale: item analysis, factor structure, and suggestions for refinement (Olatunji et al., 2007). Both of these articles would provide a good summary of existing disgust scales which may help the authors in further articulating the value of the FEE relative to these other scales.

Thank you very much for the helpful comment and the reference to very essential and interesting literature. We indeed want to compare the German version to the Disgust Scale and other measurements. Therefore, we re-wrote the discussion. Both mentioned articles are considered in the discussion (Olatunji et al.; Olatunji & Broman-Fulks, on p. 14, first and second paragraph, p.15, third paragraph, p.16, first paragraph). We hope that the comparison is now clearer.

b) There were several places in the manuscript in which incorrect terms were used (e.g., p.11: "special phobias" = specific phobias), and certain interpretations seemed overstated and not particularly relevant (e.g., p.10: "...implies on the one hand, that most participants did not suffer from either body image disturbances as anorectic patients or problems...". Please correct.

Thank you for the helpful remark. We corrected the errors and re-wrote those interpretations as well as eliminated these indeed critical interpretations (p.3).

c) The discussion also seemed to get off topic with including content explaining sex and educational differences in disgust sensitivity. I believe that these comments take away from the primary scope of this paper as a psychometric/methods report. I would suggest eliminating these comments.

Thank you very much for the comment. The reviewer gave us a good solution to stream line the discussion. Therefore, we rewrote the discussion section (p.14, last paragraph). However, we still discussed bravely gender issues based on the clear effect in the literature and based on
the comment of Reviewer #2. Gender differences led to different standard percentages (Table 6).

Reviewer #2:

The study could be improved in a number of ways to make a stronger contribution to the literature. My concerns are listed below, with those that I consider to be major compulsory revisions marked with asterisks (*)

Introduction:

1. * There are problems with the writing mechanics. To illustrate, I will focus on the second paragraph of the introduction – this paragraph may have more problems than most, but it illustrates the types of writing concerns I have with the entire manuscript. In the second paragraph of the introduction, in the third sentence, the word “evolve” is used in a way that is not related to evolutionary theory. This seems a poor word choice given the prominence of evolutionary theory in the field of disgust research.

Thank you very much for the very helpful comment. We eliminated the word as well as rewrote the passage on page 4.

The next sentence begins with “Regarding the general concept of emotional reactivity . . .”, but nothing in the remainder of the sentence appears to be related to the general concept of emotional reactivity.

Thank you, the reviewer is definitely right. We eliminated the reference to the emotional reactivity and rewrote the paragraph.

The remainder of that sentence strikes me as a tautology; disgust sensitivity predicts disgust reactions, but isn’t disgust sensitivity defined in this way?

Thank you very much for the very helpful comment. We rewrote the passage on page 3 which was really not logic.

The fifth sentence of the second paragraph of the introduction states that “. . . disgust sensitivity is a vulnerability factor . . .”. In my view, the evidence does not pass the threshold for such a claim; I know of no studies in which disgust sensitivity has been experimental manipulated to affect these outcomes, nor even any naturalistic prospective studies in which disgust sensitivity predicts the onset or course of these outcomes.

This is an excellent comment. The claim is indeed overstated. Therefore, we eliminated the passage and rewrote the statement because there are indeed no precise evidences available.

The last sentence seems to jumble two issues: (1) whether disgust sensitivity is multifaceted versus unitary, and (2) whether disgust sensitivity (or some facet of disgust sensitivity) has specificity to certain domain. Overall, the lack of clarity in the writing is substantial and needs to be addressed.

We are very thankful for this comment. We rewrote this passage and asked a colleague as well as a native speaker to review our flow as well as the English phrasing. We hope that this passage shows extensive improvements.

2. * Greater care should be taken when discussing the definition of disgust sensitivity. In its present form, the authors’ provide no citation for their definition of disgust sensitivity, and describe disgust sensitivity as “an individual’s time-invariant predisposition to reacting to disgusting materials.”

Thank you very much we included the precise reference out of which we got the definition (page 4). This definition overlaps with that of Olatunji & Broman-Fulks (2007; A taxometric study of the latent structure of disgust sensitivity: converging evidence for dimensionality. Psychological
Assessment, 4 437-448; “a predisposition toward experiencing disgust (de Jong & Merckelbach, 1998; Olatunji & Sawchuk, 2005), has been described as a genetically based personality trait that operates as a specific vulnerability factor for certain disorders (Muris, 2006).

Recently [see van Overveld, M., de Jong, P. J., Peters, M. L., Cavanagh, K., & Davey, G. C. L. (2006). Disgust propensity and disgust sensitivity: Separate constructs that are differentially related to specific fears. Personality and Individual Differences, 41, 1241-1252], it has been proposed that disgust reactions can be characterized as consisting of both disgust propensity and disgust sensitivity, with disgust propensity referring to how easily one is disgusted and disgust sensitivity referring to how bothered one is by their disgust. Based upon van Overveld et al., disgust propensity is actually what these researchers are referring to when they refer to disgust sensitivity. The authors’ should be mindful of this distinction and should, at the very least, provide a citation as to where their definition of disgust sensitivity comes from.

We are very grateful for this important comment. Based on the literature on the QADS we used the term disgust sensitivity. Since in the literature there seemed to be no consensus on the definition and use of the terms, we included in the discussion a paragraph (p. 16, second paragraph) in which we addressed this issue.

3. * The description of Schienle et al. is unclear:
   a. What are “sensitivity items” (second to last line on p. 4)? What pool of items were they selected from? Where these from Haidt et al.’s original item pool set, or generated by Schienle et al.?

We are very grateful for this feedback. Therefore, we rewrote this paragraph (on page 5, second paragraph and on page 6, first paragraph) in which we precisely explained the changes and the development of the instrument.

b. I don’t understand the second full sentence at the top of p. 5 (i.e., “The original factor structure could not be replicated since theoretical aspects guided the decision for the factor-structure by Haidt et al.”).

This obviously misleading sentence was erased.

c. The last sentence of that paragraph (i.e., “... instrument stated good reliabilities. ...”) is awkward.

This obviously illogical sentence was erased.

d. What sort of factor analysis was chosen by these researchers?

We are very grateful for this feedback. The methodological approach and kind of factor analysis is the key to understand these different results. Therefore, we rewrote this paragraph on page 6, first paragraph in which we precisely explained the statistical approach to develop the instrument.

e. Further, these researchers state “the final items were selected by the eight highest factor loadings.” How many items did the total scale consist of, the component scales consist of, and so on? This phrasing is especially confusing given that earlier on this page (page 5) these researchers note that “an additional domain deformation was included with four items.” This domain does not appear under the subsequent list of factors found via factor analysis and contains only four items. What happened to this domain?

Thank you very much, the description was definitely illogic and incomplete. Item numbers and scale construction methods are now precisely referred to on page 5 last paragraph.

4. It is confusing how the Disgust Scale becomes written as the Questionnaire for the Assessment of Disgust Sensitivity (FEE) in the remainder of the paper. Some clarification would be helpful.
We are sorry for the confusion. We included a sentence on page 6, first paragraph in which we clearly state the name and the abbreviation.

5. These researchers state that the present study seeks to “replicate the psychometric properties of this new questionnaire as well as their factor structure.” In its present form, it seems as though Schienle et al. only examined the factor structure of the FEE and did not examine any of its other psychometric properties (e.g., convergent, discriminant validity). It thus seems that these researchers are only interested in examining the factor structure of the FEE in the present study?

Thank you very much for the comment. We realized through it, that “psychometric properties” may mislead expectations towards our study. The two main foci of the present study: 1) factor structure, 2) discriminant validation to physical well-being (correlations between QADS scales and FEW-16 scales), 3) scale-wise standard percentages. In order to avoid this confusion we rephrased our sentences in this respect and included in the discussion a paragraph on the question which additional properties have to be examined in which design.

Method

1. What is “consumption rate” (last line of page 5)?

Thank you, the reviewer is correct – we have changed the wording accordingly. Coverage rate is meant as the percentage of persons asked who eventually participated.

2. The first sentence of the Method section states that the data were obtained “by order of the University of Leipzig.” Do you mean the data collection was conducted by order of the University? Although the next paragraph gives assurances about the ethical conduct of the research, this first sentence seems likely to lead to unnecessary concerns.

The reviewer is correct and we changed the phrasing.

3. The claim of representative sample requires is justified by the sampling method. Because this claim appears in the title, abstract, intro, etc., some mention of the sampling method should be given in the abstract. It is only here, in the method section, that the justification for the claim is even mentioned.

This is a very good idea. We consequently named the sampling method in the abstract and hope to satisfy the reviewer’s good point.

4. * It is unclear why the FEW-16 was included? These researchers state “physical well-being, as measured by the FEW-16, was related to the FEE scales in order to control whether the individual’s disgust perception was impaired by feeling physically unwell.” This phrasing is confusing. Further, there appear to be no analyses in this paper that describe the FEW-16 as a covariate. Was this measure used for discriminant validity? If so, why was there no measure of convergent validity? Or was this measure used for criterion validity, in which case the evidence for the FEE is lacking.

We are thankful for that remark and included a passage on the definition physical well-being and the purpose of the FEW-16 used (see last paragraph of the introduction). The stated sentence is now part of this new paragraph. We included the questionnaire with two aims: analyzing discriminant validity and control the sample for possible long-term effects of illnesses. The analyses using the FEW are included in table 5 and on page 7, first paragraph.

Results

1. * How does the FEE contain 37 items? The original DS consists of 32 items and then Schienle et al. added 28 items (indicating 60 items total). The researchers then state on page 5 that the 8 highest loading items on the five factors were retained (indicating the potential for only 40 items total).
Thank you very much for this comment – the paragraph concerning the development of the QADS (p.5-6) was expanded, and with more precision rewritten.

2. *Why an exploratory approach was undertaken. In the introduction, these researchers make it clear that they are interested in replicating the Schienle et al. study. This language leads me to believe that a confirmatory approach should be applied. If a confirmatory approach fails, the researchers might: (1) randomly split their large sample in half; (2) first run an exploratory factor analysis on one half of the sample and obtain a factor structure; (3) then run a confirmatory factor analysis on the second half of the sample to confirm the factor structure obtained in step 2. This procedure would move the field closest to having a replicable factor structure of the FEE and also test the adequacy of the factor structure found by these researchers.*

Thank you very much for the helpful guide on the statistical approach. We got inspired by the reviewer and followed his suggestion all the way through. In the result section we describe in detail the flow of the statistical analyses and the precise method chosen.

3. *Some researchers may be concerned that the Kaiser-Guttman criterion can lead to a larger number of factors than is warranted, especially when conducting factor analyses of items [Zwick, W. R., & Velicer, W. F. (1986). Comparisons of five rules for determining the number of components to retain. Psychological Bulletin, 99, 432-442.]. This seems of particular concern because the fifth eigenvalue is barely greater than 1.0. Another method for determining the number of factors to extract is to use parallel analysis [Horn, J. L. (1965). A rationale and test for the number of factors in factor analysis. Psychometrika, 30, 179-185. And Longman, R. S., Cota, A. A., Holden, R. R., & Fekken, G. C. (1989). A regression equation for the parallel analysis criterion in principal components analysis: mean and 95th percentile eigenvalues. Multivariate Behavior Research, 24, 59-69.]. Given the large sample size, I guess that parallel analysis would likely suggest that four or perhaps five factors be extracted.*

This point was well taken by the reviewer. We are thankful for his expertise and followed his guidance by implementing a parallel analysis.

4. *The authors might also want to consider factor interpretability, hyperplane items, and other criteria for determining the optimal solution [Gorsuch, R. L. (1983). Factor analysis (2nd ed.). Hillsdale, NJ: Erlbaum]. The very low number of indicators (and the correspondingly low alphas) limit the usefulness of some scales.*

We are thankful for this remark. Factor interpretability will be addressed in the new result-section in which we precisely explained the statistical analyses. Including the reviewers ideas, the hyperplane-item were precisely stated on page 6, first paragraph; pare 10, third paragraph. Both number of items per scale (9-11) and the range of item-scale-correlations (.50-.68) are comparable to the findings of Olatunji et al. (2007), who worked out the DS-R with 5-12 items per scale and a range of .41 to .83 (cf. figure 1 on p.289 in the original work). While Olatunji and colleagues as well as Schinele and colleagues found less-than-stellar internal consistency in one of their three scales, the scales we found, inherit good internal consistencies > .87 (cf. table 4). Those points were also addressed on p. 11-12.

5. *Why use an orthogonal rotation? I would have used some type of oblique rotation. Can you provide a rationale?*

This is a good idea and we implemented the advice and addressed it in the new result-subsection on page 15, last paragraph.

6. *The comparability of factor structures across studies can be quantified using coefficients of convergence [see Gorsuch, 1983].*

Thank you very much, this is an excellent idea. Due to the limited time we got for resubmission we were not able to run through these additional analyses. However, we included this point in our discussion since it is an important point for the future use of the different instruments and the comparability.

7. *I did not think that the analyses of the part-samples were necessary.*
8. There are many tables. As mentioned above, I did not think Table 5 was necessary. Please see reply on Reviewer #1. We excluded as recommended some of the analyses on the subsamples.

9. Corrected item-total correlations are preferable, as the item-total correlations will generally inflate estimates.

This is absolutely right. We reported the appropriate statistics in Table 2.

10. *Clearer language is needed when explaining the relation between the FEE and the FEW-16. Currently, it reads “no single significant interrelation was found.” However, in Table 6, it reads, “p < .001 for all correlations.” If all are nonsignificant, Table 6 could be dropped, perhaps reporting the range of nonsignificant correlations in the text. If significant, Table 6 can be combined with Table 4.

We are thankful for this comment. We reported correlations correctly for the three new scales.

Discussion:

1. *On page 9, the authors’ state, “all items were generally seen as disgusting with mostly above-average disgust ratings. Therefore, the items indicated usefulness to initiate disgust-relevant responses, objectively of the instrument and at least satisfying validity.” The claim that the present study satisfies validity seems misguided. Although females endorsed having disgust, as measured by the FEE, at higher percentages, which would be predicted based upon prior studies, can we really be sure that the FEE is measuring disgust in a German population from these results? It seems to me that this claim would be best served for data that suggest (a) the FEE has moderate to high correlations with other measures of disgust or disgust sensitivity; (b) the FEE has lower correlations with related constructs (e.g., anxiety sensitivity); (c) the FEE predicts criteria of interest.

Thank you very much for this comment. The reviewer stated correctly that validity is still not quantified. We assumed based on the DS-research and the satisfying translation that we can transfer these results on to our version. In order to be more precise we rephrased this passage on page 15.

2. *On page 9, the authors’ state, “Item re-construction or factor re-composition is indicated.” This phrasing also seems misguided. Although readers may be more confident that the present factor structure is preferable to the study by Schienle et al., due to the higher sample size and sample characteristics, it is still only based on an exploratory approach.

Thank you very much for the comment. We followed the statistical approach as proposed by the Reviewer #2 and rewrote the section accordingly based on the additional results.